Three Proposals Regarding a Theory of Chance

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Abstract

I argue that the theory of chance proposed by David Lewis has three problems: (i) it is time asymmetric in a manner incompatible with some of the chance theories of physics, (ii) it is incompatible with statistical mechanical chances, and (iii) the content of Lewis's Principal Principle depends on how admissibility is cashed out, but there is no agreement as to what admissible evidence should be. I proposes two modifications of Lewis's theory which resolve these difficulties. I conclude by tentatively proposing a third modification of Lewis's theory, one which explains many of the common features shared by the chance theories of physics.

1 Introduction

Probability is used to model the actual likelihoods, or *chances*, of various possibilities. Probability is also used to model the degrees of belief, or *credences*, a reasonable subject has in various possibilities. Now it seems there should be a relation between the two: if a subject believes that an event has a given chance of occurring, this should have some bearing on her credence that the event will occur.

The canonical account of this relation is that of David Lewis (1986). Lewis proposes that the correct relation between chance and credence is given by his Principal Principle. Lewis's account is not just an account of the chance-credence relation, however; it is partially a theory of chance as well. (A theory *of* chance, not a chance theory; I take a chance theory to be a physical theory that employs chance, such as statistical mechanics or quantum mechanics.) Worries about the compatibility of the Principal Principle and Humean supervenience have led to variations of Lewis's original principle, but the general nature of these principles and the notion of chance they employ have remained the same. I'll call Lewis's theory and these variants the *Lewisian theories of chance*.

Ceteris paribus it seems your credences should agree with your chances. If you believe that a coin has a .5 chance of coming up heads, then all else being equal you should have a .5 credence that the coin will come up heads. But you don't always want your credences to accord with the chances. Suppose you are in possession of a crystal ball which reliably depicts the future, and the crystal ball shows you that heads will come up. Then your credence in the coin coming up heads should be near 1, not .5. So it seems you should only let the chances guide your credences in an outcome when you're not possession of illicit or *inadmissible* evidence.

With the notion of admissibility in hand, the relation between chance and credence falls into place: your credences should accord with what you think the chances are unless you're in possession of inadmissible evidence. This is Lewis's Principal Principle. (Lewis later abandoned the Principal Principle in favor of a variant.[1] The details of this new principle need not concern us; any points I make assuming Lewis's original principle will apply *mutatis mutandis* to this variant.)

The Lewisian accounts also make assumptions about the objects and arguments of chance. First, the objects. They take the objects of chance to be *de dicto* propositions.[2] Following Lewis, we can take these propositions to be sets of possible worlds, and understand the chance of these propositions as the chance of one of those worlds obtaining.

Second, the arguments. They take chance distributions to be functions of two arguments. The first argument is a set of chance laws, the second is a history up to a time at some world where those chance laws hold. So on the Lewisian accounts every chance distribution is entailed by a set of chance laws and a history.

Both arguments can be picked out by a world and time. The history up to a time is picked out by the world and time. The chance laws are picked out by the world alone. So we can also treat chance distributions as functions of a world and time. In the rest of the paper, when I speak of the 'past', 'future', etc., I'll always be speaking relative to the time associated with a chance distribution. For example, a 'past event' will be an event that occurs at a time before the time associated with the relevant chance distribution.[3]

In his original paper, Lewis made two further claims about chance. The first is that the past is no longer chancy; i.e., that a chance distribution assigns only trivial chances (0 or 1) to events in its past. The second is that determinism and chance are incompatible. Both of these claims can be derived from the Lewisian theories given some additional assumptions. The first requires that the arguments of a chance distribution are admissible. The second requires that anything entailed by the laws must be assigned a chance of 1.[4] Proponents of

[1]Lewis's switch was motivated by worries about the compatibility of the Principal Principle and Humean supervenience; see Lewis (1999). I discuss these matters more in appendix A. Although Lewis's (1999) New Principle and Hall's (1994) New Principle are often spoken of interchangeably, I take them to be distinct. The comments I make apply to the former.

[2]See Lewis (2004), p. 14.

[3]We can say an event occurs at t *iff* it's entailed by a history up to t and is not entailed by any history up to $t' < t$.

[4]These two derivations are provided in Appendix B. Note that neither assumption entails the other. The first assumption, that a distribution's arguments are admissible, entails that anything the chance laws entail is assigned a chance of 1. But the complete laws at a world might entail more than the chance laws. The second assumption, that anything the laws entail is assigned a chance of 1, does entail that anything the chance laws entail is assigned a chance of 1. But the second assumption does not entail (as the first one does) that, for example, whatever is entailed by the historical argument of a distribution must be assigned a chance of 1.

the Lewisian accounts have generally rejected these assumptions due to worries regarding the compatibility of chance and Humeanism. Instead, they have adopted these claims as further, independent assumptions about the nature of chance.

Despite their popularity, there are several problems with the Lewisian theories of chance. First, they are time asymmetric, and these asymmetries are incompatible with some of the chance theories considered in physics. Second, they are incompatible with statistical mechanical chances. Third, the content of Lewis's Principal Principle depends on how admissibility is cashed out, but there is no agreement as to what a precise characterization of admissible evidence should be.

In this paper I will make three proposals regarding a theory of chance. The first two proposals address these three problems by amending the problematic parts of the Lewisian theories.[5] The third proposal offers an account of some of the common features shared by the chance theories of physics. Since the third proposal isn't needed to resolve any particular problems, it's less motivated than the first two proposals. The three proposals are independent, however, so those wary of the third proposal can adopt the first two by themselves.

The rest of this paper will proceed as follows. In the second section I motivate the first proposal in two steps. In the first part of the second section I'll look at the temporal asymmetries of the Lewisian accounts, and show how they conflict with some of the chance theories considered in physics. In the second part of the second section I'll show how the Lewisian accounts are incompatible with statistical mechanical chances, and argue that the only tenable account of statistical mechanical probabilities on offer is that they're chances. In the third section I motivate the second proposal. I'll look at whether Lewis's chance-credence principle needs an admissibility clause, and argue that we should adopt a chance-credence principle which does not make use of admissibility. In the fourth section I'll present the third proposal, and apply it to two of our physical theories. In the fifth section I'll present two problems that remain, and sketch some possible responses. I'll conclude in the sixth section.

Much of the discussion of the Lewisian accounts of chance has focused on the compatibility of chance and Humean supervenience. The work done in this paper largely crosscuts these issues. Unfortunately, the issue of Humeanism so pervades the literature on chance that it is impossible to avoid it completely. In this paper I attempt the following compromise: in the body of the paper I sidestep issues regarding Humeanism, and I leave a discussion of the (lack of) implications my proposals have on Humeanism to an appendix.

[5] Ned Hall has independently proposed that we get rid of admissibility in a recent paper; see Hall (2004). Frank Arntzenius has independently proposed revisions similar to my first two proposals in an unpublished paper.

2 The First Proposal

2.1 Time Asymmetry

There are two temporal asymmetries in the Lewisian theories of chance. First, there is the assumption that the second argument of chance distributions are histories. Second, there is the claim that the past is no longer chancy. These asymmetries make the Lewisian accounts incompatible with some of the chance theories considered in physics, such as the Aharonov, Bergmann and Lebowitz (ABL) theory of quantum mechanics and classical statistical mechanics.[6]

Lewis recognized that the temporal asymmetry of his account was a deficit: "Any serious physicist, if he remains at least open-minded both about the shape of the cosmos and about the existence of chance processes, ought to do better."[7] I propose to do better: I propose to allow the second argument of chance distributions to be propositions other than histories up to a time, and to reject the claim that the past is no longer chancy.

