CHANCE, INDETERMINACY, AND EXPLANATION

by

Nina R Emery

B.S. Physics, Philosophy
Cornell University, 2005

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Signature of the Author: ______________________________
Department of Linguistics and Philosophy
May 15, 2012

Certified By: ______________________________
Brad Skow
Associate Professor of Philosophy
Thesis Supervisor

Accepted By: ______________________________
Roger White
Professor of Philosophy
Chair of the Committee on Graduate Students

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ABSTRACT

This thesis is about the philosophical and scientific significance of chance. Specifically, I ask whether there is a single notion of chance that both plays a well-defined scientific role and proves useful for various philosophical projects. I argue that there is, but that this notion of chance is importantly different from the one that we usually come across in the philosophical literature.

In the first chapter, "Chance, Indeterminacy, and Explanation", I argue against the common and influential view that chances are those probabilities that arise when the fundamental laws are indeterministic. The problem with this view, I claim, is not that it conflicts with some antecedently plausible metaphysics of chance, but rather that it renders the distinction between chance and other sorts of probability incapable of playing any scientifically significant role. I suggest an alternative view, according to which chances are the probabilities that play a certain explanatory role—they are probabilities that explain associated frequencies.

In the second chapter, "Chance, Explanation, and Measure", I build on the view that chances are the probabilities that play a certain explanatory role by developing an account of non-fundamental chances—chances that arise when the fundamental laws are deterministic. On this account, non-fundamental chances are objective measures over relevant classes of alternative possibilities.

In the third chapter, "Chance and Counterfactuals", I show how the sort of chances I have argued for can play an important role in a very different sort of philosophical project. According to a number of recent arguments, one consequence of our current scientific theories is that most ordinary counterfactuals are not true. I argue that the best response to these arguments makes use of the non-fundamental chances that I have argued for in the first two chapters of the dissertation.

Thesis Supervisor: Brad Skow
Title: Associate Professor

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Introduction

My dissertation is about the philosophical and scientific significance of chance. Specifically, I ask whether there is a single notion of chance that both plays a well-defined scientific role and proves useful for various philosophical projects. I argue that there is, but that this notion of chance is importantly different from the one that we usually come across in the philosophical literature.

In the first chapter, "Chance, Indeterminacy, and Explanation", I argue against the common and influential view that chances are those probabilities that arise when the fundamental laws are indeterministic. The problem with this view, I claim, is not that it conflicts with some antecedently plausible metaphysics of chance, but rather that it renders the distinction between chances and other sorts of probabilities incapable of playing any scientifically significant role. I suggest an alternative view, according to which chances are the probabilities that play a certain explanatory role—they are probabilities that explain associated frequencies.

In the second chapter, "Chance, Explanation, and Measure", I build on the view that chances are the probabilities that play a certain explanatory role by developing an account of non-fundamental chances—chances that arise when the fundamental laws are deterministic. On this account, non-fundamental chances are objective measures over relevant classes of alternative possibilities. I identify the conditions under which we have evidence for such objective measures and show how such measures can prove explanatory by working through a particular case—the probabilities that arise in statistical mechanics. At the end of the chapter, I show how the view can be broadened to include probabilities that arise in other special sciences, and in some instances of non-scientific reasoning, for instance reasoning about games of chance.

In the third and final chapter, "Chance and Counterfactuals", I show how the sort of chances I have argued for can play an important role in a very different sort of philosophical project. According to a number of recent arguments, one consequence of our current scientific theories is that most ordinary counterfactuals are not true. I argue that the best response to these arguments makes use of the non-fundamental chances that I have argued for in the first two chapters of the dissertation.

That our best scientific theories ought to inform our best metaphysics is something of a platitude. Insofar as both tell stories about what there is, those stories can either coincide, or they can compete. But a scientifically informed metaphysics is not just a metaphysics that yields results compatible with our best science; it also takes our best science as a source of input. As these three chapters taken together show, careful attention to the scientific role that various entities are supposed to play can help shape our metaphysical investigation of those entities in a way that proves both illuminating and productive across a wide range of philosophical projects.

1

Chance, Indeterminacy, and Explanation

It is natural to think that there is some sort of important connection between *chance* and *indeterminacy*, along the following lines: if there is some chance that Sally will attend the party (and some chance that she won't), then it is not determined whether Sally is going to attend. Any theory of chance has at least a *prima facie* obligation to either capture this apparent connection between chance and indeterminacy or to explain it away.

Some philosophers have tried to spell out the connection between chance and indeterminacy in terms of a connection between chance and *indeterministic laws.*[1] These philosophers claim that a necessary condition for there to be genuine chances in a world is that the fundamental laws in that world be indeterministic. If there is some chance that Sally will attend the party, then the complete physical state of the world right now plus the laws of nature do not entail that Sally will attend (or that she will not attend).

In this paper I will argue that this is a mistake. No scientifically informed metaphysics should include the claim that chances only arise in worlds where the fundamental laws are indeterministic. Moreover, I will argue, we can arrive at this conclusion without making any controversial metaphysical assumptions about what chances are.[2]

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[1] The classic example of this position is Lewis 1980. A recent example is Schaffer 2007.

[2] In this way, my argument differs significantly from other recent arguments for the compatibility of chance and determinism found in Loewer 2001, Hoefer 2007, and Glynn 2010. These arguments begin by making substantive assumptions about what the correct theory of chance is. And because the question of what chances are is such a vexed one, these assumptions are inevitably controversial.

My argument begins from a single assumption: that whatever chances are, they play some role in our best scientific theories. This assumption is both minimal and significant. It is minimal in the sense that it is as uncontroversial a starting point as we are likely to find. To the extent that we are interested in chances at all it is because they show up in our best theorizing about what the world is, or might be, like. It is significant in the sense that it allows us to draw important metaphysical conclusions. Not only does it undermine the claim that genuine chances are incompatible with deterministic laws, it also suggests a new approach to the question of what chances are. On this new approach chances are not those probabilities that arise when there is some genuine indeterminacy in the world. Instead chances are those probabilities that play a certain explanatory role.

Here is a plan for what follows. In Section 1, I describe two paradigm cases—cases that are supposed to capture the distinction between chances and other types of probability. In Sections 2 and 3, I set out two accounts of what makes for the difference between the two paradigm cases. According to one of these accounts chances are those probabilities that arise when the fundamental laws are indeterministic. According to the other account, chances are those probabilities that play a certain explanatory role.

In Section 4, I show that these two accounts are competing accounts. For an important class of probabilities, the two accounts disagree. In light of this fact philosophers have tended to take one of two routes. Either they argue that there is not one but two notions of chance, each of which arises from one of the competing accounts. Or they argue that we ought to give up on the account of chance according to which chances are those probabilities that play a certain explanatory role.

In Section 5, I argue for a different tactic. Closer attention to the claim that chances are those probabilities that arise when the fundamental laws are indeterministic shows it to be unstable. Any reason we have for endorsing that claim is also a reason for adopting an even stronger claim about chance. And it follows from this stronger claim that there are no such things as chances, according to our best scientific theories.

In Section 6, I take stock of where this argument leaves us with respect to the project of developing a more detailed metaphysical account of chance.

1 Paradigm Cases

Suppose that every day, on Sally's walk to work, she passes a house where a certain cat lives. Recently, Sally has noticed that the cat's behavior seems to be following a regular pattern: most of the time, if it is sunny, the cat is out in the garden. And most of the time, if it is raining, the cat is hiding under the porch. Based on these observations, Sally has come up with the following theory about the cat's behavior:

(1) Given that it is raining, it is very likely that the cat is underneath the porch.

Now suppose that, in addition, Sally is a physicist, and she is in the process of running a certain experiment that illustrates some of the basic principles of quantum mechanics. The experiment involves sending a steady stream of silver atoms, which have all been prepared in a certain way, through a special set of magnets that will deflect each atom either up or down.[3] Sally has run many, many experiments of this sort and she knows the following two facts about experimental set-ups like this one. First, the fundamental laws governing the behavior of the silver atoms are indeterministic: there is no feature F such that all and only the silver atoms that have F before going through the experimental set-up will be deflected up.[4] Second, the results in similar set-ups exhibit a pattern: most, but not all, of the silver atoms are deflected up. Based on these observations Sally has come up with the following theory about the behavior of the silver atoms.

(2) Given that it has been prepared a certain way, it is very likely that the next silver atom to go through the magnets will be deflected up.

These two examples are supposed to illustrate two paradigmatic ways that probabilities show up in our theorizing about what the world is like. That there are differences between the two cases should be uncontroversial. Less obvious is whether or not we can make use of on one or more of these differences in order to identify a rigorous metaphysical distinction between chance on the one hand, and other types of probability on the other. This is the project that I will take up in the next two sections.

Before we begin, a couple of points worth emphasizing. First, the project is only going to be interesting and productive if it is suitably constrained. We are not interested in coming up with just any distinction between chances and other types of probabilities. Instead we are specifi-

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[3] The details do not matter for purposes of this paper, but the sort of case I have in mind is one where (i) the silver atoms were prepared by being sent through an initial set of Stern-Gerlach magnets, after which (ii) the atoms that were deflected down were discarded, and the experiment consists of (iii) sending the remaining atoms through a second set of Stern-Gerlach magnets, which is rotated, but only slightly, with respect to the initial set.

[4] Whether or not this is true of the actual world is still up for debate. According to the GRW interpretation of quantum mechanical phenomena it is; according to Bohmian mechanics (a competing interpretation) it is not.

cally interested in identifying a distinction that plays a scientific, as well as a philosophical, role. I take it as a precondition of embarking on a fruitful metaphysics of chance that we make sure that the distinction between chances and other sorts of probabilities is drawn in such a way that chances have some hope of playing this sort of role.

Second, the distinction we are aiming for, the one that captures the difference between (1) and (2), is not going to be the relatively familiar distinction between chance and *credence*. In order to forestall any confusion on this point, it is worth taking a moment here to spell out why.

The distinction between chance and credence is usually made with respect to particular claims about whether or not something is likely. Sometimes when we make a claim about what is likely we are merely reporting our *degree of belief*, or credence, that a certain proposition is true. But sometimes when we use probabilistic language we are doing more than merely reporting our credences. We are, at least on the face of things, making a claim about what the world is like that is supposed to be independent of anyone's mental state.

Here is a way of bringing out the difference between chance and credence. Suppose Sally, currently at home, says, "It's very likely that the next silver atom will be deflected up." Unbeknownst to Sally, someone has broken into her lab and tampered with her experimental set-up. If we interpret Sally's claim as a claim about the *chance* of the silver atom going up then, depending on what exactly the intruder did, her claim may turn out to be false. It might be the case that as a result of the intruder's actions, the chance that the next silver atom being deflected up is close to 0. But if we interpret Sally's claim as a report of her *credence*, then it will still be true despite the break-in. Since Sally doesn't know that the break-in has occurred she will have the same degree of belief in the proposition that the next silver atom will go up, regardless of the extent to which the experimental set-up has been altered.

Whatever the distinction is supposed to be between (1) and (2), it is not the simple distinction between chance and credence. (1) and (2) are parts of theories that Sally has come up with about the world around her. Neither is a mere report of her credence at a particular time. Instead both (1) and (2) constrain credences—they tell Sally what her credences *should* be, whenever certain conditions are met.

For this reason, in what follows, I will steer clear of the terminology of 'subjective probability', which is usually read as synonymous with 'credence'. Instead, I will characterize the distinction I am aiming for, the one that is supposed to be captured by the paradigm cases above, as the distinction between chance—objective probability—and mere *epistemic probability*. The hope is that understanding this distinction will give us some insight into what chance is. As to what epistemic probability is, careful attention to (2) tells us at least this much: they may include, but are not obviously limited to, credences.

2 Indeterminacy and Indeterministic Laws

According to one influential line of thinking, a crucial difference between (1) and (2) is that (1) assigns a probability to something that has already happened, while (2) assigns a probability to something that has yet to happen. Sally uses (1) to determine her credence in something that is already fixed—the cat has already gone under the porch, or he hasn't—but she uses (2) to fix her credence in something that is still open—the next silver atom has yet to go through the set-up.

In spelling this out it will help to contrast (2) with (3), where (3) is, presumably, also part of Sally's theory about the behavior of the silver atoms.

(2) Given that it has been prepared in a certain way, it is very likely that the next silver atom to go through the magnets will be deflected up.

(3) Given that it was prepared in a certain way, it is very likely that the last silver atom to go through the magnets was deflected up.

Like (1), (3) is a claim about the probability of something that has already happened, whereas (2), is a claim about the probability of something that has yet to happen.[5] In *A Subjectivist's Guide to Objective Chance*, David Lewis famously claimed that this difference was of central importance in drawing a distinction between chance and mere epistemic probability. He said, "The past, unlike the future, has no chance of being any other way than the way that it actually is. This temporal assymmetry falls into place as part of our conception of the past as "fixed" and the future as "open"—whatever that may mean."[6]

According to Lewis the chance of some proposition being true depends on the time at which that chance is evaluated. The way in which chances are indexed to times is analogous to the way in which chances are indexed to possible worlds. Compare the world in which Sally's experimental set-up is as originally described (call it 'ω'), to a possible world (call it 'ω_1') in which someone has broken into Sally's lab and tampered with the set-up. The chance that the next sil-

[5] More carefully, claims about chance can be time-indexed in two ways. First, each claim is indexed to a particular time of evaluation (when it is not made explicit, this is generally the time of assertion). Call the time of evaluation t_e. Second, each claim about chance may be *about* a particular time, call it t_a. For (1) and (3), t_a is before t_e. That is, at the time of evaluation, we would say that the proposition to be evaluated is about something that has already happened. For (2), t_a is after t_e. That is, at the time of evalaution, we would say that the proposition to be evaluated is about something that has yet to happen.

[6] Lewis 1980, p. 93.

ver atom will go up, given that it is prepared in a certain way, may well be very different in ω than it is in ω_1. That is, the chance of the very same proposition being true may well be different depending on the world at which it is evaluated.

Similarly, according to Lewis, the chance of the very same proposition being true will depend on the time at which we evaluate the proposition. Call the next silver atom to go through Sally's experiment 'S'. Before S goes through the experiment, according to Lewis, the chance that S will be deflected up may well be very close to, but not quite, 1. But after S goes through the experiment the chance of S being deflected up is either 0 or 1. It is only because Sally is ignorant of whether or not S was in fact deflected up that she sets her credences according to (3).

On this way of thinking about the difference between (2) and (3), there are two different sorts of propositions—propositions that are wholly about facts which are already fixed, like propositions about the past, and propositions that are about facts that are still open, like propositions about the future. If there is a non-trivial chance that some proposition is true, then that proposition cannot be wholly about facts that are already fixed. As a result (2), but not (3), may be an objective probability.

Now consider a world that is just like the world Sally lives in, except that the fundamental laws governing the behavior of silver atoms are deterministic. There is some feature F such that each silver atom either has or does not have F, and it follows from the laws that all and only silver atoms that have F are deflected up. However, the nature of F makes it impossible for Sally's counterpart to determine F for any particular silver atom.[7] As a result, even in the deterministic world, Sally's theory about the silver atoms includes claims (2) and (3).

[7] I would like to leave open here the exact sense in which it is impossible for Sally to determine F for any particular silver atom. This might be a result of practical limitations on her part, or it might be built into the laws that any attempt to determine F inevitably changes F. (The latter case is similar to what Bohmian Mechanics says about the initial positions of particles.)

