Cause and Chance*

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Abstract: I develop a framework for thinking about objective chance that combines elements of the propensity interpretation of probability, first introduced by Karl Popper, and the subjectivist account of chance developed by Mellor, Skyrms and Lewis. The framework will provide a much clearer metaphysical and formal foundation for a propensity theory of chance than exists at present. The account makes use of causal Bayes nets to represent the causal relations on which propensities depend. I will motivate the account with a suite of problems for both propensity and subjectivist accounts of chance. Some of these problems are familiar, and already have promising-looking solutions. Others are new, or at least under-appreciated. The problems do not, at first

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blush, appear to be related to one another; I will argue, however, that the problems are all inter-related, and offer an account that deals with all of them in a unified way.

Introduction

In this paper, I develop a framework for thinking about objective chance that combines elements of the propensity interpretation of probability, first introduced by Karl Popper in 1957 (Popper 1957, 1959), and subjectivist account of chance developed by Mellor (1970), Skyrms (1980) and Lewis (1980). I use the term 'framework' instead of 'theory' to acknowledge that some of the key questions are left open. In particular, I offer no definitive answer to the question of what must be true of the world in order for the chance of some event to have some particular numerical value, rather than some other. I canvass a number of potential solutions to this problem in section 16 below, but I doubt that any one of them will be adequate for all of the events to which we wish assign a numerical chance. At the same time, the framework will provide a much clearer metaphysical and formal foundation for a propensity theory of chance than exists at present.

My account will make full use of the idea that propensities are *causal* dispositions. In particular, it will make use of causal Bayes nets to represent the causal relations on which propensities depend. I will motivate the account with a suite of problems for both propensity and subjectivist accounts of chance. Some of these problems are familiar, and already have promising-looking solutions. Others are new, or at least under-appreciated. The problems do not, at first blush, appear to be related to one another; I will argue, however, that the problems are all inter-related, and offer an account that deals with all of them in a unified way.

1. Terminology

By *probability*, I mean a mathematical function *Pr* that assigns values to elements in an algebra of propositions, or to similar items such as properties, outcomes, or events, to which the normal Boolean operations can meaningfully be applied. The probability value of a contradiction or impossible outcome or event is 0, and the probability of a tautology is 1. Moreover, probabilities are additive: if $Pr(A \wedge B) = 0$, then $Pr(A \vee B) = Pr(A) + Pr(B)$. I leave it open whether *Pr* must satisfy further conditions normally associated with probability, such as countable additivity, or the closure of its domain under Boolean operations.

An *interpretation* of probability gives the meaning of ordinary probability statements, such as 'the probability of rain today is 40%'. To give an interpretation is to characterize some structure that satisfies the basic axioms of probability theory. I do not suppose that there is one uniquely correct way to interpret all such statements.

Objective probability is probability that is rooted in objective features of the world, rather than with (for example) the beliefs of some agent,[1] or the strength of the evidence available for some claim. Finite frequencies, for example, are objective probabilities.

Chance, abbreviated *Ch*, is a type of objective probability that applies to single

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[1] This is not to say that beliefs cannot be the subject of objective probability statements. For example, the statement that .55 of Americans believe in creationism is a statement of objective probability.

events or outcomes, or to narrowly circumscribed event- or outcome-types, in virtue of the properties of some kind of experimental arrangement. 'Chance' applies paradigmatically to the outcomes of measurements on quantum systems, such as the measurement of the polarization of a photon, and to the outcomes of games of chance, involving devices like tossed coins, dice, or roulette wheels. While chances are objective probabilities, not all objective probabilities are chances. For example, the actual distribution of ages among residents of the U.K. is an objective probability. 15.7% of Britons are aged 65 or older, so this probability measure would assign probability .157 to being older than 65. But this is not a chance – it is certainly not the chance that a given Briton will live to be 65 (if that chance is even well-defined). If I were to set up some process that would select a Briton at random, with an equal chance of picking each one, then .157 would be the chance that I pick a Briton 65 years of age or older. But the chance here depends upon the details of the mechanism used, and not just on the population frequencies. I will remain agnostic as to whether physical determinism implies that all chances are 1 or 0.