We will see how the asymmetries of the Lewisian accounts conflict with classical statistical mechanics in the next subsection. In this subsection we will look at how these asymmetries rule out the ABL theory.

In standard theories of quantum mechanics the chance of a measurement result at t_1 is determined by the state of the wave function prior to the measurement, the prior wave function being pre-selected by an earlier measurement at t_0. According to the ABL theory, these chances are incomplete. On the ABL theory, the chance of a given outcome at t_1 is determined by the wave functions prior *and* posterior to the t_1 measurement, the prior and posterior wave functions being pre and post-selected by an earlier measurement at t_0 and a later measurement at t_2.[8]

Say the ABL theory assigns non-trivial chances to the possible outcomes of a measurement at t_1. On the Lewisian accounts the arguments of this chance distribution will be the ABL laws and a history up to a time. But a history up to what time? The ABL laws and this history must entail the chance distribution on the Lewisian accounts. Since the distribution depends on the results of the earlier (t_0) and later (t_2) measurements, histories up to t_0 or t_1 aren't enough to entail the distribution. To obtain the desired distribution the history must run up to t_2. But on the Lewisian accounts the past is no longer chancy: events which occur before the time associated with the chance distribution—the time the associated history runs up to—receive trivial chances. If the second argument of the chance distribution is a history up to t_2, then the distribution must assign trivial chances to the possible outcomes of the t_1 measurement. Since the ABL theory assigns a non-trivial chance to the outcome of the t_1

[6]Aharonov, Bergmann and Lebowitz (1964), Aharonov and Vaidman (1991). Note that their theory is not a *complete* theory; it doesn't include a particular solution to the measurement problem, for example.

[7]Lewis (1986), p. 94

[8]More generally, the ABL theory assigns chances given pre-measurements, given post-measurements, and given pre and post-measurements. In this context, I restrict myself to the latter chances.

measurement, the Lewisian can't accomodate the chances of the ABL theory.

We can hold on to these asymmetries and reject theories like the ABL theory, but I think we should be wary of outlawing proposed physical theories on contentious metaphysical grounds. I think a better option is to reject the asymmetric components of the Lewisian theories of chance.

Note that once we reject the Lewisian account of the arguments of chance distributions, it's hard to make sense of the claim that the past isn't chancy. The claim that the past is no longer chancy presupposes that chance distributions can be associated with a time, relative to which some events are in the past. On the Lewisian accounts chance distributions can be associated with a time because they're functions of chance laws and histories, and chance laws and histories can be picked out by a world and a time. But once we allow the second argument to be propositions other than histories up to a time, we're no longer guaranteed that a world and time will pick out what the second argument is. So once we reject the Lewisian account of the arguments of chance we're no longer guaranteed a way to associate a time with chance distributions, and a way to make sense of the claim that the past is no longer chancy.

2.2 Statistical Mechanics

Statistical mechanical theories also pose a problem for the Lewisian theories of chance. I'll draw out some of these problems by looking at a particular statistical mechanical theory, classical statistical mechanics. Since it will be useful to have a concrete theory to work with in the rest of this paper, I will sketch the theory in some detail. For simplicity, I will ignore electrodynamics and assume that all masses are point particles.

Central to statistical mechanics is the notion of a *state space*. A state space is a space of possible states of the world at a time. All of the possibilities in this space are alike with regards to certain static properties, such as the spatiotemporal dimensions of the system, the number of particles, and the masses of these particles. The individual elements of this space are picked out by certain dynamic properties, the locations and momenta of the particles.[9]

In classical statistical mechanics these static and dynamic properties determine the state of the world at a time. Classical mechanics is deterministic: the state of the world at a time determines the history of the system.[10] So in clas-

[9]In versions of classical statistical mechanics like that proposed by Albert (2000), one of the statistical mechanical laws is a constraint on the initial entropy of the universe. On such theories the space of classical statistical mechanical worlds (and the state spaces that partition it) will only contain worlds whose initial macroconditions are of a suitably low entropy.

[10]Earman (1986b), Xia (1992), Norton (2003) and others have offered counterexamples to the claim that classical mechanics is deterministic. These results have been by and large ignored by the physics literature on statistical mechanics, and I will follow suit. Two comments are in order, however. First, these cases spell trouble for the standard distinction between dynamic and static properties employed by classical statistical mechanics. For example, in some of these cases new particles will unpredictably zoom in from infinity. Since this leads to a change in the number of particles in the system, it would seem the number of particles cannot be properly understood as a static property. Second, although no proof of this exists, prevailing opinion is that these indeterministic cases form a set of Lebesgue measure zero. If so, we can ignore these cases in this context, since their exclusion will have no effect

sical statistical mechanics each point in the state space corresponds to a unique history, i.e., to a possible world. We can therefore take a state space to be a set of possible worlds, and the state space and its subsets to be propositions. The state spaces form a partition of the classical statistical mechanical worlds, dividing the classical statistical mechanical worlds into groups of worlds that share the relevant static properties.

Given a state space, we can provide the classical statistical mechanical probabilities. Let m be the Liouville measure, the Lebesgue measure over the canonical representation of the state space, and let K be a subset of the state space. The classical statistical mechanical probability of A relative to K is $\frac{m(A \cap K)}{m(K)}$.

Note that statistical mechanical probabilities aren't defined for all object propositions A and relative propositions K. Given the above formula, two conditions must be satisfied for the chance of A relative to K to be defined. Both $m(A \cap K)$ and $m(K)$ must be defined, and the ratio of $m(A \cap K)$ to $m(K)$ must be defined.

Despite the superficial similarity, the statistical mechanical probability of A relative to K is not a conditional probability. If it were, we could define the probability of A '*simpliciter*' as $m(A)$, and retrieve the formula for the probability of A relative to K using the definition of conditional probability. The reason we can't do this is that the Liouville measure m is not a probability measure; unlike probability measures, there is no upper bound on the value a Liouville measure can take. We only obtain a probability distribution after we take the ratio of $m(A \cap K)$ and $m(K)$; since $m(A \cap K) \leq m(K)$, the ratio of the two terms will always fall in the range of acceptable values, [0,1].

Now, how should we understand statistical mechanical probabilities? A satisfactory account must preserve their explanatory power and normative force. For example, classical mechanics has solutions where ice cubes grow larger when placed in hot water, as well as solutions where ice cubes melt when placed in hot water. Why is it that we only see ice cubes melt when placed in hot water? Statistical mechanics provides the standard explanation. When we look at systems of cups of hot water with ice cubes in them, we find that according to the Liouville measure the vast majority of them quickly develop into cups of lukewarm water, and only a few develop into cups of even hotter water with larger ice cubes. The explanation for why we always see ice cubes melt, then, is that it's *overwhelmingly likely* that they'll melt instead of grow, given the statistical mechanical probabilities. In addition to explanatory power, we take statistical mechanical probabilities to have normative force: it seems irrational to believe that ice cubes are likely to grow when placed in hot water.

The natural account of statistical mechanical probabilities is to take them to be chances. On this account, statistical mechanical probabilities have the explanatory power they do because they're chances; they represent lawful, empirical and contingent features of the world. Likewise, statistical mechanical probabilities have normative force because they're chances, and chances normatively constrain our credences via something like the Principal Principle.

But statistical mechanical probabilities cannot be chances on the Lewisian

on the probabilities classical statistical mechanics assigns.

accounts. First, classical statistical mechanical chances are compatible with classical mechanics, a deterministic theory. But on the Lewisian accounts determinism and chance are incompatible.