Someone who agrees with Lewis's reasoning as to why (3) is not a claim about chance will almost certainly think that in the deterministic world, (2) also cannot be a claim about chance. Remember that Lewis's reasoning relied on the following necessary condition: if there is a non-trivial chance that some proposition is true, then that the proposition cannot be wholly about facts that are already fixed. But in a deterministic world facts about the future *are* already fixed—they are fixed by the facts about the past plus the laws of nature. So in a deterministic world, there will be no objective probabilities.

In the recent literature on chance, these considerations have been formalized in following criterion for distinguishing between epistemic and objective probabilities.

> *The Indeterminacy Criterion*
>
> If the chance, at world ω, at time t, of proposition p is greater than 0, then there exists a world ω' such that (i) ω' matches ω in laws, (ii) ω' and ω have the same microphysical history up until time t, and (iii) p is true at ω'.[8]

Consider again the paradigm cases that we started with. According to the Indeterminacy Criterion, regardless of whether or not it is raining, the chance that the cat is under the porch right now is either 0 or 1. This is because the microphysical history of the world up until now includes some fact about the cat's location. If the cat is not currently under the porch, then there is no possible world that matches the history of the actual world up until now in which the cat is under the porch, so the chance that the cat is under the porch must be 0. If the cat is in fact under the

[8] This principle is called the Realization Principle in Schaffer 2007 and Glynn 2010. It's a stronger version of the Basic Chance Principle found in Bigelow 1993. For a discussion of why anyone who accepts the Basic Chance Principle should also accept the Realization Principle see Schaffer 2007, p. 124.

porch, then there is no possible world that matches the history of the actual world up until now in which the cat is not under the porch. Therefore the chance that the cat is not under the porch is 0, and the chance that the cat is under the porch is 1. So the chance that the cat is under the porch is either 0 or 1. And since (1) says that the probability that the cat is under the porch is neither 0 nor 1, (1) cannot be a claim about chance.

In the original silver atom case, by contrast, all the facts about the history of the actual world up to and including the present moment, combined with the laws of nature, do not determine whether or not the silver atom will go up. There are possible worlds compatible with the entire history of the actual world up until the present moment in which the silver atom is deflected up, and possible worlds compatible with the entire history of the actual world up until the present moment in which the silver atom is deflected down. As a result the probability in (2) may be an objective probability.[9]

3 Explanation

In this section I will propose an alternative account of what makes for an important difference between the probabilities found in (1) and (2). On this account the important difference between those cases is a not difference in which facts are already fixed, but rather a difference in explanatory power.

[9] I say that, based on the argument here, the probability in (2) *may be* a genuine chance, because the Indeterminacy Criterion is only a necessary condition, not a sufficient condition—you can have genuine indeterminacy without any genuine chances if there aren't the right sorts of patterns in the events. Of course the paradigm cases were set up to exhibit precisely the right sort of pattern.

In both of the paradigm cases the evidence that leads Sally to make a claim about probabilities was evidence about frequencies: on the basis of the fact that most (but not all) of the time, when it is raining, the cat is under the porch, Sally theorizes that, given that it is raining, it is very likely that the cat is under the porch. And on the basis of the fact that most (but not all) of the time, when the silver atoms have been prepared in a certain way, they are deflected up, Sally concludes that, given that the next silver atom has been prepared in a certain way, it is very likely that it will be deflected up.

But in addition to frequencies providing evidence for probabilities, probabilities can also explain frequencies. In particular, the fact that silver atoms that are prepared a certain way *are very likely to be deflected up* is generally taken to be an explanation of the fact that most of the silver atoms that are prepared in that way *are deflected up*. Not so for the cat case. The fact that it is very likely that the cat is under the porch when it is raining does not itself seem to be any explanation of the cat's behavior. The fact that the cat is usually under the porch when it is raining is presumably explained by other facts, like that the cat does not like to get wet, and that there tends to be nothing very exciting going on in the garden when it is raining. None of these facts are in turn explained by (1), so (1) cannot explain why the cat is usually under the porch when it rains.

On this account, then, we should adopt something like the following criterion for distinguishing between chance and other types of probabilities:

The Explanatory Criterion

Suppose that events of type E are events that are independent of anyone's belief state. And consider the following two facts.

(I) The probability of an event of type E is very high.

(II) The relative frequency of events of type E is very high.

If (I) explains (II) then the probability in (I) is an objective probability.[10]

In order to fully understand what the Explanatory Criterion means and how it might constrain our metaphysics of chance, we would need to spell out in more detail what counts as an explanation.[11] But even before developing such an account it should not be controversial that in at least some cases, probabilities explain associated frequencies, and that the probability in (2) is such a case.

That (2) explains the fact that most of the silver atoms are deflected up is perhaps most clear when we emphasize that there does not appear to be any alternative explanation of that fact. In particular there is no feature F, which all and only silver atoms that are deflected up have. If there were such an F, we could explain the fact that most (but not all) silver atoms prepared a certain way are deflected up by citing the fact that most (but not all) of those silver atoms have feature F. But there is not; it seems that all we can say is that silver atoms that are prepared in a

[10] A stronger version of the criterion is found in Albert 2000 and Loewer 2001. The main difference between their criterion and mine is that they think that the criterion applies even to single events, whereas the Explanatory Criterion as set out above applies only to patterns of events.

[11] I say more about this project in my paper "Chance, Explanation, and Measure".

certain way are extremely likely to be deflected up. If that fact doesn't explain the fact that most silver atoms are deflected up, then nothing does.

The point is even stronger when we make a couple of additional observations. First, the fact that most (but not all) silver atoms that are prepared in a certain way get deflected up is the sort of fact that cries out for an explanation. It is a robust pattern that has been observed over the course of many experiments done under many different sorts of conditions.[12] And it is one of the fundamental assumptions of our scientific practice that when we observe this sort of robust pattern of events, there is some explanation for it. One of our goals in formulating scientific theories is, apparently, to avoid unexplained coincidences, especially when they are the sort of extraordinary coincidence that we would be dealing with if most (but not all) silver atoms that had been prepared in a certain way went up for no reason at all.

Second, I take it that one of the main motivations for the claim that (2) does not explain why most of the silver atoms go up is the following thought: chances cannot explain long-run frequencies because chances just *are* long-run frequencies. If (2) is just equivalent to the claim that most silver atoms that are prepared in a certain way are deflected up, then (2) cannot explain the fact that most silver atoms that are prepared in a certain way are deflected up. After all, something cannot explain itself.

If correct, this line of reasoning does more than just undermine the specific claim about the explanatory role that (2) plays, it also amounts to an objection to the Explanatory Criterion in general. But it is worth noting how limited in scope this objection is. It is available only to philosophers who endorse a particularly simple form of actual frequentism, according to which the

[12] Crucially I am not claiming that all facts cry out for explanation in this sense. Contrast, for instance, the fact that most silver atoms go up with the fact that one particular silver atom went up. I claim that it is of the utmost importance that we do not leave the former fact unexplained. Not so for the latter fact.

chance of some outcome O occurring as a result of some set-up E is just the actual relative frequency with which outcomes like O result from set-ups like E. These philosophers are few are far between.[13] It is not obvious that the worry extends straightforwardly to more sophisticated versions of frequentism, like hypothetical frequentism (according to which probabilities equal the relative frequency that *would* occur if the experiment in question were repeated infinitely many times) or David Lewis's Humean theory of chance.[14]

But even setting aside the limited scope of this objection, it is important to notice that this worry is getting things backward. Since the main reason to be interested in any particular concept of chance is that it plays a role in our best theories about what the world is like, we need to first identify the scientific role that chances are supposed to play, and only then begin to spell out a more detailed metaphysical theory of what chances are. The Explanatory Criterion identifies one such role. If that criterion conflicts with actual frequentism, so much the worse for actual frequentism.

This critique of actual frequentism will become significantly more pointed in light of what I will argue in the rest of the paper. After all, as things stand right now the frequentist might very well point out that the Explanatory Criterion is only one of two criteria that might be used to make a distinction between objective and epistemic probability. Perhaps these two criteria are really picking out two different notions of chance, only one of which corresponds to the meta-

[13] I don't mean to imply that simple actual frequentism has no intuitive appeal, only that it faces significant philosophical objections. Perhaps the most straightforward and compelling objection to the view is that it seems possible for the actual relative frequency of some type of event to differ from the chance of that type of event. For instance, it seems possible, though unlikely, that a 6 could come up far more often than one sixth of the time, when rolling a fair die. And this possibility is in fact part of what we are trying to convey when we claim that the die is fair.

For a comprehensive rehearsal of the arguments against actual frequentism see Hajek 1996. Note that Hajek takes the fact that actual frequentists cannot account for the explanatory role that chances play as an argument against actual frequentism.

[14] Humeans, at least, are going to be at pains to show that chances, on their view, *can*, play an explanatory role. See van Fraassen 1989, pp. 48-51.

physical theory of chance put forward by frequentists. In fact, in the next section I will present a case that makes it look like this is exactly what is going on: a case where the Indeterminacy Criterion seems to classify the probabilities in a particular theory as epistemic, but the Explanatory Criterion classifies them as objective. This will not prove to be much help to the frequentist however, since I will go on to argue that careful attention to cases like these, instead of giving us reason to endorse two distinct concepts of chance, gives us good reason to reject the Indeterminacy Criterion entirely.

4 A Problem Case

Here is where things stand so far: we have two criteria on the table, the Indeterminacy Criterion and the Explanatory Criterion. Each captures a seemingly important difference between the paradigm cases.

When we try to apply these criteria to a broader range of cases, however, we run into trouble. In particular, we run into cases where the criteria give conflicting results. In this section I will work through one of these problem cases: the case of classical statistical mechanics. This case is particularly interesting in part because it has received a significant amount of attention in the recent literature on probability,[15] and in part because it highlights a specific aspect of the Indeterminacy Criterion—the emphasis on the *microphysical* history of the world—which I will take issue with in my argument against the Indeterminacy Criterion in Section 5.

[15] See Loewer 2001, Albert 2000, Maudlin 2007, Schaffer 2007.

In classical statistical mechanics the dynamical laws that govern the fundamental particles are Newtonian and thus deterministic: the microstate of an isolated system at a time combined with the laws are sufficient to determine the microstate of the system at all other times.[16] Because the macrostate of the system supervenes on the microstate of the system, a complete specification of the microstate and the fundamental laws is also enough to determine the macrostate of the system at all other times.

Things get interesting when we realize that the fundamental dynamical laws apparently allow for behavior that we never observe. For instance, suppose you have a glass of lukewarm water that is sitting in front of you with a couple of ice cubes in it. According to statistical mechanics, there are microstates that are compatible with the current macrostate of the system that lead deterministically to the expected result: in an hour the ice will be melted. But there are also microstates that are compatible with the current macrostate of the system that lead deterministically to a very different result: in an hour you'll have a glass filled with even more ice and a bit of boiling water at the bottom.

So it is possible, given that the glass and the water are in the particular macrostate that they are in, that they are in a microstate that will lead to extremely strange behavior. Why, then, do we never see glasses of lukewarm water with ice in them evolve into glasses of boiling water with even more ice in them? According to statistical mechanics the reason is that although it is possible that the system evolves that way, it is extremely unlikely.

The point generalizes to all sorts of everyday situations. When we stir milk into our coffee it is possible, given the laws of nature and the macrostate, that the milk spontaneously separates

[16] Another way to say the same thing: trajectories through phase space that are compatible with the fundamental laws will never cross. Although there can be violations of determinism when the fundamental laws are Newtonian (see Earman 1986), these violations occur in very specific and unusual situations. No harm comes from ignoring these situations for our purposes.

out again. When we crack an egg into a frying pan it is possible that it regathers itself and hops back into its shell. When we drop a champagne glass from the top of the Empire State building it is possible that a host of air molecules align themselves in such a way that the glass floats gently down to the pavement without breaking. According to statistical mechanics these things are possible but we never, ever see them happen. Why? Because in each of these cases the strange behavior, the behavior we never in fact observe, is extremely unlikely.[17]

Now, given the story I have just told, it is fairly obvious why you might think that the probability distribution used in statistical mechanics is an objective probability distribution. After all it looks like what just happened was the following: we had an extremely robust pattern of long-run frequencies, and we introduced a probability distribution precisely in order to explain that pattern. According to the Explanatory Criterion, then, these probabilities are objective.

According to the Indeterminacy Criterion, however, these probabilities are not genuine chances. Consider the following claim:

(4) Given that a particular glass is full of lukewarm water with ice in it, it is very likely that the ice in the glass will be melted within an hour.

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[17] In technical terms, this involves a couple of steps. First, we separate possible microstates of a system into two different types: the normal microstates and the abnormal microstates. The normal microstates are those that lead to behavior that is compatible with the thermodynamic laws, the sort of behavior that we in fact observe: milk mixed into your coffee stays mixed, glasses dropped off of the Empire State Building break, and so on. The abnormal microstates are all the others.

Second we place a uniform measure over phase space—a 6N dimensional space in which three dimensions correspond to the position of each particle in the system and three dimensions correspond to the momentum of each particle in the system. It is one of the central claims of this approach to statistical mechanics that for any particular system, within any region of phase space that corresponds to a potential macrostate of that system, the vast, vast majority of microstates in that region will be normal microstates.

The final step is to normalize the measure over phase space and interpret it as a probability measure. The measure tells us the probability of a system being in a particular microstate given that it is in a particular macrostate. And according to the probability measure, for pretty much any non-gerrymandered macrostate, it is extremely likely that given that the system is in that macrostate, it will be in a normal microstate.

The microphysical history of the world up until the time at which (4) is asserted includes the microstate of the world at that time; therefore that history either entails that the ice is melted in an hour, or entails that it is not. Suppose that it in fact entails that the ice is melted. Then every world which matches the actual world in its microphysical history up until t, and in its fundamental laws, will be a world in which the ice melts. It follows from the Indeterminacy Criterion that the chance, in the actual world, at t, that the ice will not melt, is zero. But if the chance that the ice will be melted in ten minutes is zero, then (4) can only be true if it not making a claim about chance.

Philosophers who have considered statistical mechanical probabilities have generally responded to this tension between the Indeterminacy Criterion and the Explanatory Criterion in one of two ways. One approach is to take seriously the result of applying both the Explanatory Criterion and the Indeterminacy Criterion to the case of statistical mechanics, and claim that our concept of chance actually fractures into multiple concepts. On this view, there is more than one sort of chance; the probability in (4) is a genuine chance in one sense (the sense picked out by the Explanatory Criterion), but not a genuine chance in another sense (the sense picked out by the Indeterminacy Criterion.) An example of this approach is found in Maudlin 2007, where he distinguishes between genuine stochastic dynamics, on the one hand, and what he calls instances of *typicality*, on the other.