Credence, or *Cr*, comprises the degrees of belief that an idealized rational agent has in an array of propositions. There are a variety of arguments that Credence should have the mathematical structure of probability, and hence that Credence is an interpretation of probability.

2. Propensities, Dispositions, and Causes

According to Popper, a propensity is a disposition of an experimental arrangement. Let's begin by considering ordinary dispositions, such as the fragility of a glass, i.e. its disposition to shatter when struck. This disposition is rooted in certain causal relations. First, there is the striking, which is a cause of the shattering. Second, there is a 'causal basis' of the shattering in the structure of the glass itself. This structure is also a cause of the shattering.

Now consider a chance set-up, such as a coin that is about to be tossed. Again, we have at least two types of causes. First, there is the thumb that propels the coin upward, and imparts a rotation to the coin. Second, there is the physical constitution of the coin itself. For example, the mass may be symmetrically distributed throughout the coin. In virtue of these causes, the coin has a certain disposition to land heads when tossed. However, unlike the case of fragility, the set of causes (perhaps including others as well, such as the constitution of the surface on which the coin will land) does not comprise a sufficient condition for the occurrence of heads. This sort of partial disposition is called a *propensity*. According to the propensity interpretation of objective probability, probability is a measure of the strength of this disposition.[2] According to this picture,

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[2] This is, at any rate, the most common interpretation of what the probability measures in a propensity interpretation. It is adopted, for instance, by Popper in his later writings (e.g. 1990), Giere (1973, 1976), Miller (1996), and Fetzer (1981). In his earlier writings, Popper suggested that a propensity is a disposition to generate characteristic frequencies when the experiment is repeated. Gillies (2000) also develops a propensity interpretation that is tied to frequencies. According to Mellor (1970), a propensity is a disposition to give rise to a certain chance distribution, so for Mellor, propensity itself is not probability.

then, propensities are rooted in a causal basis. (Note that I am using the word ‘cause’ here in a liberal sense, meaning something like ‘causally relevant factor’. A cause, in this sense, need not be sufficient for its effect, nor even raise the probability of its effect.)

3. Three Puzzles for Propensity Interpretations of Probability

In the next three sections, we will consider three puzzles for propensity interpretations of probability. The first and third puzzles are not new, and there already exist promising lines of solution to them. Nonetheless, there remain open questions about how to implement these solutions, and so the puzzles may still be used to motivate a new approach. The first puzzle is Humphreys’ (1985) argument that not all objective probabilities can be interpreted as propensities. The second puzzle concerns what kinds of sets of causes give rise to probabilistic propensities. The third puzzle is that of where the mathematical structure of probability comes from.

4. Humphreys’ Reversibility Objection

Humphreys (1985) argues that propensities cannot provide an interpretation of probability. The reason is that if at least some propensities are probabilities, then it follows from the probability calculus alone that there are further probabilities that cannot plausibly be interpreted as propensities. Earman and Salmon (1989) provide the following example. A factory that manufactures frisbees has two machines, an old machine, and a new machine. The old machine produces 20% of the frisbees, while the new machine produces 80%. Each machine will occasionally produce a defective frisbee. The old machine produces defective frisbees at a rate of 2%, while the new machine produces defective frisbees at a rate of 1%. Let *f* denote a particular frisbee, *Nf* the proposition that *f* is produced by the new machine, *Of* the proposition that *f* is produced by the old machine, and *Df* the proposition that *f* is defective. It seems natural to say that $Pr(Df|Nf) = .01$, and that this is propensity of the new machine to produce defective frisbees. If we take $Pr(Nf) = .8$ and $Pr(Of) = .2$, we can also calculate the overall probability that a frisbee is defective, $Pr(Df) = .012$. This might be interpreted as the propensity of the factory to produce a defective frisbee. So far, so good. But the probability calculus tells us that we will also have conditional probabilities running in the opposite direction: $Pr(Nf|Df)$. Using Bayes' rule, $Pr(Nf|Df) = Pr(Nf)Pr(Df|Nf)/Pr(Df) =$ 2/3. But it is hard to interpret this probability as a propensity: a defective frisbee doesn't have a propensity to have been made the new machine (or if it does, it will be 0 or 1). The problem is that conditional probabilities exist in both directions, but the causal arrow points in only one direction. So it won't be possible to interpret the backward pointing conditional probabilities as causal propensities.