Second, classical statistical mechanics is time symmetric like the ABL theory, and is incompatible with the Lewisian accounts for similar reasons. Consider two propositions, A and K, where A is the proposition that the temperature of the world at t_1 is T_1, and K is the proposition that the temperature of the world at t_0 and t_2 is T_0 and T_2. Consider the chance of A relative to K. On the Lewisian accounts the arguments of the relevant chance distribution will be the classical statistical mechanical laws and a history up to a time. But a history up to what time? The statistical mechanical laws and this history entail the chance distribution on the Lewisian accounts. The distribution depends on the relative state K, and a history must run up to t_2 to entail K, so we need a history up to t_2 to obtain the desired distribution. Since the past is no longer chancy, the chance of any proposition entailed by the history up to t_2, including A, must be trivial. But the statistical mechanical chance of A is generally not trivial, so the Lewisian account cannot accommodate such chances.

Third, the Lewisian restriction of the second argument of chance distributions to histories is too narrow to accommodate statistical mechanical chances. Consider the case just given, where A is a proposition about the temperature of the world at t_1 and K a proposition about the temperature of the world at t_0 and t_2. Consider also a third proposition K', that the temperature of the world at t_0, $t_{1.5}$ and t_2 is T_0, $T_{1.5}$ and T_2, respectively. On the Lewisian accounts it looks like the chance of A relative to K and the chance of A relative to K' will have the same arguments: the statistical mechanical laws and a history up to t_2. But for many values of $T_{1.5}$, statistical mechanics will assign different chances to A relative to K and A relative to K'.

It's not surprising that the Lewisian account of the arguments of chance distributions is at odds with statistical mechanical chances. It's natural to take classical statistical mechanics T and the relative state K to be the arguments of statistical mechanical distributions, since T and K alone entail these distributions. But taking T and K to be the arguments conflicts with the Lewisian accounts, since while K can be a history up to a time, often it is not.

So the Lewisian accounts are committed to denying that statistical mechanical probabilities are chances. Instead, they take them to be subjective values of some kind. There's a long tradition of treating statistical mechanical probabilities this way, taking them to represent the degrees of belief a rational agent should have in a particular state of ignorance. Focusing on classical statistical mechanics, it proceeds along the following lines.

Start with the intuition that some version of the Indifference Principle—the principle that you should have equal credences in possibilities you're epistemically 'indifferent' between—should be a constraint on the beliefs of rational beings. There are generally too many possibilities in statistical mechanical cases—an uncountably infinite number—to apply the standard Indifference Principle to. But given the intuition behind indifference, it seems we can adopt a modified version of the Indifference Principle: when faced with a continuum number of possibilities that you're epistemically indifferent between, your degrees of belief in these possibilities should match the values assigned to them by an appropriately uniform measure. The properties of the Lebesgue measure make it a natural candidate for this measure. Granting this, it seems the statistical mechanical probabilities fall out of principles of rationality: if you only know K about the world, then your credence that the world is in some set of states A should be equal to the proportion (according to the Lebesgue measure) of K states that are A states. Thus it seems we recover the normative force of statistical mechanical probabilities without having to posit chances.

However, as Albert (2001), Loewer (2000), and others have argued, this account of statistical mechanical probabilities is untenable. First, the account suffers from a technical problem. The representation of the state space determines the Lebesgue measure of a set of states, and there are an infinite number of ways to represent the state space. So there are an infinite number of ways to 'uniformly' assign credences to the space of possibilities. Classical statistical mechanics uses the Lebesgue measure over the canonical representation of the state space, the Liouville measure, but no compelling argument has been given for why *this* is the right way to represent the space of possibilities when we're trying to quantify our ignorance. So it doesn't seem that we can recover statistical mechanical probabilities from intuitions regarding indifference after all.

Second, the kinds of values this account provides can't play the explanatory role we take statistical mechanical probabilities to play. On this account statistical mechanical probabilities don't come from the laws. Rather, they're *a priori* necessary facts about what it's rational to believe when in a certain state of ignorance. But if these facts are *a priori* and *necessary*, they're incapable of explaining *a posteriori* and *contingent* facts about our world, like why ice cubes usually melt when placed in hot water. Furthermore, as a purely normative principle, the Indifference Principle isn't the kind of thing that could explain the success of statistical mechanics. Grant that *a priori* it's rational to believe that ice cubes will usually melt when placed in hot water: that does nothing to explain why in fact ice cubes *do* usually melt when placed in hot water.

The indifference account of statistical mechanical probabilities is untenable. The only viable account of statistical mechanical probabilities on offer is that they are chances, and the Lewisian theories of chance are incompatible with statistical mechanical chances. I propose to amend the Lewisian theories such that they are compatible with physical theories like classical statistical mechanics and the ABL theory of quantum mechanics.

The first proposal is to allow the second argument of chance distributions to be propositions other than histories, and to reject the two additional claims about chance the Lewisian theories make: that the past is no longer chancy, and that determinism and chance are incompatible. The two additional claims of the Lewisian theories stipulate properties of chance distributions that are incompatible with time symmetric and deterministic chances; by rejecting these two additional claims, we eliminate these stipulated incompatibilities. By allowing the second argument to be propositions other than histories, we can incorporate the time symmetric arguments needed for theories like the ABL theory and the more varied arguments needed for statistical mechanical theories.

3 The Second Proposal

On the Lewisian theories, the relation that should hold between credence and chance is captured by the Principal Principle. Roughly, the Principal Principle claims that your credences should accord with what you think the chances are unless you're in possession of inadmissible evidence. The content of this principle depends on how admissibility is cashed out. If nothing is admissible the principle is vacuous, if everything is admissible the principle is inconsistent. Unfortunately, there is no agreement as to what a precise characterization of admissible evidence should be. So the content of the Principal Principle is unclear.

If a satisfactory chance-credence principle requires something like an admissibility clause, there's a pressing need to figure out what admissibility is. But it's not clear that a satisfactory chance-credence principle does require an admissibility clause. Let's try to construct such a principle, and look to see why admissibility is needed.

Intuitively, a subject who knows what the chances are should have credences which line up with those chances. Let G stand for the arguments of a chance distribution. G entails the chances it's the argument of, so a subject whose total evidence is G should have credences that line up with the chances that G entails. If we let '$ch_G(\cdot)$' stand for the chance distribution entailed by G, and let '$cr_G(\cdot)$' stand for the credences of a subject whose total evidence is G, we can express this as

$$cr_G(A) = ch_G(A), \text{ if } ch_G(A) \text{ is defined.} \tag{1}$$

The added clause is needed because for some arguments and object propositions $ch_G(A)$ won't be defined. On the Lewisian accounts, for example, $ch_G(A)$ won't be defined if G isn't a complete chance theory and a history up to a time.

If we assume Bayesianism, we can translate (1) into a constraint on reasonable initial credence functions, or *hypothetical priors.* (Hypothetical priors are 'prior' because they represent the subject's credences prior to the receipt of any evidence, and 'hypothetical' because it is unlikely that one ever is in such a state.) Bayesianism states that a subject whose total evidence is G should have credences equal to their hypothetical priors conditional on G. Letting '$hp(\cdot)$' stand for the hypothetical priors of a subject, we can express Bayesianism as

$$cr_G(A) = hp(A|G), \text{ if } hp(A|G) \text{ is defined.} \tag{2}$$

The added clause is needed here because $hp(A|G)$ won't be defined if $hp(G) = 0$.