A second approach, found in Schaffer 2007, is to disregard the Explanatory Criterion and claim that the probabilities in statistical mechanics are mere epistemic probabilities on the basis of the Indeterminacy Criterion. This approach can take one of two forms. Either one can argue against the Explanatory Criterion generally, or one can argue that the Explanatory Criterion was misapplied in the statistical mechanics case.[18]

Each of these options come with philosophical costs. It would be better, all things considered, if we could come up with a single theory of chance that captured both of the differences observed between the paradigm cases of objective and epistemic probabilities. And arguing that statistical mechanical probabilities do not explain associated frequencies involves reinterpreting a lot of what scientists say and write about statistical mechanics. Scientists generally talk as though the probability in (4) explains the fact that usually, when we have a glass of ice water in front of us, the ice is melted within an hour; in fact, they often say something stronger: that on any particular occasion when ice melts within an hour, (4) explains why it melted.[19] It also may require reinterpreting the historical account of why scientists adopted statistical mechanics instead of alternative theories, in the second half of the nineteenth century. According to some historical accounts,[20] at the time it gained wide-spread acceptance statistical mechanics made no novel predictions; it differed from alternative theories only in the *explanations* it gave of already well-documented phenomena.

In the next section, I will argue that there is no need to pay any of these philosophical costs, because in fact we have no good reason to accept the Indeterminacy Criterion. The line of reasoning that motivated that criterion, while initially attractive, is unstable. The same reasoning could be used to justify an even stronger claim about chance. And it follows from this stronger claim that there are no chances, according to our best scientific theories.

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[18] I am unsure which of these arguments to attribute to Schaffer since he focuses on the question of whether or not probabilities can explain single case outcomes (i.e. whether or not the fact that it is very likely that the next silver atom goes up can explain why the next silver atom goes up), whereas the Explanatory Criterion as I have set it out applies only to patterns of outcomes.

[19] See Albert 2000, pp. 64-65.

[20] For instance, Strevens 2000, pp. 12-14, which is a concise version of the account found in Brush 1983.

5 Against the Indeterminacy Criterion

In this section I will argue against using the Indeterminacy Criterion to distinguish between objective and epistemic probability. The argument begins by comparing the Indeterminacy Criterion, presented again here:

> *The Indeterminacy Criterion*
>
> If the chance, at world ω, at time t, of proposition p is greater than 0, then there exists a world ω' such that (i) ω' matches ω in laws, (ii) ω' and ω have the same microphysical history up until time t, and (iii) p is true at ω'.

with the following alternative criterion:

> *The Macrophysical Indeterminacy Criterion*
>
> If the chance, at world ω, at time t, of proposition p is greater than 0, then there exists a world ω' such that (i) ω' matches ω in laws, (ii) ω' and ω have the same macrophysical history up until time t, and (iii) p is true at ω'.

Notice that the Indeterminacy Criterion and the Macrophysical Indeterminacy Criterion give importantly different results. If we adopt the original Indeterminacy Criterion, statistical mechanical probabilities are not genuine chances. If we adopt the Macrophysical Indeterminacy

Criterion, then statistical mechanical probabilities may well be. (The macrophysical history of the world up until the time that (4) is asserted, in combination with the laws of nature, does not entail either that the ice will melt or that it will not melt.) So if we are going to adopt the Indeterminacy Criterion we need to come up with a good reason why. We need to identify some important difference between these two criteria; a difference that justifies our using the former instead of the latter.

I claim that this cannot be done. There is no good reason for adopting the Indeterminacy Criterion instead of the Macrophysical Indeterminacy Criterion.

Look again at the two criteria. Both rely on a claim about what is possible relative to a certain set of facts. According to the former what is important is whether or not an outcome is possible, given the set of propositions that pick out *the entire microphysical history of the world up until t* and the actual laws of nature. According to the latter what is important is whether or not an outcome is possible given the set of propositions that pick out *just the macrophysical history of the world up until t* and the actual laws of nature.

In neither case, however, is what is important whether or not an outcome is possible given the set of propositions that pick out *the entire history of the world and the laws of nature.* According to the Macrophysical Indeterminacy Criterion there are some facts that do not matter, that we are allowed to ignore when figuring out what the chances are—facts about the microphysical level. But it is also the case that according to the Indeterminacy Criterion there are some facts that do not matter, that we are allowed to ignore when figuring out what the chances are—facts about the future.

Any good reason for using the Indeterminacy Criterion instead of the Macrophysical Indeterminacy Criterion will therefore rely on there being an important difference between restricting our attention to just the facts about the present and the past, on the one hand, and restricting our attention to just facts about the macrophysical level, on the other hand. I will argue that there is no such difference. To restrict our attention to the facts about the past and present is just as arbitrary as restricting our attention to facts about the macrophysical world.

There is no question but that this is a surprising claim to make. Our pre-theoretical understanding of the world around us implies that the past and the present are importantly different from the future. But there are good reasons, both philosophical and scientific, to think our pre-theoretical understanding is wrong.

5.1 The ontological motivation

The most straightforward reason to favor the Indeterminacy Criterion over of the Macrophysical Indeterminacy Criterion is an ontological reason. If you think the future does not exist, then you will think that the reason that we ought to determine the chances relative to the entire microphysical history of the world, instead of relative to just the macrophysical history of the world, is simple: the latter leaves something out, whereas the former is complete.

In order to make the sort of view I have in mind explicit, assume for the moment that there is a unique way of separating spacetime into regions of space and instants of time. We can divide metaphysical theories of spacetime into two different camps: those according to which the future is *open*, and those according to which the future is *closed.* According to both the open-future view and the closed-future view the future will exist, and the future does not presently exist. The crucial difference between the two views is that according to the closed-future view the future also exists, where 'exists' is meant tenselessly. On the open-future view, it does not.[21]

The question of whether or not the future is open is orthogonal to the question of whether or not the fundamental laws are deterministic. If the fundamental laws are deterministic then a complete specification of the present state of the universe plus the laws of nature determine what the complete state of the universe will be at all future times, but that does not address the issue of whether or not those other times exist. Nonetheless, adopting an open future view would motivate the Indeterminacy Criterion. If you assume that we live in a universe with an open future, then it is easy to see why the Indeterminacy Criterion is interested in possibility relative to the entire microphysical history of the world up until the relevant time—the entire microphysical history of the world up until that time is all there is.

There are two main lines of objection to the open-future view. The first focuses on difficulties involved in spelling the view out in a philosophically rigorous manner. The view that the future does not exist but will exist seems to require that there is an objective flow of time, sometimes called *objective becoming*, and various philosophers have argued that this objective flow of time is difficult to make sense of. They say things like: "If there is some objective becoming, then at the moment the universe is smaller than it will be in the future; but if the universe is expanding, what is it expanding into? At what rate is it growing?"[22]

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[21] If you're tempted to think there isn't a genuine debate here, or to worry that there's something suspect going on with the tenseless use of 'exists', it helps to draw an analogy with the debate over the existence of concrete possible worlds. According to both the actualist and the modal realist, merely possible worlds possibly exist, and merely possible worlds don't actually exist. What the two views disagree about is whether merely possible worlds exist *simpliciter.*

[22] For further discussion of these and other issues see the first chapter of Price 1996, Markosian 1993, Smart 1949. A recent defense of the view that time passes can be found in Maudlin 2007c—but that view, notably, does not require that the future be open in the sense I've described above, merely that it have a unique direction.

These are notoriously tricky questions to answer, and I won't go into them in detail here, because there is a second sort of objection to the open-future view, an objection which is, in and of itself, decisive. Accepting this second objection is enough to render the question of whether or not objective becoming makes sense effectively moot. The second objection is this: the open-future view is incompatible with our best contemporary scientific theories.

In relativistic theories of spacetime there is no absolute standard of simultaneity. In other words, according to these theories, there is no privileged way to separate four-dimensional spacetime into three spatial dimensions and one temporal dimension. There are instead many ways of separating regions of space from instants of time, each of which is called a *reference frame*. Depending on how fast you are moving relative to other entities, different reference frames will be more or less natural choices for you to use when making calculations or in representing various states of affairs, but no choice of reference frame is privileged. According to our best scientific theories, then, there is no single answer to the question: are events e_1 and e_2 simultaneous? Similarly there is not, in general, a single answer to the question: is e_1 past, present, or future? And if we adopt an open-future view, according to which only the past and present exist, then there will accordingly be no single answer to the question: does e_1 exist. But to endorse that sort of relativism about existence is absurd.[23]

These considerations put us back where we started: What justifies choosing the original Indeterminacy Criterion over the Macrophysical Indeterminacy Criterion? Both evaluate prob-

[23] See Sider 2003 for more on this objection. This is a complicated issue, and deserves more attention than I have space to give it here, especially as there are a few philosophers who had taken the position that in light of the conflict between the open-future view and special relativity, we ought to revise special relativity. (Perhaps the best examples of this is found in Prior 1996.) Two points are worth keeping in mind if one is at all sympathetic to these revisionist views. First, revising special relativity will involve giving up a certain sot of simplicity assumption that has played a significant role in numerous instances of scientific theory choice. Second, even if we can save the open-future view from this objection by altering special relativity, it is by no means obvious whether a similar move is possible once we move from special relativity to general relativity.

abilities on the basis of what is possible, relative to some *restricted* set of propositions. In what sense is the set of propositions that includes the microphysical history of the actual world up until the present a better context against which to judge whether or not a probability is a genuine chance than the set of propositions that include just the macrophysical history of the actual world up until the present? Since we live in a closed-future universe, neither includes all of the propositions that are true of the actual world.

In fact, if the ontological motivation was what was really behind the Indeterminacy Criterion then, in light of the fact that we live in a closed-future universe, it looks like we should endorse the following criteria:

The Absolute Indeterminacy Criterion

If the chance, at world ω, at time t, of proposition p is greater than 0, then there exists a world ω' such that (i) ω' matches ω in laws, (ii) ω' and ω have the same complete history through all time, and (iii) p is true at ω'.

But by this standard not even the probabilities found in indeterministic theories, like the GRW interpretation of quantum mechanics, count as objective. That is to say, if you insist on using the Absolute Indeterminacy Criterion in order to distinguish between objective probabilities and mere epistemic probabilities then you will have adopted a distinction that has no role in a scientifically-informed metaphysics. And before we conduct a more thorough investigation into other possible ways of making the distinction—in particular, before we take a closer look at the Explanatory Criterion—that conclusion seems premature.

5.2 Two Alternative Motivations

Of course the ontological motivation is not the only possible motivation for choosing the Indeterminacy Criterion over the Macrophysical Indeterminacy Criterion. Perhaps the reason we should be interested in what is possible relative to the microphysical history up until some time is not because that is all there is, but instead because that history is important or special in some more general metaphysical sense. Of course just saying that the past is "special" isn't enough: what we are looking for is some reason for thinking that facts about the present and past contribute differently to fixing the chances than do facts about the future.[24]

As far as I can tell, the most promising way to spell this out appeals to a temporal asymmetry in causation. It is common to assume, either implicitly or explicitly, that causes must precede their effects. And assuming that causes always precede their effects, if you take into account the entire microphysical history leading up to some event, you will have taken into account all of the potential causes of that event. Perhaps it is for this reason that we should favor the Indeterminacy Criterion over the Macrophysical Indeterminacy Criterion.[25]

The first thing to say about this motivation is that it may well be that if we fully internalize the discussion above about the implausibility of an open-future view, this claim about causation (that causes always precede their effects) is something that should itself be revisited. But even if we assume for the moment that causes do always precede their effects, at most this assumption motivates the claim that we should restrict our attention to past facts when trying to determine

[24] For instance, it will not do to just appeal to the fact that the microphysical facts are fundamental while the macrophysical facts are not. Remember that, according to the Indeterminacy Criterion we are allowed to ignore some of the fundamental facts—the facts about the future. How then can it be an objection to the Macrophysical Indeterminacy Criterion that it discounts some of the fundamental facts?

[25] Thanks to Brad Skow for suggesting this motivation.

what the chances are. It does not motivate the claim that we should take into account *all* of the past facts.

Again, suppose we grant that the chance that some event e will occur is fixed by everything that plays a causal role in bringing e about, and that everything that plays a causal role in bringing e about occurs before e. This alone does not require that we accept the Indeterminacy Criterion over the Macrophysical Indeterminacy Criterion. Rather, it seems that what we ought to do in any particular context is determine what the causal facts are in that context, and then hold those fixed. And it is by no means obvious that the causal facts will always include facts about the microphysical level.

One some accounts of causation, for instance, causes must be *commensurate* or *proportional* to their events. In other words, they should not include too much extraneous information.[26] On that view, it looks as though what causes the ice in a glass of water to melt is the initial *macrostate* of the system, not the initial *microstate*. After all, the vast, vast majority of the information contained in the initial microstate could have been different and the macrophysical behavior of the ice remained exactly the same.

There is one final alternative motivation that I will consider. Maybe taking into account the microphysical history up until some time t is the best we can do, not because of some independent metaphysical feature of the world, like that the future doesn't exist or that causes always precede their effects, but because of some fact about us. Perhaps the reason why we should use the Indeterminacy Criterion instead of the Macrophysical Indeterminacy Criterion to pick out the objective probabilities is because we have a different sort of *access* to microphysical facts than we do to facts about the future. We just don't have access to the future except by way of facts

[26] See, for instance, Yablo 1992 and Strevens 2000.

about the present or past in the same way that we have access to facts about what is going on at the microphysical level.

The problem with this motivation is that when you focus on the sort of microphysical facts that actually play a role in theories like statistical mechanics, it is by no means obvious that we have a different sort of access to these microphysical facts than we have to the future facts. Even assuming that the glass of water with ice in it is an isolated system, in order to determine, for certain, whether or not the ice will melt, we would need to know the exact position and momentum of each of the fundamental particles in the glass, the water, and the ice at a single time. This is not the sort of information that we actually have access to, when we are trying to predict or explain the evolution of an everyday macrophysical object like a glass of water with ice floating in it.

Of course, it is easy to imagine someone protesting that what is important is not the sort of information we *actually* have access to, but instead the sort of information that we could *in principle* have access to. We could *in principle* know the exact microstate of the system; we could not in principle know what is going to happen in the future, except to the extent that it is determined by some present events. In response to this I can only request clarification as to what is meant by "in principle". Certainly you and I cannot determine the exact microstate of the cup of water with ice in it; neither can the best-equipped physicists. Perhaps the idea is that a different sort of being than us, the sort of demon that Maxwell once conjectured, would be able to determine the exact microstate. But unless there is some important metaphysical distinction between the past and the present on the one hand, and the future on the other, then it isn't obvious why there couldn't also be an analogous being, an Einsteinian demon, say, who would be able to determine the exact future state of the cup.

6 Chance and Explanation

The arguments of the previous section show that to the extent that we want a distinction between objective and epistemic probability that plays a scientifically significant role that distinction cannot be based on the Indeterminacy Criterion. So, assuming there is some other plausible way to draw that distinction, we ought to give up on the Indeterminacy Criterion.

This conclusion yields important results. First, if we give up the Indeterminacy Criterion, a probability may be a genuine chance even if it is part of a deterministic theory, and even if it attaches to a proposition that is entirely about the present or past. As a result, instead of thinking of chance as being relative to a specific world and time of evaluation as Lewis did, we should think of chance as being relative to a more general context—where a context is a just a set of propositions. Relative to the set of propositions that includes facts about the current microstate of the glass, the chance that the ice will be melted in an hour is either 0 or 1. But relative to the set of propositions that includes only facts about the current macrostate of the glass, the chance that the ice will be melted in an hour is neither 0 nor 1, but some number very close to 1. There is no contradiction here, just as there is no contradiction in saying that the chance that the ice will melt within an hour is not quite 1 now, even though in 59 minutes the chance that it will melt within an hour will be 1 (because it has already melted, say).[27]

Second, giving up on the Indeterminacy Criterion removes a significant obstacle that might previously have deterred us from taking seriously the role that explanation plays in picking out a distinctive category of probabilities. The important difference between the paradigm cases that we are left with is a difference in explanatory power. If we are interested in picking out a class of probabilities that has any hope of playing a scientifically significant role, we should be interested in the probabilities that play a certain explanatory role—the probabilities that explain long-run frequencies.