The correct response, I think, is that we should not require of an adequate propensity interpretation of probability that all probabilities be interpretable as propensities. Rather, following Suppes (1987), what we should require is that it be possible to prove a *representation theorem,* along the lines of the traditional representation theorems for degrees of rational belief. Given a system of propensities, it should be possible to show that we can construct a probability measure (ideally unique) in which the propensities are represented as probabilities. Thus for example, given that the propensity of the factory to produce defective frisbees is .012, the propensity of the new machine to produce frisbees is .01, and the propensity of the old machine to produce frisbees is .02, a probability measure in which *Pr*(*Nf*) = .8 and *Pr*(*Of*) = .2 uniquely yields probability values for *Pr*(*Df*), *Pr*(*Df*|*Nf*) and *Pr*(*Df*|*Of*) that equal the corresponding propensities.

Suppes (1987) provides a variety of such representation theorems for specific kinds of physical system, such as a tossed coin, or an unstable atom. My goal will to be provide an analogous representation theorem that can be applied to a wide variety of causal systems.

5. The Causal Basis

The second puzzle for propensity theories is to specify what kinds of sets of causes give rise to chances. The standard answer, given explicitly by Popper (1990) and Miller (1996), and also implicit in Lewis (1980), is that the chance of an event *E* is determined by the complete state of the world at a time *t* earlier than *E*, or at least the complete state of a spacelike surface that completely intercepts the backward light-cone of *E*. There is certainly something importantly right in relativizing chances to times. Suppose, for example, that there are three coins in front of me. One is fair, and the other two are biased. The biases of the latter two coins are equal and opposite. At noon, I will flip the fair coin. The outcome will determine which biased coin I will flip at 12:02. If the result of this second flip is heads, I will win a prize. In this set-up, my chance of winning will be .5 at 11:59 A.M., but at 12:01 P.M. my chance of winning will be different from .5. In effect, the chances at the two times correspond to two distinct chance set-ups. The chance set-up at 11:59 is the two-stage process involving the three coins. As of 12:01, however, the chance set-up involves only the one toss of a particular biased coin.

Fetzer (1981) claims that chances are determined by the complete set of causes of the outcome, rather than the total state of the universe. This seems an improvement, for it would make it substantially easier to reproduce chance set-ups. On the other hand, if by 'the complete set of causes' we mean all of the causes that ever occur, there would seem to be no way in which the chance of an event could evolve with time. A view that combines elements of both views would be that chances are determined by the complete set of causes that are present *at a specific time*.

But even this suggestions is too restrictive, for at least two reasons. The first is that the requirement that the complete state of the world at a time, or the complete set of causes at a time, determine chances would imply that physical determinism is incompatible with chances other than 0 or 1. That is a consequence that some will not regret, but I think that it is at least a drawback.

The second, more troubling problem is that there is no reason why temporally heterogeneous sets of causes should not be able to determine genuine chances. Suppose that Alice, Barry, and Carol each have a fair coin. Alice will flip her coin twice (at 12:00 and 12:01), then Barry will do the same (at 12:02 and 12:03), followed by Carol (at 12:04 and 12:05). If all six tosses yield the same result, the three will share a prize. Suppose that Alice has already flipped her coin twice, and Bob has flipped his coin once. All three flips came up heads. At this point (shortly after 12:02), three flips remain, and all three must land heads up for the prize to be won. Thus the chance of success at this particular time is 1/8. So far so good. But now suppose instead that the three flip their coins simultaneously: they each flip once at noon, and once at 12:02. Now it seems perfectly sensible to ask what chance is conferred upon success by Alice's two tosses and Barry's first toss all coming up heads. The chance is 1/8, just as before. But now the set of causes that go into determining this chance are not all simultaneous. Rather, the chance is determined by the state of Alice's tosses after 12:02, of Barry's tosses at 12:01, and of Carol's prior to noon. Because of the causal independence of the coin tosses, it does not matter to the chance of success when they occur. What is wanted is some way of specifying what counts as a sufficiently complete set of causes that focuses on the causal structure underlying a chance process, rather than on the time at which the causes occur. To my knowledge, this problem has never been addressed in the literature.