Using (2), we can present (1) as the following chance-credence principle:[11]

$$hp(A|G) = ch_G(A), \text{ if } hp(A|G) \text{ and } ch_G(A) \text{ are defined.} \tag{3}$$

[11] Strictly speaking, (3) is not a reformulation of (1), since (3) is slightly weaker than (1). (3) will fail to apply when $hp(A|G)$ is undefined, whereas (1) does not have this limitation. In section five we will see that problems with the standard definition of conditional probability will motivate the adoption of primitive conditional probabilities. With this adoption $hp(A|G)$ will always be defined, and (1) and (3) will be equivalent.

This is similar to the rule that Lewis proposes, but without a clause regarding admissibility. If we add such a clause, we get the following principle:

$$hp(A|GE) = ch_G(A), \text{ if } GE \text{ is admissible relative to } ch_G(A), \text{ and } hp(A|G) \text{ and } ch_G(A) \text{ are defined.} \tag{4}$$

This is a version of Lewis's Principal Principle.

Why is (4) preferable to (3)? I.e., why is (3) inadequate without an admissibility clause? If we assume that the arguments of a distribution are admissible, as Lewis originally did, then (4) is a strictly stronger principle than (3). We get (3) as a special case of (4) when E is a tautology. So if we're worried about (3), we should be worried that (3) isn't strong enough without an admissibility clause: it doesn't give us all the relations between chance and credence that we intuitively think should hold.

(3) and (4) by themselves don't tell us anything about our current credences if we have evidence. For (3) or (4) to have a bearing on our current credences, we need to employ a rule which relates our hypothetical priors to our credences, like Bayesianism. So let's assume that Bayesianism holds.

In the first section, I motivated the introduction of admissibility with the following story:

> "*Ceteris paribus* it seems your credences should agree with your chances. If you believe that a coin has a .5 chance of coming up heads, then all else being equal you should have a .5 credence that the coin will come up heads. But you don't always want your credences to accord with the chances. Suppose you are in possession of a crystal ball which reliably depicts the future, and the crystal ball shows you that heads will come up. Then your credence in the coin coming up heads should be near 1, not .5. So it seems you should only let the chances guide your credences in an outcome when you're not possession of illicit or *inadmissible* evidence."

Now, it's true that you don't always want your credences to accord with the *same* chances. If your total evidence is G, then your credence in heads should line up with $ch_G(H)$. If a crystal ball then gives you some new evidence E, and $ch_{GE}(H) \neq ch_G(H)$, then your credences should no longer line up with $ch_G(H)$. But this doesn't raise any problems for (3). (3) only requires that your credences line up with the chances when your total evidence is the same as the arguments of those chances. As your evidence changes, so do the chances (3) requires your credences to line up with.

But we might worry about cases where our total evidence doesn't equal the arguments of any chance distribution. On the Lewisian picture of the arguments of chance distributions, for example, the crystal ball's evidence might be such as to leave us with a chance theory and a *partial* history up to a time as our total evidence E. Since E isn't the argument of a chance distribution, it's not clear what constraints (3) will place on our credences. With the worry that (3) is too weak in mind, it's natural to worry that (3) doesn't tell us enough about what our credences should be in these cases.

If we assume Bayesianism, though, then (3) is strong enough to capture all of the relations between chance and credence that we think should hold. We can divide our uncertainty into two kinds, uncertainty about the outcome of chance events and uncertainty about other things. We should only expect chances to have a bearing on our uncertainty about the outcome of chance events. But once we eliminate our uncertainty about other things, (3) and Bayesianism are enough to completely fix our credences in the outcomes of chance events. So no admissibility clause is needed to strengthen (3).

To see that (3) and Bayesianism fix our credences, let's look at a simple case. Assume with the Lewisian that chance distributions are functions of chance laws and histories. Consider a world where there are only three chance events, three fair coin flips, that take place at times t_1 through t_3, respectively. Consider a subject at this world who knows the laws T and the history up to t_0, H_0. Let T and H_0 entail everything about the world except how the coin tosses come up, so the subject knows everything about the world except the outcome of these chance events.

In this case the subject knows she's in one of 8 possible worlds. (3) and Bayesianism entail that her credence in each world should be $\frac{1}{8}$. If the subject learns the history up to t_1, H_1, then she'll be left with 4 worlds, and (3) and Bayesianism will entail that her credence in each of these remaining worlds should be $\frac{1}{4}$. Now consider the case we were worried about: what if she gets evidence such that her total evidence E consists of the laws and a *partial* history up to t_1?

We know this new evidence will leave her with at least 4 worlds. Precisely how many and which worlds are left will depend on what her new evidence is. But once we're told what the new evidence is, it's trivial to determine what her new credences should be. She should set her credence in the worlds incompatible with the evidence to 0, and normalize her credences in the rest. That is, like any good Bayesian she should conditionalize.

Call (3) the Basic Principle. The second proposal is to discard the admissibility clause that Lewis built into the Principal Principle, and to adopt something like the Basic Principle instead.[12] An admissibility free chance-credence principle like the Basic Principle and an updating rule like Bayesianism tell us all we need to know about the relation between credence and chance. In section four I will use the Basic Principle to show how our credences are constrained by the chances of some of our actual chance theories. I will sketch the chances of two of the complete chance theories considered in physics, classical statistical mechanics and statistical Bohmian mechanics, and then sketch the acceptable priors of a subject with respect to these theories.

[12] Although admissibility is no longer needed to decipher the relation between credence and chance, admissibility-free principles like the Basic Principle provide us with the means to provide a precise characterization of what admissibility is. For example, we can characterize 'admissible evidence' as used in (4) as follows: GE is admissible relative to $ch_G(A)$ *iff* the priors (4) would then assign are the same as the priors recommended by (3); i.e., $hp(A|GE) = hp(A|G)$. With a characterization of admissibility in hand, the Principal Principle is no longer vague. But since we only eliminate this vagueness by using the Basic Principle, the Basic Principle is the more fundamental of the two. For another way of cashing out admissibility, see Hall (2004).

4 The Third Proposal

The third proposal is an account of the arguments and objects for which chance distributions are defined, and of their relation to the values assigned by chance theories. Unlike the first two proposals, the third proposal is not motivated by any particular problems. As such, it is more tentative than the first two. In support of the third proposal, I offer the following: it is compatible with the chance theories considered in physics, and, if true, it explains several similarities between the chance theories that physics considers that would otherwise be accidental.

The three proposals are largely independent, so those who reject the third proposal can still accept the first two. In any case, the third proposal provides a useful framework for working with the chance theories of physics, and *a fortiriori* for figuring out the constraints that these theories place on our credences. If all three of the proposals are correct, we can say quite a lot about chance, chance theories, and the chance-credence relation. I do not claim, however, that this provides a complete theory of chance. Among other things, the proposals leave two issues unresolved; issues which come into the spotlight once we look closely at the chance theories of physics. In the next section I will briefly present these issues, and sketch some possible responses.

The third proposal is that every chance theory T has the following structure. First, the worlds of the theory can be partitioned into *coarse sets*. The coarse sets are the broadest regions of possibility to which the theory assigns well defined chances. In classical statistical mechanics the coarse sets are the state spaces; i.e., sets of classical statistical mechanical worlds which share the relevant static properties.

Second, the coarse sets can be partitioned into *fine sets*. The fine sets provide the smallest units of possibility to which the theory assigns well defined chances. In classical statistical mechanics the fine sets are the points of the state spaces; i.e., individual possible worlds.

Third, each coarse set is associated with a countably additive measure. Each measure is defined on an algebra S over the associated coarse set C, where S includes all of the fine sets of C but no proper subsets of these sets except the empty set.[13] These measures encode the chances of the theory, although they themselves need not be probability measures. In classical statistical mechanics these measures are the Liouville measures over the state spaces.