That chances are the probabilities that play this distinctive explanatory role will constrain what sort of further theory we can adopt as to what chances are. For instance, this view rules out simple frequentist interpretations of chance. If chances have to explain actual long-run frequencies, then they cannot simply be actual long-run frequencies. It also puts some pressure on more sophisticated versions of frequentism, like hypothetical frequentism and Lewis's Humean theory of chance.

When it comes to the role that chance plays in various scientific theories, this is a somewhat disappointing conclusion. After all, if we had good reason to accept both the Indeterminacy Criterion and the Explanatory Criterion and to think both criteria targeted a single concept of chance, then we could put these criteria to significant use. In particular, if we had an independent

[27] If you insist on keeping chance a three-place function from propositions, world, and times, these considerations will likely push you toward a view like the one put forward in Loewer 2001, where deterministic chances are probabilities over the initial state of the universe, and therefore are something significantly different than the chances that arise in an indeterministic context. But in light of the argument we used above against the Indeterminacy Criterion, I can see no reason for refusing to think that chances can be relative to a context, more generally. There is nothing special about the set of propositions that pick out the entire history of the world up until a particular time. It is still a restriction of the total set of propositions that are true of the world. And unless we have a good reason to think otherwise, a unified metaphysical theory chance is preferable.

grip on which probabilities were chances, and we knew that chances could play a certain explanatory role, then we might have been able to learn something about the explanatory power of certain scientific theories that we had not known, or had not been able to justify clearly before.

Consider, for instance, Bohmian mechanics, one of the various competing interpretations of quantum mechanical phenomena. Like all such interpretations, Bohmian mechanics relies heavily on probabilistic claims similar in form to (1) and (2). But since Bohmian mechanics is a deterministic theory, it would follow from the Indeterminacy Criterion that the probabilities it employs are epistemic. And if the probabilities in Bohmian mechanics are merely epistemic, it would follow from the Explanatory Criterion that those probabilities do not explain any associated frequencies. This would be an important fact to learn, one that is likely end up influencing how we evaluate Bohmian mechanics in relation to its competitors. It would also be interesting from a purely philosophical point of view: by looking at something which is relatively simple to appraise—whether or not a theory is deterministic—we can gain insight into a question that looks much more difficult to answer—what counts as a genuine probabilistic explanation.

Unfortunately, one upshot of getting rid of the Indeterminacy Criterion is that we loose any independent grip we had on the relevant distinction; in order to decide if the probabilities employed in the theory are objective we have to directly investigate the extent to which those probabilities play an explanatory role. But then learning that the probabilities are objective does not tell us anything we did not already know.

So in some ways this is a frustrating conclusion but it does help us sharpen our focus. What we should be interested in is coming up with a more detailed account of probabilistic explanation, and especially probabilistic explanation in deterministic contexts. With that sort of theory in hand we can evaluate the relative merits of theories like Bohmian mechanics and other interpretations of quantum mechanics directly.

Finally, it is worth asking where this conclusion leaves us with respect to the initially plausible connection between chance and indeterminacy that was spelled out in the introduction. Recall that although according the original Indeterminacy Criterion the probabilities in statistical mechanics were epistemic, there was an alternative criterion—the Macrophysical Indeterminacy Criterion—according to which they might still count as genuine chances. This suggests that at the very least we will end up with the following generalized connection between indeterminacy and chance: if there is a genuine chance that something will happen then, relative to some context—some set of propositions—it is not yet determined whether that thing will happen. But this sort of indeterminacy is playing no important role in distinguish chance from mere epistemic probability. In order to learn more about what chances are, I have suggested, we should turn instead to the connection between chance and explanation.

7 Conclusion

Objective probabilities are compatible with deterministic laws. This claim does not follow from any particular metaphysical theory of chance. It follows instead from the fact that we are only interested in chances insofar as they play a role in our best theories of what the world is like, and a closer look at those theories reveals that the distinction between probabilities that arise when the fundamental laws are indeterministic, and those that arise when the fundamental laws are deterministic, is not a significant distinction. Instead the significant distinction seems to be between the probabilities that are able to explain associated long-run frequencies, and those that are not.

Chances, then, are not the probabilities that arise when the fundamental laws are indeterministic. Instead chances are those probabilities that play a certain explanatory role.

This framework sets us up quite nicely with a program for future research. We have in hand a criterion that distinguishes objective from epistemic probability. But in order to apply that criterion broadly and in controversial cases we need to further spell out what counts as a genuine instance of probabilistic explanation. I develop this account and show how it yields important results for both scientific and philosophical projects that go beyond pure metaphysics in the next two chapters of my dissertation.

2

CHANCE, EXPLANATION, AND MEASURE

Suppose that you have been spending some time watching the roulette tables in Las Vegas and you have noticed a few patterns in the games you have observed: the dealer always releases the ball and spins the wheel in opposite directions, there are always an even number of red and black numbers on the wheel, and the ball never lands on the same number twice in a row.

With each of these patterns, as with any robust pattern in the world around us, two related questions arise: First, what explains the pattern? And second, should you expect the pattern to continue? (The questions are related because the answer to the first generally provides you with an answer to the second.) For the first two patterns, the answer to these questions is simple, so simple that you're unlikely to spend much time thinking about it unless you have never played roulette before: It is part of the rules of the game that the dealer must release the ball and spin the wheel in opposite directions, and that there are always an even number of red and black numbers on the wheel. There aren't an uneven number of red and black numbers because there *cannot* be; the dealer doesn't spin the ball and the wheel in the same direction because he *cannot.* So, yes, you should expect these patterns to continue.

For the third pattern you observed things are a little trickier. If you have observed enough roulette wheels you are likely to also have noticed that the outcome of each spin does not appear to depend on the outcomes of previous spins. And close examination of the roulette set-up should convince you that there is nothing in the operation of the wheel that would make successive spins correlated in any way. These facts give you very good reason for thinking that the ball can in fact land on the same number twice in a row. So what explains the pattern you saw? Presumably the roulette ball very rarely land on the same number twice in a row because, although the ball *can* land on the same number twice in a row, the probability of that happening is very small.

At least at first glance, this sort of everyday, probabilistic reasoning appears perfectly acceptable. We observed a robust pattern in the phenomena—the relative frequency of a certain type of event happening is extremely low. A natural first move is to give a modal explanation for this pattern—events of that type do not happen in those situations because they *cannot.* When presented with evidence that undermines the modal explanation, a natural second move is to retreat from the *modal explanation* to a *probabilistic one*. It isn't that events of the relevant type *cannot* happen in the relevant sort of situation, they are merely *very unlikely*.

Philosophically, however, this sort of reasoning is enormously controversial. The heart of the issue is this. In order for the proposed explanation to have any hope of working we need to interpret the claim that the ball is unlikely to land on the same number twice as claim about some *objective probability*—some feature of the world that exists independently of our beliefs and expectations. (As sufficient time observing the roulette tables should have taught you, where you or I expect the ball to land has nothing whatsoever to do with where it actually lands.) At the same time the claim that that the ball is unlikely to land on the same number twice does not appear to be a *fundamental probability*—it does not appear in the fundamental dynamical laws, and that has as a consequence the indeterminacy of those laws. The same bit of reasoning would seem just as acceptable, even if there was nothing probabilistic at all about what is going on at the fundamental level when the roulette wheel gets spun.

It is not difficult to come up with an interpretation of non-fundamental objective probabilities. A simple frequentist interpretation will do. On such an account all it means to say that the ball is very unlikely to land on the same number twice is just that, most of the time, balls do not land on the same number twice. But notice that this account undermines the explanatory role that non-fundamental objective probabilities appeared to be playing in the example above. On this account, that it is very unlikely for a roulette ball to land on the same number twice *just means* that, most of the time, roulette balls don't land on the same number twice, so it cannot *explain* the fact that, most of the time, roulette balls don't land on the same number twice.

The goal of this paper is to present a metaphysical interpretation of non-fundamental objective probabilities that does not undermine the explanatory role that non-fundamental objective probabilities appear to play in examples similar to the one above. Elsewhere I have argued that probabilities that play this sort of explanatory role are a group of special metaphysical significance, and have an especially good claim to the name 'chance'. Put in my favored terminology then, the main goal of what follows is to present an metaphysical account of *non-fundamental chance*.

It is worth emphasizing, as I hope that the above example does, that however philosophically contentious these kinds of probabilities may be, they are *prima facie* acceptable. But *prima facie* acceptability does not always count for much. Sometimes our philosophical views force us to reinterpret significant pieces of our everyday discourse. So it is significant that there are, and much of my focus in what follows will be on, instances of the same pattern of reasoning our best scientific theories. In the next two sections of the paper I will work through my theory of non-fundamental chance as it applies to statistical mechanics. This is the most straightforward instance of non-fundamental chance, but not the only one. In the fourth and final section of the paper I will demonstrate how the account generalizes to other non-fundamental sciences, and even how it might underwrite the sort of everyday reasoning that appears in the example I started with.

One final disclaimer. In what follows I have tried to say very little that is controversial about the nature of explanation in general. This may surprise readers familiar with recent works like Strevens 2008, Woodward 2005, and Weslake 2010, in which the authors present highly detailed accounts of explanation that allow for non-fundamental probabilistic explanation. In part this is a practical matter—giving such an account is far beyond the scope of the present paper. But in addition, as this paper will demonstrate, it turns out that a detailed and controversial account of explanation is not required in order to establish that there are non-fundamental objective probabilities that play an important explanatory role in some of our best scientific theories. Even those who do not accept the specifics of Strevens's of Woodward's or Weslake's accounts of explanation can still accept my theory of non-fundamental chance.

1 Non-Fundamental Chance in Statistical Mechanics

The world around us appears to be temporally asymmetric. There are all sorts of entirely everyday and apparently disparate processes which apparently never happen in reverse. Ice cubes melt when placed in glasses of room-temperature water, but cold water never spontaneously separates into room-temperature water with an ice cube floating in it. Acorns grow into oak trees, but oak trees never shrink back down into acorns. When a balloon is punctured all of the air rushes out of it, but air never forces itself into an already deflated balloon.

One way of explaining these phenomena is by appeal to the Second Law of Thermodynamics. If processes like the ones described above were to happen in reverse they would involve a decrease in the value of a particular property of the system; a property called *entropy*. And, according to the Second Law, the entropy of an isolated system never decreases over time.[28] From the perspective of navigating everyday life, all of this is well and good. Assume that the Second Law of Thermodynamics is indeed a law and you'll be able to predict and explain virtually all the phenomena you encounter on a regular basis. But from a scientific perspective, this explanation is problematic. Entropy, as it arises in thermodynamics, is a *macroscopic* variable; systems which have the same entropy can still differ enormously in their *microscopic* arrangement—the positions and velocities of the individual particles that make up that system. And when we look at the laws that govern the way that microscopic arrangements of particles evolve over time, it turns out that these laws *do* allow for entropy to decrease over time.

Take a system that consists of a glass of room temperature water and the air immediately surrounding it. It is possible, according to these dynamical laws, to arrange the particles that make up this system so that an ice cube spontaneously forms in the middle of the glass. So why don't we ever see ice cubes spontaneously forming in glasses of water? According to one influential interpretation of Statistical Mechanics, the answer to this question is probabilistic. Although it is possible for an ice cube to spontaneously form in a glass of water, it is very unlikely.

This retreat, from a *modal* explanation to a *probabilistic* one, should look familiar—it is exactly the same sort of reasoning that looked so appealing in the roulette case. And as in the roulette case, if this proposed probabilistic explanation is going to be satisfactory, the probability of the

[28] This is not to say that entropy always increases. If the system is in the so-called equilibrium state—the state with maximal entropy—then it will stay there. We are of course assuming here that we are dealing with systems that are suitably isolated. See Elga 2004.

ice cube forming must be an *objective probability*—a probability that exists independently of our beliefs or expectations. Whether or not I happen to expect ice cubes to spontaneously form in glasses of water plays no role whatsoever in explaining why they in fact form or do not form.

But the probability at play here cannot be a *fundamental* probability. The fundamental laws, according to statistical mechanics, are not probabilistic. For any given situation where there is a glass of room-temperature water, the dynamical laws plus a complete specification of the microscopic arrangement of the particles, either entails that an ice cube spontaneously forms in the glass, or entails that it doesn't; these laws never entail that an ice cube will spontaneously form with some probability between 0 and 1.

The goals of the next two sections, then, is to present a way of understand (2) such that (2) explains (1).

(1) We never observe ice cubes spontaneously forming in glasses of room-temperature water.

(2) The chance of an ice cube forming in a glass of room-temperature water, though non-zero, is extremely low.

1.1 The Statistical Mechanical Chance Postulate

A helpful way to think about classical statistical mechanics is in terms of *phase space*.[29] The phase space of an N-particle system is a 6N-dimensional space in which we use three dimensions to specify the position of each particle and three dimensions to specify the momentum of each particle. Each point in phase space corresponds to a possible *microstate* of the system—a complete specification of the positions and velocities of each of the particles in the system—and a trajectory through phase space corresponds to a possible continuous sequence of microstates— one way that the system could evolve over time. Since the laws governing the behavior of the fundamental particles are deterministic, trajectories through phase space do not cross; for any given point in phase space there is a unique trajectory that passes through it. And since the macrovariables of a system (e.g. the entropy) supervene on the microstate of the system, each point in phase space falls into one of two groups. Either it lies on a trajectory that leads to entropy decreasing over time, or it does not. If it does lie on such a trajectory we call it an *abnormal microstate.* If it doesn't, we call it a *normal microstate.*

Regions of phase phase space correspond to various macrophysical conditions (e.g. being at 72° F, or having a volume equal to $2m^3$). Strictly speaking, statistical mechanics deals with regions of phase space that correspond to the specification of certain macrophysical variables like pressure, temperature, and volume. For our purposes, when we talk about the *macrostate* of the system, we're referring to those, or even more coarse-grained states of the system, e.g. being a

[29] In this section I am following Boltzmann's approach to statistical mechanics (as opposed to Gibbs's approach.) This is standard in textbooks on statistical mechanics, as well as recent philosophical discussion. See Uffink 2006 for details.

glass of water. (Where the latter can be thought of as a disjunction of a series of statistical mechanical macrostates, the limits of which are set by linguistic context, e.g. we do not generally refer to anything with a volume larger than 100ml as a 'glass'.)

Here is an empirical claim about phase space, which has been argued for extensively and which I will assume to be true:

> *Empirical Claim*
>
> Within any macrostate, the region containing all and only the abnormal microstates is *tiny* and *scattered*.[30]

What it means to say that a region in phase space is scattered should be fairly obvious: the points that make up that region are generally surrounded by points that belong to some other region; they are not clustered together. What it means to say that a region in phase space is small is not as straightforward. After all, every region within phase space contains infinitely many points. In order to handle this issue, phase space comes with a standard measure which attaches volumes to regions of phase space. Using the standard measure you can assign relative sizes to subregions within various macrostates. You can determine for instance, whether, within the region that corresponds to being a glass of water, the subregion that contains all and only the normal microstates is larger than the subregion that contains all and only the abnormal microstates.[31]

Consider a system that consists of a glass, filled with water, and the air immediately surrounding it. According to the above claim, the region of phase space that represents this system

[30] You can show this to be true for very simple systems in particularly simple macrostates, but it is a significant assumption to claim it is true for all systems in all macrostates. But it is an assumption generally agreed upon by philosophers and physicists. Cf. Albert 2000, Uffink 2006, Sklar 1995.