6. The Problem of Mathematical Structure

Suppose that a particular coin has a certain propensity to land with heads facing up when tossed. Where does the mathematical structure of probability come from? We may break this question down into two parts. First, if we say that the propensity to land heads is .5, what does this number mean? What is there, in the world, that has just this magnitude? Some authors (e.g. Popper 1959, Giere 1973) say that propensity is a physical magnitude akin to mass, force, or charge. But is there really some *thing* that is there (perhaps in equal quantities) when I toss a coin, a physicist measures the polarization of a photon, and Shaquille O'Neal shoots a free throw? I will have something to say about this part of the question in section 16 below, but my main focus will be on the second part of the question.

The second part of the question is why we should think that propensities satisfy the basic axioms of probability. It is surely an empirical matter, e.g., that charge comes in discrete quantities (one-third the charge of the electron) and in both positive and negative magnitudes. This cannot be determined *a priori*. But it is hard to imagine discovering empirically, e.g., that chances could be greater than 1 or less than 0, or that they are not additive. Perhaps, following Putnam (1969), we might allow that *some* features of chance can be determined empirically. Perhaps, for instance, we might take quantum mechanics to show that objective chances do not always obey the distributive law: $Ch(A \wedge (B \vee C)) = Ch((A \wedge B) \vee (A \wedge C))$. But it is hard to understand what it could even *be* to discover empirically that chances can be greater than 1 or less than 0.

In order to make this problem vivid, consider the probabilistic interpretation of the amplitudes in the quantum wave function, introduced by Max Born in 1926 (Born 1926). The quantum state of a system is represented by a state vector ψ of unit length. If $\mathcal{O}$ is some observable magnitude (energy, spin, momentum, etc.), $\mathcal{O}$ corresponds to an operator, and the possible values of the observable correspond to eigenvectors of that operator. If $\{\psi_1, \psi_2,\ldots\}$ are the eigenvectors corresponding to values $\mathcal{O}_1, \mathcal{O}_2,\ldots$ of $\mathcal{O}$, then we can write ψ as $\psi = \sum_i c_i\psi_i$, where the c_i's are complex numbers such that $\sum_i |c_i|^2 = 1$. Born proposed that if we measure the value of $\mathcal{O}$ on a system in state ψ, then we will get outcome $\mathcal{O}_i$ with probability c_i. Then, in a footnote added in proof, he said that the probability is rather $|c_i|^2$. My puzzle is, why was the correction offered in the footnote necessary? Why did Born not rather say in his footnote 'thus we have the rather surprising consequence that probabilities can be complex valued, and that probabilities need not sum to one'? After all, the amplitudes c_i, are objective features of the system; what disqualifies them from being the chances?

If we literally think that propensities are physical magnitudes akin to mass, force, or charge, I do not think that there can be any satisfactory answer to this problem.

However, there have been a number of attempts to characterize the values of propensities, to calibrate them, as it were, by tying them to other structures that do satisfy the axioms of probability (or something close enough). The candidate structures are: (i) the set of possible outcomes of an experiment; (ii) frequencies; and (iii) rational credences.[3] We will consider each in turn.

I. *Outcomes.* Both Giere (1976) and McCall (1994) propose that propensity is a measure of the possible worlds in which the different possible outcomes occur. For example, when a coin is tossed, the probability of heads is the measure of the set of outcomes in which the coin lands heads, divided by the measure of the total set of outcomes. For McCall, these outcomes have an objective reality. In any chancy experiment, the world 'branches' with different branches corresponding to the different possible outcomes. The propensity for a particular outcome is the proportion of worlds in which that outcome results. If there are infinitely many possible outcomes, the propensity is determined by the fine details of the branching structure. Giere proposes a similar model, but advances it only as a 'semantics' for propensities; his commitment to the underlying ontology is unclear. Since proportions obey the standard laws of probability,

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[3] There is a third possible line, developed by Giere (1976) and McCall (1994). This is the idea is that propensity is a measure of the possible worlds in which the different possible outcomes occur. This view will share many of the difficulties of the classical conception of probability. For example, if the bias of a coin gives it an irrational probability of coming up heads, Giere and McCall have to posit an uncountable infinity of possible worlds for the two outcomes. This strikes me as ontologically extravagant.

propensities would inherit this mathematical structure.