Given this structure, I propose that the chances of the theory T are as follows: for any propositions A and K, either (i) $ch_{TK}(A) = \frac{m(A \cap K)}{m(K)}$, where m is the measure T associates with a coarse set C that contains K, or (ii) $ch_{TK}(A)$ is undefined, if the above prescription fails to pick out a unique well-defined value. This entails that $ch_{TK}(A)$ is defined *iff* (a) T is a complete chance theory and K is a subset of a coarse set C of T, (b) the ratio of $m(A \cap K)$ to $m(K)$ is defined, and (c) $A \cap K$ and K are elements of S, the algebra over

[13]A *countably additive measure* over (C, S) is a function $m : S \to [0, \infty]$, where S is a sigma algebra over C, such that $m(\emptyset) = 0$, and such that if $s_i \in S$ are disjoint, then $m(\cup_{i=1}^{\infty} s_i) = \sum_{i=1}^{\infty} m(s_i)$. A *sigma algebra* S over C is a non-empty family of subsets of C which is closed under complementation and countable unions.

which m is defined. As we saw in section two, this lines up with the chances that classical statistical mechanics assigns and the conditions under which the classical statistical mechanics chances are defined.

With this picture of chance theories in hand, let's turn to the question of how our priors should be constrained by the chances according to an admissibility free chance-credence rule like the Basic Principle. There may be a number of objective constraints on one's priors, but in this context we're only interested in those imposed by the chances. So let us assume a version of subjective Bayesianism on which the Basic Principle is the only objective constraint on our priors.

Given the structure outlined above, we can divide up the space of possible worlds into smaller and smaller regions by applying finer and finer partitions. We can partition the space of possible worlds into chance theories T_i, partition the chance theories into coarse sets C_j, partition the coarse sets into fine sets F_k, and partition the fine sets into individual worlds W_l.[14] Now consider an arbitrary proposition, A. We know that if some sets X_i form a partition of A, we can express $hp(A)$ as

$$hp(A) = \sum_i hp(A \wedge X_i) = \sum_i hp(X_i)hp(A|X_i) \tag{5}$$

By applying (5) repeatedly for each of the above partitions, we can express $hp(A)$ as

$$\begin{aligned} hp(A) &= \sum_i hp(T_i)hp(A|T_i) \\ &= \sum_{i,j} hp(T_i)hp(C_j|T_i)hp(A|T_iC_j) \\ &= \sum_{i,j,k} hp(T_i)hp(C_j|T_i)hp(F_k|T_iC_j)hp(A|T_iC_jF_k) \\ &= \sum_{i,j,k,l} hp(T_i)hp(C_j|T_i)hp(F_k|T_iC_j)hp(W_l|T_iC_jF_k)hp(A|T_iC_jF_kW_l) \\ &= \sum_{i,j,k,l} hp(T_i)hp(C_j|T_i)hp(F_k|C_j)hp(W_l|F_k)hp(A|W_l) \qquad (6) \end{aligned}$$

So we can determine the value of $hp(A)$ by figuring out the values of the five sets of terms in (6).[15]

The first set of terms are of the form $hp(T_i)$, and represent our prior in a chance theory. Since we need to assume that a particular chance theory holds before we can get any chances, how we should divide our priors among chance theories is beyond the scope of chance. So our priors in the first set of terms

[14] What about theories without chances? We can take these to all correspond to some dummy chance theory T_0 with only one coarse set and one fine set, and divide the priors assigned to that "theory" among individual possible worlds directly.

[15] I'm implicitly assuming that the indices i, j, k, l range over countably infinite members at most. Strictly speaking, this assumption should be discarded and these sums should be replaced by integrals over the appropriate probability densities.

will be determined by subjective considerations. The second set of terms are of the form $hp(C_j|T_i)$, and represent our prior in a coarse set given a chance theory. Since we need to fix on a coarse set before a theory can assign chances, how we should divide our prior in a chance theory among its coarse sets is also beyond the scope of chance. So our priors in the second set of terms will also be determined subjectively.

The third set of terms are of the form $hp(F_k|C_j)$, and represent our prior in a fine set given a coarse set. This is the regime where chances come in. If $hp(C_j) > 0$ and $ch_{T_i,C_j}(F_k)$ is defined, then the Basic Principle applies and $hp(F_k|C_j) = ch_{T_i,C_j}(F_k)$. So our priors in these terms will generally be fixed by the chances. If $hp(C_j) = 0$ or $ch_{T_i,C_j}(F_k)$ is not defined, then the Basic Principle won't apply. To the extent to which the other constraints the chances have imposed and your previous prior assignments leave it open, your prior in $hp(F_k|C_j)$ will be determined subjectively.

The fourth set of terms are of the form $hp(W_l|F_k)$, and represent our priors in an individual world given a fine set. Since the fine sets are the smallest units to which chances are assigned, once we've fixed on a fine set the chances have nothing more to say. So as with the first two sets of terms, our priors in the fourth set of terms will be determined subjectively. The fifth set of terms are of the form $hp(A|W_l)$, and represent our priors in A given an individual world. These are trivial to determine. If $W_l \in A$ then $hp(A|W_l) = 1$, if $W_l \notin A$ then $hp(A|W_l) = 0$.

Given the third proposal, it's clear where and to what extent the chances constrain our priors. Likewise, it's clear how to determine what our priors are in the worlds of a given chance theory. We subjectively determine our priors in the chance theory and its coarse sets, align our priors in the fine sets of these coarse sets using the chances, and then subjectively determine our priors in individual possible worlds. So to determine our priors in the classical statistical mechanical worlds, we determine our subjective prior in classical statistical mechanics, divide this subjectively among the state spaces, and divide our prior in each state space among its points in accordance with the statistical mechanical chances. Since the points of state space are individual possible worlds, we don't need to divide our priors any further.

We've seen how the third proposal works for classical statistical mechanics. Let's look at how the proposal works for a different chance theory, statistical Bohmian mechanics, the complete chance theory encompassing Bohmian mechanics and quantum statistical mechanics. Unlike classical statistical mechanics, the chances of statistical Bohmian mechanics are generally segregated into the chances of Bohmian mechanics and the chances of quantum statistical mechanics. To apply the third proposal to statistical Bohmian mechanics we need to glue the chances of Bohmian mechanics and quantum statistical mechanics together, and fit them into the framework given above.

Fortunately, this framework makes this easy to do, since it can be applied to Bohmian mechanics and quantum statistical mechanics independently. It then becomes clear how to merge the two theories into a single theory in the above framework.

I will first give a brief description of quantum statistical mechanics and

Bohmian mechanics. To avoid a lengthy discussion of these theories, I won't present them in as much detail as I presented classical statistical mechanics. Instead, I will simply give a gloss of their relevant features, and then sketch how each fits into the above framework.

As with classical statistical mechanics, quantum statistical mechanics considers spaces of possibilities that share certain static properties, such as spatiotemporal dimensions of the system, the number of particles, etc. The elements of these spaces are picked out by certain dynamic properties, in this case the property of having the same wave function at a given time. Quantum statistical mechanics assigns a canonical measure over these possibilities from which the chances are derived.[16]

Bohmian mechanics is an interpretation of quantum mechanics that adds hidden variables to the formalism, in this case the positions of the particles. In Bohmian mechanics a complete description of a system at a time is given by the static properties considered above and the wave function and particle positions of the system. Both the wavefunction and the particles evolve deterministically, so a complete description of the system at a time fixes the history of the system. Bohmian chances come in when we consider possibilities that have the same wave function and relevant static properties but differ in particle positions. Bohmian mechanics assigns a special measure over this space which determines the chances.[17]

The framework given above straightforwardly applies to each of these theories. In quantum statistical mechanics the coarse sets are sets of possibilities that share the relevant static properties, and its fine sets are the sets of possibilities with the same wave function. In Bohmian mechanics the coarse sets are sets of possibilities that share the relevant static properties and have the same wave function, and its fine sets are the sets of possibilities with the same particle positions. Since the relevant static properties, wave function, and particle positions at a time determine the history of a system, these fine sets are individual possible worlds.