[31] What determines the standard measure? I answer this question in section 1.3.

being in the described macrostate is taken up almost entirely by points that correspond to normal microstates—microstates that entail that no ice cube will spontaneously form in the glass. There are points within that region that correspond to abnormal microstates—those that lead to an ice cube spontaneously forming—but they take up a tiny portion of the space and are scattered widely throughout it.

So there is a correlation between the relative size of regions of phase space and the relative frequency with which we observe certain sorts of events: within the region that corresponds to being a glass of water, the size of the region corresponding to the abnormal microstates is tiny; and so is the relative frequency with which we observe ice cubes spontaneously forming in glasses of water. And this correlation suggests that there is a connection between size of regions of phase space and chance, along the following lines:

> *The Statistical Mechanical Chance Postulate*
>
> Given that it is in a certain macrostate M, the chance that a system is in an initial microstate with feature F is the normalized standard measure of microstates compatible with M that have feature F.[32]

The Statistical Mechanical Chance Postulate says, then, that what it means for the chance of some initial macrostate leading to some outcome to be very high is, roughly, that most of the ways of of realizing that initial macrostate are such that they lead to that outcome.

1.2 The Macro-Micro Relativity of Chance

[32] The normalized standard measure is just the standard measure normalized so that the measure of the region corresponding to the macrostate is 1.

The immediate worry that is raised about the Statistical Mechanical Chance Postulate is that it seems to lead to conflicts of the following sort. Take a particular system, S, which consists of a glass of water and the air immediately surrounding it. Most of the microstates that are compatible with this macrostate are normal microstates. So, according to the Statistical Mechanical Chance Postulate, the chance of an ice cube spontaneously forming in S is very low. But suppose that S is in fact in an abnormal microstate, specifically one that leads to an ice cube spontaneously forming in the glass. Then the initial microstate of S, plus the laws of nature, entail that an ice cube will spontaneously form in the glass. So it seems that the chance of an ice cube spontaneously forming in the glass is actually 1. Isn't this a contradiction? The chance of an ice cube spontaneously forming can't be both very low and 1, can it?

This is the same worry that motivates David Albert, who argues for the Empirical Claim above, to resist the Statistical Mechanical Chance Postulate.[33] Albert observes, correctly, that we ought to make predictions and provide explanations using the chances given by the Statistical Mechanical Chance Postulate *only under certain conditions*. If we know the initial microstate of a particular glass of water there is no reason to consult the Non-fundamental Chance Postulate at all, indeed that postulate might turn out to be seriously misleading.

On the face of it, this seems like a troubling observation. But consider an analogous argument which applies to fundamental probability. The GRW interpretation of quantum mechanics allows you to calculate a fixed probability per unit time of certain odd phenomena. For in-

[33] The relevant passage is the bottom of page 64 and the top of page 65. Albert ends up endorsing the weaker Statistical Postulate: "The right probability distribution to use for making inferences about the past and the future is the one that's uniform, on the standard measure, over those regions of phase space which are compatible with whatever other information—either in the form of *laws* or in the form of *contingent empirical facts*—we happen to have." Notice the difference: the Statistical Postulate doesn't say anything about what the probability actually is, only about what probability distribution we ought to use under certain conditions of ignorance.

stance, using the GRW theory we can calculate, for any particular glass of water, the probability of an ice cube that was previously in the nearest freezer spontaneously disappearing from the freezer and appearing in that glass within the next five minutes—this probability will be extremely low, but it will be non-zero. But notice that this probability distribution is the correct probability distribution to use in making predictions only under certain conditions. If we know somehow (an oracle tells us, say) that in five minutes there is no ice cube in the glass, there is no reason to consult the GRW interpretation, indeed the dynamical laws of that interpretation might turn out to be seriously misleading.

This argument is not taken to present a seriously objection to the GRW interpretation. Instead it shows is that it is common ground that chance—fundamental or non-fundamental—can be relative to time. The chance of some event happening may be different depending on the time at which you evaluate it. Similarly, I take it, it is common ground that chance is relative to a specification of the laws of nature. The chance of some event happening may be different if the laws of nature are different. The thought behind the Statistical Mechanical Chance Postulate, then, is that just as chances are relative to the laws, or to a specification of a time, they can also be relative to the context of evaluation in a more general sense. In particular, the chance of some event happening may be different depending on whether you evaluate it relative the microstate or macrostate of the system.[34]

Making use of this additional relativity, we can respond to the worry above in the following way. It is not the case that the chance of an ice cube spontaneously forming is both low and high, because there is no fact of the matter as to the chance of an ice cube forming *full stop.*

[34] Glynn 2010 uses a similar argument to motivate what he calls the "level-relativity" of chances.

Claims about chance only make sense relative to a specification of the macro- or micro-level.[35] (Just as we cannot give the chance of an ice cube tunneling out of the freezer *full stop*; claims about chance only make sense relative to a specification of the time of evaluation.)

What is true, is that the chance of an ice cube spontaneously forming *relative to the macrostate of S* is very low, and the chance of an ice cube spontaneously forming *relative to the microstate of S* is very high (in fact it is 1). But whether or not I happen to know the exact microstate of S makes no difference whatsoever to the fact that, relative to the macrostate, of S the chance of an ice cube spontaneously forming is very low. Of course, if I do happen to know the exact microstate of S, I should use that information in order to make predictions and provide explanations. But this does nothing to diminish the objective standing of the chances generated by the Statistical Mechanical Postulate.[36]

1.3 The Nature of the Standard Measure

A second worry about the Statistical Mechanical Chance Postulate stems from a worry about the nature of the standard measure. The Statistical Mechanical Chance Postulate, if it truly is a postulate about chance, i.e. about some *objective* probability, seems to require that the standard measure also be objective. That there must be a single right way of measuring the size of regions of phase space. So it seems that if we are going to rely on the Statistical Mechanical Chance Posulate, we will need to come up with an argument that the standard measure is the *correct* measure, and not just because, for instance, it is the easiest one for us to use.

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[35] Of course we do not often make explicit the specification of macro- or micro-level, but neither do we always make explicit the time of evaluation. Usually both are provided by the linguistic context.

[36] See my paper, "Chance, Indeterminacy, and Explanation" for a more detailed argument.

No mere argument from 'naturalness' will do here. It may be that the standard measure is the measure that is the particularly natural to use with phase space, which is the standard way of parametrizing the possible states of a statistical mechanical system. But there are, of course, other ways of parameterizing the possible states of a statistical mechanical system and a measure that is 'particularly natural' relative to those parametrizations—call it an *alternate measure*—may yield chances that differ significantly from associated observed frequencies.[37]

This worry about the Statistical Mechanical Chance Postulate is similar to a worry that is often voiced about the so-called Principle of Indifference, and a careful examination of how the worry applies in the latter case will help illustrate why it misses its mark in the former.

The Principle of Indifference is a rule for assigning credences; it says that in the absence of evidence in favor of one possible outcome over another, distribute your credences equally over all possible outcomes. The often stated problem with the principle is that there is no single way of carving up the space of possibilities, and different ways of individuating possibilities will lead to different credence-assignments. Suppose that you know only this much about a certain box-factory: that the factory produces boxes that have a side-length of 1m or less. What should your credence be that the next box produced by the factory will have a side-length greater than .5m? At first glance this question seems to have an obvious answer: distribute your credences evenly over the possible side-lengths that the next box could have, and your credence that the next box will have a side-length greater than or equal to .5m will be .5.

The problem arises when you realize that precisely the same box factory produces boxes with a side-area of 1m^2 or less. What should your credence be that the next box will have a side-

[37] Uffink 2004, section 4.1 describes one such alternate measure that Boltzmann originally considered before settling on the standard measure. On the alternate measure, phase space was parametrized in terms of position and energy, instead of position and momentum.

area greater than or equal to .25m^2? According to the Principle of Indifference, .25. But boxes that have a side-length greater than or equal to.5m just are boxes that have a side-area greater than or equal to .25m^2. So should your credence in the next box belonging to that group be .5 or .25? At best the Principle of Indifference seems unable to answer this question; at worst it has led us into contradiction.

It is easy to imagine a similar objection to the Statistical Mechanical Chance Postulate: Using the standard measure, the postulate tells us that the chance of an ice cube spontaneously forming is high, but using some alternate measure, the postulate tells us that the chance of an ice cube spontaneously forming is very low. So should our credence in the ice cube spontaneously forming be very low or very high? At best the Statistical Mechanical Chance Postulate seems to unable to answer this question; at worst it has led us into contradiction.

But there is a very basic disanalogy between these two cases. Since the Principle of Indifference only applies when you have no evidence in favor of one possible outcome or another, it precludes the very possibility of gathering evidence as to which way of partitioning the space of possibilities is the right way. The Statistical Mechanical Chance Postulate faces no such restriction.

What it would be for a measure to be the objectively correct measure to use in the Statistical Mechanical Chance Postulate? Just the following: that measure would generate chances that allowed us to make predictions that were, on the whole, accurate, and underwrite explanations for what would otherwise be surprising events. How do we know we should use the standard measure and not the alternate measure to fix the statistical mechanical chances? Because, among many other things, using the standard measure yields the result that the chance of an ice cube spontaneously forming in a glass of water is very low, and in fact we very rarely see ice cubes spontaneously form in glasses of water.[38]

The evidence we have in favor of the standard measure being the objectively correct measure is, of course, defeasible. There is always some chance that the relative frequency with which some outcome occurs will differ significantly from the chance of that outcome occurring, and in the case of non-fundamental chance this seems to be a particular worry. After all, it might be that the underlying dynamics is actually designed to produce outcomes with frequencies that differ significantly from the macro-level chances. It might be, for instance, that the correct measure is the alternate measure, on which the normal microstates within the macrostate of S are actually tiny and scattered, but it turns out that the dynamics that produces suitably isolated glasses of water is designed precisely to produce glasses in normal microstates.

In this case the underlying dynamics is actually an *undermining dynamics.* An undermining dynamics produces results that are surprising given the objectively correct measure by producing suitably isolated systems in a way that regularly undermines the chances in the Statistical Mechanical Postulate. And if an undermining dynamics is in place, the evidence we will have about what is the correct measure is likely to be compromised.

In the case of statistical mechanics however, we have excellent evidence for the absence of an undermining dynamics. We can take apart our faucets and examine our glass-making processes. We can examine the biological and mechanical processes that result in an oak tree growing in the yard. These processes are extremely coarse, relative to the parameters that determine

[38] Actually, the evidence we have from observing everyday macroscopic things like glasses of water actually only gives us evidence that the right measure is within a certain group, which includes the standard measure, and every measure that is absolutely continuous with it. (See Maudlin 2007b.) This is no more problematic than saying that we are not sure of the exact value of a certain fundamental constant, but we are sure that it falls within a certain range. Also, the measure will not provide the right retrodictions unless we also assume that the universe started off in a certain sort of low-entropy state. See Albert 2000 for more on this assumption.

whether or not an ice cube with spontaneously form or the tree will begin to shrink—they produce systems whose microstates vary widely and unsystematically. Take a particular molecule in a glass of room-temperature water. If an ice cube is going to spontaneously form in the glass, that particle has to be in a very particular position, and have a very particular velocity, relative to all of the other particles in the system. But the processes that bring about glass of room-temperature water are such that the position and velocity of that particular particle varies widely relative to the other particles in the glass.

Taken together, this lack of evidence for an undermining dynamics, and the fact that assuming that the standard measure is the objectively correct measure, and using it in the Statistical Mechanical Chance Postulate, yields predictions and explanations that are both accurate and useful, amounts to a great deal of evidence for thinking that the standard measure is the objectively correct way to measure sizes of regions of phase space. It is possible, of course, that that evidence is misleading, but the same could be said about virtually any supposedly objective entity postulated in the course of scientific theorizing. We assume that there is a concrete world around us, for instance, because that assumption allows us to make accurate predictions about, and provide explanations for otherwise surprising patterns in, the phenomena. The success of those predictions and explanations counts as evidence in favor of our assumption. Of course all of that evidence might be misleading. But that is a worry we can set to one side. Similarly it is possible that all of the evidence we have in favor of the standard measure being the objectively correct measure may be misleading. It may be that, through a stroke of enormously bad luck, all of the glasses of water we have ever encountered have been in normal microstates even though, roughly speaking, most of the ways of being a glass of water are abnormal. But that too is a worry that we can set to one side.

2 The Explanatory Role

Look again at (1) and (2).

(1) We never observe ice cubes spontaneously forming in glasses of room-temperature water.

(2) The chance of an ice cube forming in a glass of room-temperature water, though non-zero, is extremely low.

We now have some sense of what (2) means, and why, according to statistical mechanics, (2) is true. Now for the question of whether (2) explains (1).

The explanatory relationship between chance and frequency is clearest in the case of fundamental chances, where some fact about the chances is often the *only* possible explanation for an observed frequency. It is possible, for example, for an ice cube to quantum mechanically tunnel out of the nearest freezer and appear in a glass of water, yet we never see such things happen. Why not? Presumably because the chance of them happening is very low. Anyone who is unsatisfied with this explanation is going to have to say that there is *no explanation at all* for the fact that we don't ever see ice cubes quantum-mechanically tunnel out of freezers and into glasses of water. And anyone who takes that position will have a significant amount of work to do in explaining why we ought to expect that pattern to continue.

In the case of non-fundamental chances, we cannot avail ourselves of the same argument because there are in fact alternative possible explanations. But the above argument will prove important in a number of different ways. First, it allows us to rule out one group of theories about explanation. Explanation cannot require deductive entailment. Given the above argument, at least some times event e_1 explains event e_2, even though it is not the case that e_1 (combined with some laws of nature) deductively entails e_1.

Second, it emphasizes the similarity between explanations that appeal to chance and the modal explanations like the ones we saw at the beginning of the paper. In some sense these are fairly weak explanations—they do not give very much insight into the actual, detailed causal processes that brought about some set of phenomena. But in one sense, at least, they are a particularly strong type of explanation—they convey, very clearly, what sort of further data we should expect, and what sort of further data we should find surprising, in similar situations. To the extent that the goal of explanation is understanding, this sort of information certainly seems explanatory.

By this criteria, (2) also looks like a successful explanation. Understood as a claim about the structure of the space of possibilities that are compatible with there being a glass of water left at room-temperature, (2) tells us what sorts of further data we should expect and what would be surprising. Roughly: most of the ways of being a glass of water are such that they guarantee that no ice cube will form. This fact, combined with the further evidence we have that no *undermining dynamics* is in place, gives us good reason to expect that ice cubes will not spontaneously form in any glasses of water that we encounter or gain evidence about in the future.

It will help here to distinguish between two different claims we might be making about the explanatory role of (2). On the one hand, we might be making the stronger claim that (2) is *the best* explanation of (1). On the other hand, we might be making the weaker claim that (2) is *an* explanation of (1), whether or not it is the best explanation.