Since the fine sets of quantum statistical mechanics are the coarse sets of Bohmian mechanics, gluing the two theories together is simple. Let the coarse sets of quantum statistical mechanics be the coarse sets of the combined theory, and let the fine sets of Bohmian mechanics be the fine sets of the combined theory. Then we get the appropriate measures for the combined theory, statistical Bohmian mechanics, by essentially taking the product of the quantum statistical mechanical measures and the Bohmian mechanical measures.

With statistical Bohmian mechanics formulated in terms of the above framework, we can sketch what our priors should be in the manner given above. First we determine our subjective prior in statistical Bohmian mechanics, and divide this subjectively among the coarse sets of the theory. Then we divide our prior

[16] In quantum statistical mechanics one generally works with probability density operators, not probability measures over states, and the density operators underdetermine the probability measures that could be used to justify it. But a satisfactory justification for the density matrix used in quantum statistical mechanics can (and perhaps must) be obtained from a measure over states. For one way to do this, see Tumulka and Zanghi (2003).

[17] See Berndl, Daumer, Durr, Goldstein and Zanghi (1995).

in each coarse set among its fine sets in accordance with the chances. Since in this case the fine sets are individual possible worlds, we don't need to divide our priors any further.

Although we've considered the combination of quantum statistical mechanics and Bohmian mechanics, a similar procedure can be used to obtain the complete chance theory of quantum statistical mechanics and other quantum mechanical interpretations.[18] For genuinely indeterministic interpretations, for example, we can obtain the chances of histories that share the relevant static properties by essentially taking the product of the quantum statistical mechanical chances for their initial wave functions and the stochastic chances of their histories given those initial wave functions.

5 Two Problems

These three proposals leave a number of issues unresolved. Two of these issues become particularly urgent when we look closely at the chance theories of physics. In this section I will raise these two issues, and sketch some possible responses.

5.1 The first problem

The first problem concerns the tie between credence and chance. Assume that, like Lewis, we formulate our chance-credence principle in terms of hypothetical priors. The problem then is that our priors in worlds where chance theories like classical statistical mechanics only end up being constrained by trivial chances. That is, the values of the non-trivial statistical mechanical chances are epistemically irrelevant, since they have no effect on our priors.

A rigorous derivation of this result is given in Appendix B.3, but the following is a rough sketch of how the problem arises. If the relative state K of a statistical mechanical chance is of infinite measure, then that chance will be trivial or undefined.[19] So the relative state K of a non-trivial chance must be of finite measure. Now, any prior you have in a classical mechanical state space is required by the chances to be spread uniformly over that space in accordance with the Liouville measure. Since the state spaces of classical statistical mechanics are of infinite measure, any finite measure region of such a space will be assigned a 0 prior.[20] So the relative state K of a non-trivial chance will be

[18]By this I mean *complete* quantum mechanical interpretations, not interpretations whose content hangs on vague terminology or which are otherwise imprecise. I take it that I am under no obligation to provide a precise account of the chances of chance theories which are not themselves precise.

On some quantum mechanical interpretations the status of quantum statistical mechanics changes to the extent that a procedure for gluing quantum mechanics to quantum statistical mechanics isn't needed. For example, Albert (2001) has argued that if we adopt the GRW interpretation of quantum mechanics an additional statistical theory isn't needed to explain thermodynamic phenomena.

[19]I'm assuming the extended real number line and the standard extension of the arithmetical operators over it; in particular, that $\frac{x}{\infty} = 0$ if x is finite, and $\frac{\infty}{\infty}$ and $\frac{x}{0}$ are undefined.

[20]There is one state space of finite measure, the trivial state space of a system with no particles. But since the chances associated with this space are trivial, we can safely ignore it.

assigned a 0 prior. But the Basic Principle only applies if one's prior in the arguments TK of the chance distribution are non-zero, since otherwise $hp(A|TK)$ is undefined. Since one's prior in the relative state K of any non-trivial chance will be 0, it follows that the Basic Principle never applies to non-trivial statistical mechanical chances.

We saw the source of the problem in section two. The problem arises because chance-credence principles like (3) and (4) attempt to equate statistical mechanical chances with conditional priors. But as we saw in section two, we can't equate the statistical mechanical chance of A relative to K with a conditional probability. To do so would require us to make sense of the probability of A *simpliciter*, where the probability of A *simpliciter* is set equal to the Liouville measure of A. But the Liouville measure is not a probability measure, since there is no upper bound to the values it can assign. So these values generally won't make sense as probabilities. The clauses in (3) and (4) that require $hp(A|TK)$ and $ch_{TK}(A)$ to be defined prevent contradictions by severing the chance-credence connection in problematic cases. But after severing the problematic chance-credence connections we find that most statistical mechanical chances don't have an effect on our priors, and those that do are trivial.

One way to respond to this problem is to adopt a chance-credence principle like (1) that equates chances with credence-given-total-evidence. Since this principle doesn't attempt to equate chances with conditional probabilities, it avoids the problems that (3) and (4) run into. But if, like many Bayesians, we would like to encode the constraints on how we should update in our hypothetical priors, then we would like our chance-credence principle to be formulated in terms of priors.

A second way to respond to this problem is to adopt Alan Hajek's (2003) proposal to reject the standard definition of conditional probabilities. Hajek proposes that we take conditional probabilities to be primitive, and understand the formula $p(A|K) = \frac{p(A \wedge K)}{p(K)}$ to be a constraint on the values of conditional probabilities when $p(K) > 0$. Adopting Hajek's proposal avoids the problem because $hp(K) = 0$ no longer entails that $hp(A|TK)$ is undefined, and thus (3) can still apply when $ch_{TK}(A)$ is non-trivial. If we adopt this response, we can keep something like (3) as our chance-credence principle.

5.2 The second problem

The second problem concerns how we understand the objects of chances. The problem arises for chance theories whose models have certain physical symmetries. Consider an example of this problem in classical statistical mechanics.[21] Take a classical statistical mechanical state space S. Consider two disjoint regions in S of finite and equal Liouville measure that are related by a symmetry transformation. That is, the points in the first region map to the points in the second by a rotation about a given axis, a spatial translation, or some other symmetry of the relevant systems. Let A_1 and A_2 be the first and second regions,

[21] Similar kinds of case have been raised in the context of the hole argument by Marc Wilson (1993), Gordon Belot (1995) and Frank Arntzenius (manuscript).

and let K be the union of these regions. What is the statistical mechanical chance of A_1 relative to K? Since the Liouville measure of A_1 is half that of K, the chance of A_1 relative to K should be $\frac{1}{2}$. Likewise, the chance of K relative to K should be 1.