This distinction is important because coming up with a complete theory about what makes one explanation better than another is not only complicated, it may turn out that what makes one explanation better than another depends on the context. There may be multiple criteria by which we evaluate explanations, and they may even conflict, so that we assign a greater value to different criteria depending on the specific context in which we are operating.[39] In that case the stronger claim can only be proved relative to one context or another, but the weaker claim may still hold in general. And the weaker claim is all that we need to establish here.

2.1 Problems for Micro-Explanation

Call (2) the *macro-explanation* of (1). The standard *micro-explanation* given for (1)—the standard *alternative* to (2)—is:

(3) 61Every glass of water that has ever been observed has started off in a normal microstate.[40]

Where (3) is a particularly simple form of writing down the following detailed information about the causal processes that brought about the pattern observed in (1): Given a list of all the glasses of water that have ever been observed, G_1, G_2, … , G_N; G_1 started off in microstate m1 and the

[39] Indeed the considerations I gave above would seem to suggest just this sort of situation. The thought that the level of detailed information about the actual causal process is a context-dependent criteria by which we evaluate explanations, is discussed in more detail Jackson and Pettit 1992. It is for this reason that Sober 2010 rejects the idea that there is any important connection between chance and explanation, though it is not clear why the position I take—that even if there is no single best explanation there are still objective facts about which propositions are explanations—is unavailable to him.

[40] This is the *standard* micro-explanation at least in the literature on explanation. See Woodward 2005, Strevens 2008b, Weslake 2010.

fundamental laws entailed that m_1 evolved over time into n_1, and n_1 realized a macrostate in which no ice cube has spontaneously formed; G_2 started off in microstate m_2 and the fundamental laws entailed that m_2 evolved over time into n_2, and n_2 realized a macrostate in which no ice cube had spontaneously formed; .... and G_N started off in microstate m_N and the fundamental laws entailed that m_N evolved over time into n_N, and n_N realized a macrostate in which no ice cube had spontaneously formed.

In each instance where there has been a glass of water, then, we have identified an earlier state of the system which, combined with the laws, deductively *entailed* that no ice cube would spontaneously form. But notice how we have achieved this level of entailment: by positing an explanans that is just as surprising as the explanandum. The standard micro-explanation itself, far from helping us to understand *why* (1) is true, seems itself to demand some further story about why $G_1, G_2, \ldots, G_N$ started off in the microstates they did.

Here is a way of making the worry precise: For all (3) says, it could have been mere coincidence that $G_1, G_2, \ldots, G_N$ started off in exactly the microstates that they did. And given that (3) leaves it open that (1) is the result of that sort of coincidence, it gives us *no reason at all* for expecting (1) to continue. Maybe we should even expect the pattern observed in (1) to cease immediately. But, as with the fundamental-level explanations described above, it seems that part of what we are looking for when we are looking for an explanation of (1) is an explanation of not only why we have observed a certain pattern so far, but also why we expect that pattern to continue.

The options for the advocate of micro-explanation here are limited. She cannot, for instance, get off the hook by claiming that maybe we should not in fact expect the pattern in (1) to continue. After all, think about a comparison between the macro-explanation of (1) and the standard micro-explanation made 1 year ago. (In such a case, the standard micro-explanation would presumably only include the glasses of water that had been observed up until that time.) At that point the explanation given by (2) allowed us to predict that over the course of the next year, we would continue to fail to observe ice cubes spontaneously appearing in glasses of ice water. The explanation given by the analogue of (3) did not. So the observations we have made over the past year seem to provide some sort of evidence for the explanation provided by (2).

Nor does the advocate of micro-explanation gain much by adopting (4) instead:

(4) Every glass of water that has ever been (or will be) observed began (or will begin) in a normal microstate.

Unlike (3), (4) does explain why we expect the pattern observed in (1) to continue. But what sort of evidence do we have for thinking that (4) is true? If they are going to stick to the spirit of the micro-explanation, advocates of (4) cannot begin appealing to, for instance, the fact that glasses of water are likely to start off in a normal microstate, or the fact that most of the way of realizing a glass of water are normal microstate. And without any further argument of this kind, (4) is an extraordinarily surprising claim to make.

The most plausible move for the advocate of micro-explanation to make at this point is to retreat to (5).

(5) The universe as a whole started in microstate m, and m, combined with the laws of nature, entails that every glass of water that has ever been (or will be) observed began (or will begin) in a normal microstate.

(5) is an improvement over (4) because (4) is unquestionably surprising—when there are patterns like the one described in (4), we expect there to be some explanation as to why those patterns exist; in the absence of any such explanation it takes a significant amount of evidence to convince us that such patterns exist. (5), on the other hand, is a fact about the initial condition of the universe. And perhaps the initial conditions of the universe are precisely the sort of thing for which no explanation can be plausibly given—they are always, in some sense, surprising.[41]

Even so, notice how far we have come from the standard micro-explanation. The advocate of micro-explanation is now giving the exact same explanation for all sorts of disparate phenomena. Why don't oak trees shrink back up into acorns? Because the universe started in microstate m. Why don't large volumes of air force themselves back into deflated balloons? Because the universe started in microstate m. In insisting on providing a complete and fully fundamental explanation we have stripped the notion of explanation of any practical purpose, and this is problematic. It certainly appears that at least part of what makes for a good explanation involves pragmatic criteria—how useful the explanation proves to be to us—if only in the following minimal sense: Explanations purport to portray what is occurring in the actual world with some degree of accuracy, and the only way to tell whether or not an explanation is succeeding on those grounds is to make use of it—to assume that explanation is true, use it to make further predictions, and see whether those predictions turn out to be true.

Perhaps this position is defensible if the advocate of micro-explanation is only arguing the (5) is *in some sense*, or *in some contexts*, a better explanation of (1). (Certainly (5) looks to be the best *fundamental* explanation of (1).) But such a position is entirely compatible with the claim I am trying to establish in this section—that (2) is *an* explanation of (1).

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[41] See Callender 2004. Also Leeds 2003, p. 10.

Here the considerations raised at the beginning of this section are again relevant. The most plausible reason for thinking that (5) is *the only* explanation of (1) is a general claim about explanation, namely that deductive entailment is a necessary condition for something to be an explanation at all. With that view off the table, however this arch-reductionist view is spelled out, it will require significant metaphysical assumptions. Assumptions that are surprising given our ordinary language, our scientific theorizing, and even the rest of our metaphysics.

For instance the arch-reductionist might claim that (2) is not an explanation because explanations must be causal and the only *real* causes are causes that operate at the fundamental level.[42] Or perhaps the arch-reductionist might claim that the only things that *really* exist are things at the fundamental level, and no fundamental entities appear in (2). But all of these, I take it, are highly contentious claims. It is up to the person who thinks that (5) is the only explanation of (1) to provide a convincing argument for them.

3 Non-fundamental Chance Generalized

In statistical mechanics, at least, it is possible to identify objective features of the world that make claims like (2) true:

(2) The chance of an ice cube forming in a glass of water left at room-temperature, though non-zero, is extremely low.

[42] Without this explicit restriction of causation to the fundamental level, (2) can very well be a causal explanation.

These objective features are measures over relevant alternative possibilities; (2) is true because, roughly speaking, most (though not quite all) of the ways of being a glass of water at room-temperature are such that they guarantee that no ice cube will form in the glass. We gain evidence for these objective features by observing the relative frequency with which ice cubes form in glasses of water left at room-temperature, and by examining the processes by which glasses of water come into existence as suitably isolated systems to make sure that those processes are not designed to produce systems in microstates that lead directly to the observed relative frequency.

This specific example generates the following schema for identifying non-fundamental chance, in general.

First, begin with a system that can (i) be described at multiple levels, in terms of fine-grained, *underlying states*, and in terms of coarse-grained *encompassing states* that partition the underlying states, and (ii) in which there are both clear dynamical laws that govern the evolution of the underlying states, as well as robust patterns in the instantiation of the encompassing states that appear to be good candidates for probabilistic explanation.

Second, put a measure over the possible initial underlying state descriptions and interpret the measure in the following way: the probability that a system in some encompassing state S began in an underlying state with feature F is the normalized measure of microstates compatible with S that have feature F.

Third, look for evidence of an undermining dynamics, that is, evidence that the processes that bring about encompassing states tend to bring them about in very specific types of underlying states.

Finally, if there is no evidence of an undermining dynamics, and the probabilities generated by the measure accurately predict and usefully explain observed frequencies then you have evidence that the measure is an objective measure. Given such evidence, you may adopt the following postulate and take the probabilistic explanations at the encompassing level to be genuine explanations.

> *The Non-fundamental Chance Postulate*
>
> Given that it is in encompassing state S, the chance that a certain system is in underlying state with feature F is the normalized standard measure of microstates compatible with S that have feature F.

To see how this works, consider again the roulette wheel case described at the beginning of the paper. In that case, you could describe the system in terms of more fine-grained underlying states—the exact position and momentum of the ball relative to the wheel at the beginning of each spin—and more coarse-grained encompassing states—the number that the ball landed on. The exact relative position and momentum of the ball at the beginning of each spin determine the exact relative position of the ball at the end of the spin,[43] but there also appear to be robust patterns at the encompassing level—in particular, the fact that you never saw the ball land on the same number twice in a row was plausibly explained by the fact that such an outcome was extremely unlikely.

The next step is to put a measure over the possible initial underlying states—the possible relative positions and momenta of the ball at the beginning of the spin. A natural suggestion is the following: divide the possible underlying states into groups that correspond to which number

[43] I am making this assumption for the sake of simplicity. If the underlying states in the roulette wheel are indeterministic, then we will need to understand the application of the Non-fundamental Chance Postulate slightly differently—the relevant features of underlying states that we will be interested in are not, for instance 'being certain to land on number 37', but rather, for instance 'being very likely to land on number 37.'

the ball lands on (i.e. that correspond to the coarse-grained encompassing states), and choose a measure that assigns equal size to each of these groups. Interpreted as a probability measure, this means that on each spin, the probability of the ball landing on each number is equal.

There is no evidence of any undermining dynamics in this case. The wheel and ball are set in motion by humans, and given how sensitive the wheel is to the relative initial position and momentum of the ball, and some general facts about human physiology, most humans cannot reliably "throw" specific numbers. Moreover this probability measure yields accurate predictions (it allows us to predict for instance, that over a very long run of spins, the frequency with which the ball lands between 1 and 15 will be about the same as the frequency with which the ball lands between 15 and 30), and useful explanations (it explains why, for instance, you never saw the ball land on the same number twice in a row—the probability of that happening was very low). Therefore, given the schema above, we can adopt the Non-fundamental Chance Postulate for the roulette wheel case.

The same proves true when we apply the schema to the probabilities that arise in other special sciences. Take, for instance, one way in which probabilities purport to play an explanatory role in evolutionary biology: After a long period of drought, the population of finches on a certain island comes to contain more finches with large beaks than it had previously. Why? Because during a drought competition for food becomes more significant, and large, tough seeds become an important food source. Beak size varies among the finch population, and finches with larger beaks have an easier time breaking open large, tough seeds. It is of course possible for small-beaked finches to survive to reproductive age—if it is born at just the right time and in just the right spot with just the right characteristics so that it makes just the right decisions about where to look for food. Nonetheless, given the general conditions in place, large-beaked finches are more likely to survive to reproductive age.

Here again, this is a system that can be described in terms of underlying states—complete specifications of the conditions under which a bird is born on the island during the drought, including the time and place of the birth, the genetic make-up of the bird, the distribution of food on the island, the weather conditions, etc.—and in terms of encompassing states—whether or not the bird has a certain phenotype, e.g. a relatively large beak. The underlying state determines whether or not the finch will survive, but there also appear to be robust patterns in the instantiation of encompassing states (e.g. more large-beaked birds survive to reproductive age) that are candidates for probabilistic explanation.[44]

Next, the schema directs us to put a measure over the possible underlying states. Here the natural choice is a measure that works in the following way: the size that the measure assigns to the region that includes all ways of being a small-beaked finch that survives to reproductive age is much smaller than the size that the measure assigns to the region that includes all ways of being a large-beaked finch that survives to reproductive age.[45] Interpreted as a probability distribution, the measure generates predictions that are accurate and explanations that are useful. And since there is no evidence for an undermining dynamics, it seems that in this case as well, we should adopt the Non-fundamental Chance Postulate and take the probabilistic explanations at the encompassing level seriously.

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[44] Once again here, I make the assumption that the underlying laws are deterministic for the sake of simplicity.

[45] Notice that, in these cases, we have very little insight into what the correct measure looks like, other than the results that it gives. But this is just the problem of the alternate measure that was described in section 2, and can be responded to in a similar way.

4 Conclusion

Here, then, is the picture we are left with: Chances do not only arise at the fundamental level. Anytime there is evidence for an objective measure over underlying states that predicts and explains frequencies at the encompassing level, we have evidence for the existence of some non-fundamental chance.

One question that arises, then, is how to understand the relationship between non-fundamental chance and fundamental chance. Chances that arise at the fundamental level cannot be interpreted as measures over some underlying states—it is precisely because there are no such underlying states that a level counts as the fundamental level. So it may be that this interpretation of non-fundamental chance forces us to give up the hope of a unified theory of chance. But whether or not it in fact has this consequence depends on which interpretation of quantum mechanics you end up adopting—if you are willing to endorse Bohmian Mechanics, for instance, you can tell a completely unified theory of chance. And in any case it seems that a non-unified theory of chance is a small price to pay in order to allow non-fundamental objective probabilities to play the explanatory role that they appear to in our best scientific theories and our everyday reasoning about the world.

3

Counterfactuals and Chance: Everything but the Truth

This morning I was in too much of a rush to make my customary mug of coffee. But suppose that I had not been in such a rush. And suppose that, while standing in my kitchen, I had dropped my mug of coffee. It seems obvious that (1) is true.

(1) If I had dropped a mug full of coffee while standing in my kitchen this morning, coffee would have spilled all over the floor.

In fact, it seems so obvious that (1) is true that you might take it as a sort of prerequisite—any serious account of the truth conditions for claims like (1) must capture the initial judgement that (1) is true.

In this paper I will argue against taking the truth of (1) as that sort of prerequisite. (1), I will argue, is not true. In fact, most counterfactuals—most claims of the form *if A had happened then C would have happened*—are not true. This is a surprising claim, but not as surprising as it might first appear because it comes hand in hand with very good explanation for both why we are making this systematic error, and why claims like (1) continue to be useful even though they are not true. This explanation appeals to the existence of certain surprising possibilities—extremely strange situations that are licensed by our best scientific theories.

That these possibilities exist, and that they have consequences for our semantics of counterfactuals, has been long been recognized by philosophers—David Lewis attempted to address it in his paper 'Counterfactual Dependence and Time's Arrow' in 1979—and recent papers on the topic include Hawthorne (2005), Williams (2008), and Alan Hajek (MS). My view differs from these philosophers in two important ways. First, these philosophers have been particularly concerned with what I will call the Conceptual Connection Problem. This problem arises from the fact that these surprising possibilities apparently underwrite the truth of claims like (M),

(M) If I had dropped a mug full of coffee while standing in my kitchen this morning coffee might not have spilled all over the floor.

combined with the fact that there seems to be a conceptual connection between 'might' and 'would' such that both (1) and (M) cannot be true (or, perhaps, cannot both be asserted). At the end of this paper I will propose a solution to the Conceptual Connection Problem, but for the most part I will not be concerned with it here. I think the problems for counterfactuals run deeper than just the apparent conflict between (1) and (M). The surprising possibilities that I will discuss in this paper are such that even when we set aside our immediate judgements and work instead with our best fully-developed sesmantics for counterfactuals, most counterfactuals are false.

Second, the solution that I will propose in the later sections of the paper is novel in its connection to a particular theory of chance. Even when other philosophers have suggested that, as a last resort, we might endorse the sort of error theory that I have in mind, they seem not to notice that such an error theory will only work if chances are very specific sorts of entities. The argument in this paper, therefore, serve as an argument for my favored theory of chance. Once we adopt that theory we are able to take the following position: Although the surprising scientific possibilities undermine the truth of counterfactuals like (1), because the chance of those possibilities actually happening is very low, counterfactuals can still play an important role in our reasoning, deliberation, and communication. We are still warranted in asserting counterfactuals, they just aren't true.

1 Counterfactuals and the Standard Semantics

Counterfactuals are pervasive in ordinary discourse, and for very good reason. Although counterfactuals are ostensibly about something that did not happen, they also carry information about what things are actually like; what would have happened if things had been different often tells us something about the way that things actually are. That I would have been late to class if I had slept through my alarm tells you that I did not have much extra time built into my schedule this morning between waking up and arriving at class. That McCain would have won the election if Obama had not, tells you that McCain was the runner-up. That my dog, Charlie, would not have run off if he had not seen a squirrel tells you something about what sort of dog he is—that he is generally interested in being well behaved, although those good intentions can be trumped by extremely exciting things, like squirrels.

This sort of information about the actual world is important because it allows us to make predictions about and provide explanations for actual sequences of events. That I would have been late to class if I had slept through my alarm this morning allows me to predict that if my alarm fails to go off next Monday, I am going to be late. That McCain would have won the election if Obama had not explains why some people who did not agree with all of McCain's views still endorsed him as a candidate—he was the best hope they had of defeating Obama.[46]

In addition to playing this role in prediction and explanation, counterfactuals also play an important role in a number of philosophical analyses. Perhaps the most famous example is David Lewis's analysis of causation: According to Lewis, the rock caused the window to break just in case, if the rock had not been thrown the window would not have broken.[47] Similar analyses have been proposed for normative concepts including obligation, regret, and blame: I ought not have done what I did (or I ought to regret what I did, or I ought to be blamed for doing what I did) just in case, if I had done something different, the consequences would have been better. Other examples of philosophical analyses that rely on counterfactuals include common analyses of what it is for something to be a law, what it is for something to have a disposition, and what it is for someone to know something.

Given the importance of counterfactuals it would be helpful to have *semantics* for counterfactuals—a clear understanding of the conditions under which such claims are true. The most common semantics was developed independently by David Lewis and Robert Stalnaker:[48]

[46] There is of course an enormous amount of background information that we're holding fixed, which allows us to make these predictions and explanations, and not all of this background information is part of the content of the sentence. That does not, I take it, threaten the point.

[47] See Lewis 1973a. There are a cluster of objections to Lewis's analysis centered around pre-emption cases, but many philosophers think that it still provides at least a sufficient condition. See Hall 2004 for a discussion.

[48] See Lewis 1973b and Stalnaker 1984.

The Standard Semantics

> A counterfactual of the form *if A had happened then C would have happened* is true just in case, in all of the closest possible worlds in which A is true, C is true.[49]

The standard semantics is supposed to capture the fact that the truth of a counterfactual depends on what could have happened (as opposed to what actually happened), while also allowing that not everything that could have happened is relevant. It strikes us as obvious that if Obama had not won the election, McCain would have, even as we acknowledge that it was possible for some other third party candidate to have won. The idea that Stalnaker and Lewis are trying to capture with their talk about the closeness of possible worlds is that the possibility of Nader winning the election, for instance, is not relevant because it would have taken so much more than just Obama losing in order for Nader to have won. People's preferences across the entire country would have to have been dramatically rearranged in order for Nader to have won. And so Nader's winning is a *distant* possibility in the sense that things would have had to be *very different* than they actually are.

Of course, should we want to apply the semantics to more difficult cases, we will need a more detailed account of what determines closeness, and on this point Lewis and Stalnaker differ. Whereas Stalnaker says only that closeness is determined by the linguistic context in which the counterfactual is uttered,[50] according to Lewis closeness is determined by *similarity*, where similar-

[49] The standard theory is not, of course, universally accepted. See Bennett 2003 for an overview. One recent serious objection concerning so-called Sobel sequences found in von Fintel 2001 and Gillies 2007. Moss 2007 contains both a brief overview of the objection and a reply.

[50] More carefully, according to Stalnaker, (1) is true if the context in which (1) is uttered fixes a closeness relation such that there is a unique closest world in which I drop the mug and the coffee spills in that world (or a unique set of closest worlds in all of which I drop the mug). See Stalnaker 1984, Chapter 7.

ity between worlds is a technical notion involving multiple criteria clearly distinguished and placed in order of importance.[51]

In what follows I will often, at least as a first pass, take similarity as a guide to closeness. The thought here is not only that it certainly seems like, at least in normal contexts, we're using a relatively intuitive notion of similarity in order to determine which worlds are closest (hence the natural slide between 'distant' and 'difference' above). In addition, the very possibility of counterfactuals conveying the sort of information about the actual world that we noted above—the very possibility of these conditionals helping us make predictions about, and given explanations for, *actual* events—seems to require that we restrict our attention to worlds that are sufficiently like ours when evaluating counterfactuals.

But it will turn out that the details of how we spell out the relevant notion of similarity will not matter; advocates of the standard semantics for counterfactuals are going to run into problems on any plausible account. These problems, and my preferred way of solving them, will form the topic of the rest of this paper.

2 The Original Argument and Two Problems for the Standard Semantics

Let's return to (1).

(1) If I had dropped a mug full of coffee while standing my kitchen this morning, coffee would have spilled all over the floor.

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[51] See Lewis 1973b, sections 1 and 3.4, and Lewis 1986.

It seems obviously true that if I had dropped a mug full of coffee while standing in my kitchen this morning, coffee would have spilled on the floor.

Now consider the following three ways the world could be:

SPILL

The world that is as similar to the actual world as possible except for the fact that I dropped a mug this morning and coffee spilled all over the floor.

BOUNCE

The world that is as similar to the actual world as possible except for the fact that I dropped a mug this morning and it bounced back up into my hand.

TUNNEL

The world that is as similar to the actual world as possible except for the fact that I dropped a mug this morning and it tunneled through the floor.

According to the standard semantics, if (1) is true then all of the closest possible worlds in which I dropped a mug in the kitchen this morning are worlds in which the coffee spills. So, according to the standard semantics, it is necessary condition on (1) being true that SPILL is closer to the actual world than either BOUNCE or TUNNEL.

More generally, we can call worlds in which coffee spills, like SPILL, *normal*, and worlds in which there are unexpected results, like BOUNCE OR TUNNEL, *abnormal.* Given the standard semantics, in order for (1) to be true, we need to identify some reason for thinking that SPILL is closer to the actual world than any abnormal world.

Perhaps the most obvious reason for thinking that SPILL is closer to the actual world than either BOUNCE or TUNNEL is the following:

> *The Original Argument*
>
> The only way you get unexpected results like mugs bouncing or tunneling is if there is a *miracle*—a violation of the actual laws of nature.[52] The existence of miracles in a world detracts from the closeness of that world to the actual world. Therefore, since BOUNCE and TUNNEL require miracles that aren't required in SPILL, SPILL is closer.

At first glance, the Original Argument has a lot going for it. It not only identifies a feature of worlds that plays a role in determining how close they are to the actual world, it is immediately obvious why that feature would be relevant. What happened in some other possible world will not have much bearing on what will happen in the actual world (or help us explain events in the actual world) if that possible world includes violations of the laws of nature.

Of course the argument also relies on the assumption that either (i) we have no other reason for thinking SPILL is farther from the actual world than BOUNCE and TUNNEL, or (ii) that avoiding miracles is so important that the existence of a miracle in BOUNCE and TUNNEL trumps any other reason for thinking that those worlds are closer than SPILL. As a result the Original Argument leads us very quickly toward further commitments about what makes one world closer than another.

[52] This terminology is Lewis's.

For instance, SPILL, as the closest world in which the mug spills, is going to include a number of events which both detract from its similarity to the actual world (and thus potentially detract from its closeness to the actual world) and which do not occur in BOUNCE and TUNNEL. These are events which happen after I drop the mug: my running around looking for a dishtowel, my being late to catch my train, there being a broken mug in my trash can, and so on. [53]At the very least, advocates of the Original Argument will need to tell a story about how the similarity ordering on possible worlds (or the linguistic context in which (1) is uttered) makes these events irrelevant.[54]

But we need not worry about the details of this story. As it turns out, the Original Argument fails for a different and more decisive reason. It fails because, according to our best scientific theories, the first premise of the Original Argument is false. Unexpected results like those seen in BOUNCE and TUNNEL do not, in fact, require miracles.

When examined closely, our best scientific theories give us not one but two different reasons for thinking that BOUNCE and TUNNEL need not contain miracles. The first problem, which I will call the *Genuine Indeterminacy Problem,* arises from the fact that we may live in a world where the fundamental laws are indeterministic. If we do it is possible for the mug to do strange things like tunnel through the floor, without any violation of the laws of nature. And, moreover, mugs can do these strange things even if the exact initial arrangement of particles—the complete specification of the position and velocity of each of the particles in the system comprising my hand, the mug, the air around the mug, the floor, etc.—is the same as the exact initial arrange-

[53] See Fine 1975 for more on this point, stated as an objection to the use of similarity as a semantic criterion.

[54] Much of the debate over the standard semantics has revolved around the way that this story is told. Some philosophers, like Jonathan Bennett (2003), are willing to build a temporal asymmetry into the semantics for counterfactuals and simply assert that similarity before the time at which the antecedent would have occurred matters more than similarity after that time. Philosophers like Lewis and Barry Loewer (2007) who hoped to use counterfactuals to explain or underwrite temporal asymmetries, have tried to find a different route.

ment in a world where the coffee spills. In other words, if we live in a genuinely indeterministic world, there is a world in which the mug tunnels that is exactly the same as SPILL up until the point at which the mug is dropped[55] and which does not include any miracles. And so TUNNEL, as the closest world in which the mug tunnels, will be at least as close as SPILL.[56]

The physicists have yet to determine whether we live in a world where the fundamental laws are indeterministic, but even if we do not, the Original Argument faces as second problem. This second problem, which I will call *The Underspecification Problem*, arises from the fact that the antecedents of counterfactuals like (1) are radically underspecified. There are an enormous number of ways of arranging all of the particles in the mug, my hand, the air, and the floor that are compatible with my dropping the mug; and different ways of arranging those particles lead to very different outcomes.[57] It might be, for instance, that the particles are all arranged such that the mug is *certain* to bounce—the complete specification of the positions and velocities of all of the particles in the system when the mug is dropped, combined with the laws of nature, entail that the mug will bounce.

Now these non-miraculous worlds in which the mug bounces will require a very special initial arrangement of the particles—there must be very many air molecules crowded into the space between my mug and the floor; all of these must have a significant upward velocity, so that as the mug falls, it collides with these molecules and slows; and the particles in the floor must be

[55] Whether or not we live in a world that is genuinely indeterministic depends on which interpretation of quantum mechanics turns out to be true. It is worth noting that the best contender for a deterministic interpretation of quantum mechanics—Bohmian mechanics—would actually give rise to an even more fundamental version of the Underspecification Problem than the one described above.

[56] TUNNEL is either the world that is exactly the same as SPILL up until the point at which the mug is dropped, and in which the mug tunnels without any miracle, or it is some closer world. Even if it is the former it will be at least as close as SPILL, since the only differences between TUNNEL and SPILL will be differences in what happens after the mug is dropped, and these are likely to count against SPILL for the reasons described above.

[57] Again, where arranging the particles involves not only giving them each a position, but also a velocity.

arranged in such away that that they are vibrating and, at just the right moment, they give the mug an upward push that carries it back into my hand. And, of course, there is every reason to think that the way that the particles in the system are arranged when I drop the mug in some world is a factor in determining how close that world is to the actual world. But—and this is the crucial point—there is no reason to think that the initial arrangement required to get the mug to bounce is any *less* similar to the actual initial arrangement than is the initial arrangement required to get the mug to spill. After all, the differences between worlds where the mug bounces (deterministically) and those where the mug spills, are extremely fine-grained; they require a specification of the microscopic arrangement of all of the particles in the system. Compare these fine-grained differences to the enormous differences between either of those types of worlds, on the one hand, and the actual world, on the other hand. In the actual world there is no coffee, the mug is still in the cupboard, I was never standing in the kitchen, and so on. BOUNCE, therefore, as the closest world in which the mug bounces, does not contain a miracle. And there is no longer any reason for thinking that it is not at least as close to the actual world as SPILL.

So the Original Argument fails. Despite the seemingly miraculous nature of worlds like BOUNCE and TUNNEL, such worlds do not require violations of the actual laws of nature. Unless we can come up with some other reason for thinking that SPILL is closer to the actual world than any abnormal world, the standard semantics appears to yield the result that (1) is not true.[58]

This result becomes much more serious when we note that the reasons for thinking that (1) is not true are entirely general. The phenomena that undermine the Original Argument—un-

[58] For more on both of these problems see Hawthorne 2005, Williams 2008, and Hajek MS. As noted above, all three of these authors are concerned first and foremost with the Conceptual Connection Problem. I will propose a solution to this problem in the last section.

derspecification and genuine indeterminacy—apply to virtually all physical processes, not just the dropping of everyday objects like coffee mugs. If we admit that (1) is not true then we'll have to admit the same for virtually all counterfactuals that we might have hoped to make use of, whether in practical reasoning or in philosophical analysis.[59]

I take it that we can all agree that this is a surprising and troubling result. Given how important counterfactuals are we would like to be able to continue to use them without having to wait to see how fundamental physics turns out, and without have to fully specify their antecedents. We would like to "save" counterfactuals. And so a natural response to the worry above is to try to make some change or addition to the standard semantics so that (1) turns out to be true after all.

Attempts to fix the semantics in this manner will be discussed in the next section, but before we begin it will pay to get clear on the ultimate goal. The game of trying to save counterfactuals by fixing the semantics is an easy one to play—clever readers will undoubtedly be able to think of suggestions other than the ones I will consider here. In addition, the reasons I give for setting aside each of the proposals considered below are by no means decisive. My goal is only to convince the reader that there is no obvious way to fix the standard semantics in order to solve both of the problems listed above, and that any proposal considered below comes with considerable costs. With that in mind, I will argue in section 4 that there is in fact a much easier way to save counterfactuals.

[59] The main exception here is counterfactuals with a fully specified antecedent, assuming that there is no genuine indeterminacy. For a further discussion of possible exceptions see Hajek MS.

3 Attempts to fix the semantics

Attempts to fix the standard semantics in order to save counterfactuals can be divided into two camps. The first camp includes attempts that retain the general form of the standard semantics but build some additional structure into the ordering on possible worlds so that worlds like BOUNCE and TUNNEL are farther away from the actual world than SPILL. The second camp includes attempts that are willing to depart more radically from the standard semantics.