Now, the objects of statistical mechanical chances are regions of state space. We have been assuming that the objects of chances are *de dicto* propositions, i.e., sets of possible worlds. So it needs to be the case that we can take regions of state space to correspond to sets of possible worlds. In situations with symmetries like the one sketched above, it's hard to see what set of worlds to associate with a region of state space like A_1. The worlds in A_1 are qualitatively identical to the worlds in A_2, and qualitatively identical worlds are generally thought to be numerically identical. So if we say A_1 contains a world if any of its state space points correspond to that world, then it will contain the same worlds as A_2 and K. But if A_1 and K are the same proposition, then the chance of A_1 relative to K should be the same as the chance of K relative to K, which it is not. Alternatively, if we say A_1 contains a world *iff* it contains all of the state space points that correspond to that world, then A_1 will contain no worlds. But if A_1 is the empty set, then it follows from the probability axioms that $ch_{TK}(A_1) = 0$, which it does not.[22]

The problem stems from the tension between three individually plausible assumptions. The first assumption is that our chance theories successfully assign the chances they seem to assign. The second assumption is that there are no non-qualitative differences between possible worlds. This assumption addresses the intuitive difficulty of making sense of qualitatively identical but distinct possible worlds. The third assumption is that the objects of chances are *de dicto* propositions. This captures the intuition that chances are about the way the world could be. In these terms, the problem is that our chance theories seem to assign chances which are hard to make sense of if we take the objects of chance to be sets of possible worlds and take qualitatively identical worlds to be identical.

A natural response to this problem is to reject one of these three assumptions. One option is to reject the first assumption, and reject as unintelligible any apparent chance assignments whose objects or arguments don't neatly correspond to sets of possible worlds. In the context of classical statistical mechanics, this constraint will be that the object and relative state of a chance assignment must contain either all of the state space points corresponding to a world or none of them. In the above example, this gets around the problem of making sense of the chance of A_1 relative to K by denying that such chances are intelligible.

Another option is to reject the second assumption, and use *haecceities* to individuate between qualitatively identical worlds. With *haecceities* we can distinguish between worlds related by symmetry transformations, and make sense of chances with these worlds as objects. In the above example, this makes analyzing the chance of A_1 relative to K straightforward, since A_1 and K represent distinct and well-defined sets of possible worlds.

A third option is to reject the third assumption and take the objects of

[22]That $ch_G(\cdot)$ is a probablity function over possible worlds follows from the criteria laid out in section four and the assumption that the objects of chances are sets of possible worlds.

chances to be something other than (*de dicto*) propositions. On this approach A_1 would not correspond to a set of possible worlds; instead, the chance of A_1 relative to K would be made intelligible by resorting to an alternative account of the relevant space of possibilities. This option is more open ended then the first two. In addition to providing a different account of the objects of chance, this response requires a different chance-credence principle. Chance-credence principles like the Basic Principle and the Principal Principle equate values associated with the same objects; i.e., equate the chance of a proposition with a credence in a proposition. Changing the objects of chance from propositions to non-propositions requires a modification of the chance-credence principle to account for this. Either the chance-credence principle must be modified to account for how chances in non-propositions link up with credences in propositions, or the chance-credence principle must be modified so that chances in non-propositions are linked up with credences in non-propositions, and an account of credence in these non-propositions must be provided.

6 Conclusion

I have made three proposals regarding a theory of chance. The first proposal amends the Lewisian theory to accommodate physical theories like statistical mechanics and the ABL theory of quantum mechanics. I suggest we allow the second argument of chance distributions to be propositions other than histories up to a time, and I suggest that we reject the two further claims about chance that the Lewisian accounts make: that the past is no longer chancy and that determinism and chance are incompatible. The second proposal disambiguates the relation between credence and chance. Here I suggest we adopt a chance-credence principle without an admissibility clause, such as the Basic Principle. The third proposal is a partial account of the structure of chance theories, and the relation between the values of chance and the arguments and objects of chance distributions. I suggest that each chance theory should be associated with coarse sets, fine sets, and countably additive measures which determine the theories' chances.

Although I am optimistic that this makes some progress toward a complete theory of chance, further work remains to be done. I will end by noting three issues that require investigation. First, we need to clarify and analyze the possible responses to the problems raised in section five. I have briefly sketched some possible responses, but more needs to be done to see what other options are available, and to evaluate which of these responses we should adopt. Second, I've assumed that the objects of belief are *de dicto* propositions. Work may need to be done to see if, and how, chance-credence rules like the Basic Principle need to be modified when we consider *de se* and *de re* beliefs.[23] Third, I have presented

[23] See Lewis (1983). The Basic Principle can be used without modification in the context of *de se* beliefs. The Basic Principle is then a constraint on a special subset of *de se* beliefs: beliefs in centered propositions that contain all and only those centered worlds that correspond to some set of possible worlds. Interestingly, recent literature on the sleeping beauty problem has looked, in part, at the interaction between *de se* beliefs and the admissibility of chances (see Elga (2000), Lewis (2001),

the third proposal as tentative because I think it possible, if not likely, that some of the details of this proposal will be changed in a satisfactory final theory. One possible modification, for example, would be an extension of the proposal to accomodate non-standard probability spaces.[24] It is an open question what form the third proposal will eventually take.[25]

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Dorr (2002), Halpern (2004), Meacham (2003)).

[24] See Halpern (2003) for a discussion of some of these possibilities.

[25] I'd like to thank Frank Arntzenius, Maya Eddon, Ned Hall, John Hawthorne, Tim Maudlin and Jonathan Weisberg for valuable comments and discussion.

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Appendix

A. Humeanism

Much of the literature on chance has focused on the compatibility of a satisfactory chance-credence principle and Humean supervenience. My three proposals have little bearing on this issue, as I will show. The majority of this section will look at the impact of adopting an admissibility-free chance-credence principle on the debate over Humeanism. I will end with a quick note on the bearing of my other two proposals on this debate.

Lewis (1994) and others have noted that at worlds where Humean supervenience holds, a chance theory T will generally assign a positive chance to $\neg T$. Consider a simple Humean theory, frequentism. On this account, the chance of a chance event is determined by (i) assigning a chance to outcomes equal to the actual frequency (past and future) of these outcomes, while (ii) treating these events as independent and identically distributed. Now consider a world where frequentism is true, and where there are only two chance events, two coin flips, one which comes up heads and one which comes up tails. Then the chance of a coin flip coming up heads is $\frac{1}{2}$, and the chance of two coin flips coming up heads is $\frac{1}{4}$. But if the coin came up heads twice, then frequentism would assign chance 1 to the coin toss coming up heads. So it seems that Humean chances *undermine* themselves: they assign a positive chance to an outcome on which they wouldn't be the correct chances. More generally, they assign a positive chance to some other chance theory being true.

Given Lewis's Principal Principle, this appears to lead to a contradiction:

$$0 < ch_{TH}(\neg T) = hp(\neg T|TH) = 0, \tag{7}$$

where the middle equality is furnished by the Principal Principle. On further inspection, this does not lead to a contradiction because Lewis's Principal Principle is equipped with an admissibility clause. The admissibilty clause can be used to disrupt the middle equality of (7) and prevent a contradiction. But we only avoid a contradiction by making so much inadmissible that the Principal Principle is useless.

How does the Basic Principle fare? The Basic Principle leads to the same apparent contradiction as the Principal Principle, and since the Basic Principle has no admissibility clause, admissibility cannot be used to disrupt the middle equality of (7). So given the Basic Principle, undermining does appear to lead to a contradiction. Regardless of whether we adopt the Principal Principle or the Basic Principle, the Humean seems to be in bad shape.

Lewis (1994) later tried to avoid this problem by adopting an alternate principle:

$$hp(A|THE) = ch_{TH}(A|T), \text{ if } THE \text{ is admissible relative to } ch_{TH}(A|T) \text{ and } hp(A|THE) \text{ and } ch_{TH}(A|T) \text{ are defined.} \tag{8}$$

Since $ch_{TH}(\neg T|T) = 0$ even on a Humean account of chance, adopting (8) escapes the problem. A similarly modified version of the Basic Principle avoids the problem in the same way.