In either case we can identify two desiderata that we would like any such attempt to satisfy. First, any such attempt should address *both* of the problems outlined in the previous section. There is little reason to adopt a new semantics in order to avoid the Underspecification Problem if the Genuine Indeterminacy Problem still threatens the truth of most ordinary counterfactuals. Second, if possible, any such attempt should provide at least some analysis of the terms it introduces. This analysis does not necessarily need to be fully reductive, but it does need to be illuminating.

What this second desiderata means is perhaps most clear if we take an example. One might, for instance, think that the way to fix the standard semantics in order to save counterfactuals, is immediately obvious. The worlds which create problems for the standard semantics are, as we called them above, *abnormal* worlds. So why not just include the following in your account of closeness?

Attempt 1.1

All else being equal, abnormal worlds are farther away than normal worlds.

Certainly this addendum to the standard semantics would solve both the underspecification problem and the genuine indeterminacy problem. But it wouldn't do so in any sort of satisfying or principled way. Recall that we introduced the term "abnormal" to pick out those worlds in which strange things (like mugs bouncing back up into our hands or tunneling through the floor) happen. To simply stipulate that such worlds are irrelevant is unilluminating. If there is some feature of those worlds that makes them farther away then we ought to be able to identify what that feature is, and build an ordering of possible worlds around it. And it is not enough to simply say that such worlds contain events that are surprising to us (which is how we introduced the term "abnormal"). Those events are surprising to us at least in part because we expect (1) to be true.

Here is a related but similarly flawed proposal:

> *Attempt 1.2*
>
> All else being equal, worlds in which the laws of thermodynamics are violated are farther away than worlds in which those laws are not violated.

This attempt goes a step farther than simply stating that abnormal worlds are farther away. It tries to identify some feature that abnormal worlds have in common that accounts for their being father away—such worlds involve violations of certain sorts of laws, in particular the laws of thermodynamics. Although, according to the fundamental laws, it is possible to arrange all of the particles in the mug, my hand, the air, and the floor such that the mug bounces—such arrangements violate the second law of thermodynamics. The problem is, it is not clear how Attempt 1.2 solves the Genuine Indeterminacy Problem. The mug tunneling through the floor need not involve any violation of the laws of thermodyanmics.

It may be that the advocate of Attempt 1.2 had something more general in mind. Perhaps the thought was that worlds in which any special science laws—the laws of chemistry or biology for instance—are violated are farther away than worlds in which those laws are not violated. But again, how does this solve the Genuine Indeterminacy Problem? What law of chemistry or biology is violated when the mug tunnels through the floor?

It seems to me that in order to save her proposal, the advocate of Attempt 1.2 will have to broaden it even further. She will have to say that, in general, there is a class of lawlike generalizations that, when violated in a world, count against the closeness of that world to the actual world, and that one of these lawlike statements is 'solid objects do not tunnel through other solid objects'. But then the question becomes, what picks out these lawlike statements? What distinguishes them from other true generalizations? It cannot be because they are exceptionless—the fundamental laws tell us otherwise. In fact it seems that the relevant sense in which these generalizations are "lawlike" is that they are intimately connected to counterfactuals. At least part of the reason why we think that 'solid objects do not tunnel through other solid objects' is importantly different than, for instance, 'all of the coins in my pocket are silver' is because we think that counterfactuals like 'if I had dropped that mug it would have broken' and 'if I had thrown that ball it would have bounced' are true, while we doubt that if I had another coin in my pocket it would be silver.[60]

A third attempt to fix the semantics picks up on what one might think is at the root of the fact that abnormal worlds are so unexpected—the low probability of their occurrence.

[60] See Strevens 2008b.

Attempt 1.3

> All else being equal, worlds in which unlikely events happen are farther away than worlds in which unlikely events do not happen.

Unfortunately this attempt again falls foul of the first desiderata—it solves only the Genuine Indeterminacy Problem and does nothing to solve the Underspecification Problem. This is because *any* complete specification of the arrangement of particles in the kitchen this morning is extremely unlikely. While it is extremely unlikely that the particles in the kitchen this morning be arranged in precisely they way that they are in BOUNCE, it is also extremely unlikely that the particles in the kitchen this morning be arranged in precisely they way they are in SPILL. So once again there is no reason for thinking that BOUNCE is any farther away than SPILL.

More promising is a related proposal, found in Williams 2008:

Attempt 1.4

> All else being equal, *atypical* worlds are farther away than *typical* worlds, where a world is atypical if some proposition p is true in that world where p is both (i) very unlikely and (ii) simple.

So BOUNCE is atypical because the proposition expressed by (A)

(A) The mug bounces back up into my hand.

is true in BOUNCE, and (A) is both very unlikely and simple. But SPILL is not atypical even though the proposition expressed by (B)

(B) The exact initial arrangement of particles when I drop the mug is α.

is true in SPILL, because although (B) is unlikely, it is not simple. Of course (B) looks simple, but that's just because I'm using a simple name, 'α', to pick out at a very complicated state.

But again here we run up again the second desiderata: 'simple' looks like an unanalyzed primitive, a know-it-when-we-see-it sort of property. And I take it that is the sort of primitive that we would like to avoid in our theory if we possibly can.[61]

Finally, as an example of the second sort of attempt to fix the semantics, consider:

Attempt 2.1

If (1) is true then *most* of the closest possible worlds in which I dropped a mug are worlds in which coffee spilled.

This proposal is inspired by the claim, central to statistical mechanics, that although it is possible that the mug bounces back up into my hand, most of the ways in which the mug is dropped lead to spilling.[62] As for how this proposal is supposed to extend to solve the Genuine Indeterminacy Problem, it is unclear. If certain fundamentally indeterministic interpretations of QM are true

[61] Williams refers to Elga 2004, in which Elga gives a mathematical criteria for determining which sequences of coin flips are simple, and which are not. But it is not obvious how to translate Elga's criteria into criteria for English-language counterfactuals.

[62] That this is an *objective* fact is often disputed in the philosophical literature. See my "Chance, Measure, and Explanation" for a detailed argument.

then for every world in which the coffee spills on the floor, there is a world that is exactly like it except that the mug tunnels through the floor.[63]

Again, remember that the goal of this section is not to provide an exhaustive list of ways one might fix the semantics in order to avoid the problems laid out in section 2 and to provide knock-down arguments against each of these proposals. Instead I hope to have convinced the reader that there is no obvious, non-costly way of fixing the semantics. But that does not, I contend, mean that there is no obvious and non-costly way of saving counterfactuals.

Suppose that instead of attempting to fix the standard semantics we accept that the semantics cannot be fixed—that (1) is not true. Given how pervasive counterfactuals are in our ordinary discourse, this seems like a radical proposal. But not if we can explain why ordinary counterfactuals are still important.

4 An error theory

In order to save counterfactuals while still admitting that (1), and indeed most counterfactuals, are not true, we will need to adopt a non-reductive error theory. According to this theory although (1) is not true there is some nearby proposition that is true, and in virtue of that nearby proposition (1) still counts as assertible.

It will pay to say a little more about what I mean here by "assertible". The point of assertion is to convey information to others in our linguistic community, and the nature of that com-

[63] More carefully, given a genuinely indeterministic theory (e.g. the GRW interpretation of quantum mechanics) there is no reason to think that the measure of worlds in which the coffee spills is any different than the measure of worlds in which the mug bounces.

munity requires that we take into account practical limitations in our ability to both communicate pieces of information to one another, and our ability to put information to good use in deliberation. So we often struggle to balance the competing demands of accuracy—of only asserting true things—and practicality. The most obvious example of this trade-off involves our epistemic justification. I am entirely justified in asserting that Robert is wearing a blue shirt today, even if I have not seen him in the past ten minutes and he may in fact have changed. My evidence—that he was wearing blue ten minutes ago, and that he does not generally change shirts without a reason in the middle of the day—warrants my assertion, even if what I assert turns out to be false.

In the case of counterfactuals we face a similar trade-off. The crucial role that counterfactuals play in allowing us to easily predict and explain what follows from various possible courses of action, including courses of action that we never in fact take, is so enormously helpful that counterfactuals may well be assertible even if they are not strictly speaking true. What is important is that the cases where counterfactuals would lead us astray in our reasoning are enormously unlikely.

In other words, what rescues counterfactuals is the truth of corresponding probabilistic claims. If (2) is true, I claim, (1) is assertible in normal contexts.

(1) If I had dropped a mug full of coffee while standing my kitchen this morning, coffee would have spilled all over the floor.

(2) If I had dropped a mug full of coffee while standing in my kitchen this morning, the chance of coffee spilling all over the floor would have been very high.

And (2) is true—strictly speaking true, true on the standard semantics, and true regardless of which of SPILL, BOUNCE, or TUNNEL is closest to the actual world.

In order to understand why, we need to establish a few claims about chance, particularly about the way in which chances are determined relative to certain features of the context in which they are uttered. In his paper on chance, Lewis makes use of the example of someone attempting to complete a maze before 12:00pm in order to argue that chances are context-relative in at least two ways.[64] First they are determined relative to some particular possible world. If we evaluate the chance of the person completing the maze in a world where it is possible to pass through the walls of the maze, the chance is much higher than the chance of the same result if the walls are impenetrable. Similarly, Lewis argues, chances are determined relative to some time. The chance of the person finishing the maze by 12:00pm might be very different at 11:00pm than it is at 11:59pm. If it is a very difficult maze, the chance of the person finishing by 12:00pm might be very low at 11:00pm, but very high at 11:50pm. (Suppose for instance that by 11:50pm in that world the person was nearly finished and just making her way down the home stretch.)

The time-relative nature of chance is important when we consider the possibility (left open by the discussion above) that the world is genuinely indeterministic and TUNNEL is at least as close to the actual world as SPILL. In that case, we need to establish that in TUNNEL the chance of the mug spilling is very high. And indeed it is, as long as we evaluate that chance relative to the time at which the mug is dropped. (If we instead evaluated it relative to some time after the mug is dropped, then the chance would instead be 0).

But careful consideration of the way chances work in scientific theories also demonstrates that in addition to being world- and time-relative, chances are relative in another, more general

[64] See Lewis 1980.

way. Sometimes when we evaluate chances we evaluate them relative to the microphysical context, that is, we take into account all of the microphysical facts about the relevant world up until the relevant time. And sometimes when we evaluate chances we evaluate them relative to the macrophysical context, evaluating them relative only to the macrophysical facts.[65]

To take a particularly simple example, suppose that I have before me a glass of water with a cube of ice in it. What we expect to happen in this situation is very straightforward: over the course of the next fifteen minutes, we expect the ice cube to melt. But according to our best scientific theories, it is entirely possible that the initial arrangement of particles in the glass and the water and the ice is such that that it leads, deterministically, to the following, very strange situation: instead of melting, the ice cube in front of me might get bigger over the next fifteen minutes, increasing in size while the remaining water around it grows warmer and warmer. Of course we never see ice cubes get bigger while the water around them begins to boil, and our best scientific theories have a straightforward explanation for that fact: the chance of the ice cube getting bigger instead of melting, although non-zero, is extremely low.

Now notice that if chances are always evaluated relative to the microphysical context—if we have to take into account all of the microphysical facts about the relevant world at the relevant time—then in every possible world the chance of the ice melting will be either zero or one. And so statistical mechanics requires that at least in some cases we are permitted to evaluate the chances relative to only the macrophysical facts. Even in a world where the ice cube does in fact get bigger instead of melting, relative to the macrophysical variables like the temperature of the water at the ice, the volume of the glass, etc., the chance of the ice melting is very, very high.

[65] Lewis famously did not endorse this additional level-relativity. He thought that once we had specified a world and a time, the chances were entirely fixed.

To see why all of this is relevant, consider the analogous case for the example of the mug dropping. Suppose that BOUNCE is at least as close to the actual world as SPILL. In order for (2) to be true we need to establish that the chance of the mug spilling in BOUNCE is very high, even though at the relevant time (just before the drop), it is already determined that the mug will bounce. This is true if we evaluate the chance of the mug spilling relative to the macrophysical context.

So it does not matter which of BOUNCE or TUNNEL or SPILL is closest to the actual world. In any case, given the right context, (2) is true.

And, to reiterate, the thought is that the truth of (2) gets us everything we want, and for very little cost. It does not matter that we cannot reinterpret ordinary counterfactuals like (1) to get rid of the underspecification in the antecedent, and it does not matter that the fundamental laws may turn out to be genuinely indeterministic. We can accept both of these scientific claims and, without having to undermine the initial plausibility of the standard semantics, retain the enormously helpful role that ordinary counterfactuals play in practical reasoning. Nearby chancy counterfactuals like (2) allows us to rescue ordinary counterfactuals, even while we allow that strictly-speaking such ordinary counterfactuals are not true.

5 What about truth?

As an advocate of the error theory described above, I have often been asked why I do not go further and endorse a fully probabilistic semantics according to which (2) does more than just make (1) assertible—it provides the truth conditions for (1).

Whether or not you find this probabilistic semantics attractive will depend on how you come down on a number of other questions, including whether or not you think the standard semantics is successful in most other cases and whether or not you think that the philosophical analyses that make use of counterfactuals can be recast in terms of corresponding claims about chance. But it should also depend on whether you think that there is a good explanation for a systematic error in our evaluation of counterfactuals—the sort of explanation that makes it easy to understand why we have been consistently getting things wrong.

In this case, such an explanation is readily to hand. It turns out that our folk physics of ordinary events like someone dropping a mug of coffee turned out to be wrong. We thought, for instance, that solid objects could not tunnel through solid objects; quantum mechanics has taught us otherwise. Nonetheless our folk physics is still a good enough approximation to make it useful for making predictions and providing explanations in our ordinary lives.

And in addition, the error theory helps explain why, in some contexts, counterfactuals that look similar to (1) are not assertible. For instance (3) sounds strange, although (4) is true:

(3) If I had played the lottery, I would have lost.

(4) If I had played the lottery, the chance of losing would have been very high.

If we stick to an error theory then we may be able to explain these cases by appealing to some other norm of assertion. For instance, you might think that when it is contextually salient that you are in exactly the same epistemic position with respect to each one of an exhaustive set of possibilities, you ought not make assertions that rule out any one of those possibilities.[66] The fact that something is called a lottery, generally carries with it exactly this sort of implication—that you are in exactly the same epistemic position with respect to each one of the ways of playing. As a result it sounds strange to assert (4) immediately after (3).

This latter suggestion may also explain the phenomenon that has troubled many philosophers who write on counterfactuals, and which above I labelled the Conceptual Connection Problem: the fact that once we have asserted (5), it sounds strange to then assert (1).

(5) If I had dropped a mug full of coffee while standing in my kitchen this morning, coffee might not have spilled on the floor.

(1) If I had dropped a mug full of coffee while standing in my kitchen this morning, coffee would have spilled on the floor.

This phenomenon is readily explained by the norm of assertion described above. By asserting (5) we have made it contextually salient that we are in exactly the same epistemic position with respect to each possible initial arrangement of particles. And in that situation we ought not make an assertion that rules out any of those initial arrangements of particles, which is exactly what we do when we then assert (1).[67]

[66] An alternative explanation involves the fact that the stakes might make it worth playing the lottery, but no conceivable stakes could make it worth betting that the mug will bounce or tunnel.

[67] See Hawthorne 2005, Williams 2008, and Hajek MS.

For all that has been said above, it may still be hard to let go of feeling that we are losing something when we give up on the truth of most counterfactuals. But I think we should let go. Most counterfactuals are not true, but we can make use of them nonetheless.

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