In either case, the move Lewis proposes is questionable. First, Arntzenius and Hall (2003) have shown that adopting this principle leads to highly counterintuitive consequences. Second, as Vranas (2002) shows, the problem that motivated Lewis's adoption of (8) is only apparent.

Take a world w at which both Humean supervenience S and the chance theory T hold. Let H be an *undermining history* of w relative to T, such that $S \wedge H \Rightarrow \neg T$. T will generally assign a positive chance to w, and so a positive chance to S. Likewise, T will generally assign positive chances to some histories like H, histories that would entail $\neg T$ if they held at a world where S held. But it doesn't follow from this that T must assign a positive chance to *both* H and S being true, and thus a positive chance to $\neg T$. T can assign a positive chance to H and a positive chance to S while assigning a 0 chance to the conjunction of H and S. So Humean chances don't need to undermine themselves. And this is true regardless of whether the chance-credence principle has an admissibility clause.[26]

The proposal to adopt an admissibility-free chance credence principle has little bearing on the issue of Humeanism. The proposal to revise the Lewisian account of the arguments of chance and to reject the two additional claims the Lewisian theories make also has little bearing on the issue. What about the third proposal? At first the third proposal seems at odds with Humeanism. In presenting the third proposal I implicitly assume that the measures associated with chance theories are assigned over the worlds where that theory holds; as a consequence, chance theories will always assign themselves a chance of 1. Since it appears that on Humeanism the chance a chance theory assigns to itself is generally less than one, this seems like an anti-Humean assumption. But as Vranas has shown us, this is a mistake; this assumption is not incompatible with Humeanism. So neither of these proposals have much bearing on the issue of Humean supervenience.

B. Derivations

B1. The past is no longer chancy

Let T be a complete theory of chance at a world, and H any history up to a time t at that world. Let E be any proposition about the past (relative to t). Since E is about the past, H entails either E or its negation.

Now, if H entails E, and $ch_{TH}(E)$ is defined, then:

$$\begin{aligned} ch_{TH}(E) &= hp(E|TH) \\ &= \frac{hp(ETH)}{hp(TH)} \end{aligned} \tag{9}$$

[26]Note that while the revised theory is compatible with the truth of Humean supervenience at this world, it's incompatible with the more ambitious claim that Humean supervenience is metaphysically or nomologically necessary.

Frank Arntzenius has pointed out that Vranas' treatment still leads to counterintuitive results for subjects who are confident that Humeanism obtains. Given this, it seems none of the Humean responses to the undermining problem are without cost.

$$
\begin{aligned}
&= \frac{hp(TH)}{hp(TH)} \\
&= 1
\end{aligned}
$$

The first line follows from the Principal Principle (see section three) and the assumption that the arguments of a distribution are always admissible relative to its chances.

On the other hand, if H entails $\neg E$, and $ch_{TH}(E)$ is defined, then:

$$
\begin{aligned}
ch_{TH}(E) &= hp(E|TH) \\
&= \frac{hp(ETH)}{hp(TH)} \\
&= \frac{hp(\neg EETH)}{hp(TH)} \\
&= 0
\end{aligned} \tag{10}
$$

As before, the first line follows from the Principal Principle and the assumption that the arguments of a distribution are always admissible relative to its chances.

So if $ch_{TH}(E)$ is defined, $ch_{TH}(E) = 1$ or 0. Since this is true for any proposition E about the past, it follows that the past is no longer (non-trivially) chancy.

B2. Determinism and chance are incompatible

Let L be the laws at a deterministic world, and T the complete chance theory at that world. Let H be any history up to a time at this world, and A be any proposition. If L is deterministic, then either $LH \Rightarrow A$ or $LH \Rightarrow \neg A$, since deterministic laws and a complete history up to a time entail everything. Equivalently, either $L \Rightarrow A \vee \neg H$ or $L \Rightarrow \neg A \vee \neg H$.

Suppose $L \Rightarrow A \vee \neg H$ and $ch_{TH}(A)$ is defined. Then:

$$
\begin{aligned}
1 &= ch_{TH}(A \vee \neg H) \\
&= hp(A \vee \neg H|TH) \\
&= \frac{hp((A \vee \neg H) \wedge TH)}{hp(TH)} \\
&= \frac{hp(ATH)}{hp(TH)} \\
&= hp(A|TH) \\
&= ch_{TH}(A)
\end{aligned} \tag{11}
$$

The first line follows from the Lewisian account of the arguments of chance and the assumption that anything the laws entail gets assigned a chance of 1 by the chance laws. The latter fact also ensures that $ch_{TH}(A \vee \neg H)$ will be defined. The second and last steps follow from the Principal Principle (see section three) and the assumption that the arguments of a distribution are always admissible relative to its chances.

So if $L \Rightarrow A \vee \neg H$ and $ch_{TH}(A)$ is defined, then $ch_{TH}(A) = 1$. If $L \Rightarrow \neg A \vee \neg H$ and $ch_{TH}(A)$ is defined, then an identical derivation yields $ch_{TH}(\neg A) = 1$, i.e., $ch_{TH}(A) = 0$. So for any history H and any proposition A, if $ch_{TH}(A)$ is defined, $ch_{TH}(A) = 1$ or 0. I.e., all of the chance distributions associated with T assign only trivial chances. Since this is true for any chance theory of a deterministic world, it follows that determinism and (non-trivial) chances are incompatible.

B3. The First Problem

If we formulate the chance-credence principle in terms of hypothetical priors, then we find that for chance theories like classical statistical mechanics, our priors only end up being constrained by trivial chances.

To see this, assume that (3) is our chance-credence principle. Now consider the Liouville measure of a state space S. If there are no particles in the systems of a state space, then the space will consist of a single point, and the associated chances will be trivial.[27] So let's confine our attention to state spaces whose systems have at least one particle. In classical mechanics there's no upper bound on the velocity of a particle, so the Liouville measure of any state space with particles will be infinite.

Assume the extended real number line and the standard extension of the arithmetical operators over it; in particular, that $\frac{x}{\infty} = 0$ if x is finite, and $\frac{\infty}{\infty}$ and $\frac{x}{0}$ are undefined. Now consider the chance of A relative to K, for some arbitrary propositions $A, K \subset S$. If $m(K) = \infty$ then $ch_{TK}(A)$ will either be undefined (if $m(A \cap K) = \infty$) or 0 (if $m(A \cap K) \neq \infty$). If $m(K) \neq \infty$, on the other hand, then $ch_{TK}(A)$ can take on non-trivial values. But if $m(K) \neq \infty$, then the chances require $hp(A|K)$ to be undefined, and (3) won't hook up our priors to these chances.

To see that the chances require $hp(A|K)$ to be undefined, suppose otherwise, i.e., suppose that $hp(K) > 0$. The chance of K relative to S will be

$$ch_{TS}(K) = \frac{m(K \cap S)}{m(S)} = 0, \tag{12}$$

since $m(K \cap S)$ is finite and m(S) infinite. And if $hp(K) > 0$ then $hp(S) > 0$, since $K \subset S$, so $hp(K|S)$ is defined. Since both $hp(K|S)$ and $ch_{TS}(K)$ are well defined, (3) applies, and

$$\begin{aligned} ch_{TS}(K) &= 0 \\ hp(K|S) &= \\ \frac{hp(K \cap S)}{hp(S)} &= \\ \frac{hp(K)}{hp(S)} &= \\ &\Rightarrow hp(K) = 0, \end{aligned} \tag{13}$$

contradicting our supposition.

[27] I follow Tolman (1938) here in not taking the total energy to be one of the relevant static properties. If we do adopt the total energy as one of these properties, then some of the details will be different.