This view will share many of the difficulties of the classical conception of probability. For example, if the propensity of heads is not quite .5, but rather, say, .5027, there must be not just two outcomes, but ten thousand. And if the propensity of heads is irrational, there must be an uncountable infinity of distinct outcomes. This strikes me as ontologically extravagant, at least if one takes McCall's line and imbues the outcomes with genuine existence. Moreover, in the infinite case, one needs additional structure beyond the set of outcomes. For Giere, one needs a measure over the set of outcomes. But it is not at all clear what features of the world could give content to the values assigned by the measure; the measure seems to re-introduce the very problems we are seeking to avoid. For McCall, one needs to posit an elaborate branching structure, which only adds to the overall metaphysical implausibility of the view.

II. *Frequencies.* In his earlier writings, Popper often talked as though propensities were tied to frequencies. We may consider two types of frequencies: relative frequencies in a finite set of trials, and limiting relative frequencies in a hypothetical, infinite sequence of trials. Consider, for example, a set of coin tosses. If a coin is tossed n times, with m of those tosses resulting in heads, then the relative frequency of heads in that set of tosses in m/n. In a hypothetical, infinite sequence of tosses, let H_n denote the number of heads in the first n tosses. Then the limiting relative frequency of heads is the limit as n goes to infinity of H_n/n, if it exists. Finite relative frequencies obey all the standard axioms of probability; limiting relative frequencies are not countably additive, and the set of outcomes for which limiting relative frequencies exist is not necessarily closed under the normal Boolean operations, but limiting relative frequencies otherwise have all of the properties that we normally associate with probabilities. Thus if propensities are somehow tied to relative frequencies, they could inherit the mathematical structure of relative frequencies. There are at least three different sorts of connection that one might try to establish between propensities and frequencies: causal/explanatory, conceptual/definitional, and evidential.

II.i. *Causal/Explanatory Connections.* According to the first proposal, propensities cause certain characteristic frequencies to result from repeating an experiment a large number of times, or explain why such frequencies occur. For example, if a coin has a propensity of .5 to land with the heads side facing up when tossed, this propensity will cause the coin to land heads roughly half the time in a large sequence of trials, and the propensity will explain this frequency. This way of tying propensities to frequencies is problematic in a number of respects. First, the propensity looks something like Molière's *virtus dormitiva*. We cannot explain why tryptophan makes one sleepy simply by saying that it is disposed to produce sleep: that is doing little more than re-stating the explanandum. Similarly, to 'explain' why a coin landed heads on roughly half of its tosses by citing its propensity of .5 to land heads seems to be saying little more than that the coin was disposed to behave as it did. If there is any causal explanation to be given, it is surely the causal basis of the propensity, rather than the propensity itself, that will have to do the heavy lifting.

Another worry with this approach is that a fair coin (e.g.) *may* produce a sequence of tosses in which the coin always lands heads, always lands tails, or has any relative frequency in between. Jeffrey (1969) has argued that the quality of the explanation is just the same, regardless of the result. The most that one can do by way of explanation is to describe the causal setup, state what the probability of various outcomes are, and state which outcome did, in fact, occur (see also Railton 1978). So there is no one characteristic frequency to which the propensity of the fair coin stands in a causal/explanatory relationship. Rather, it stands in the same causal/explanatory relationship to any relative frequency of heads and tails is in fact produced.

It might be suggested that the explanatory connection between a propensity and a relative frequency is not causal, but rather fits Hempel's (1965) model of *deductive-statistical* explanation. Thus, if the propensity for a coin to land heads up when tossed is .5, then if the coin is tossed 50 times, the probability is roughly .97 that the frequency of heads will be between 18 and 32 (inclusive). But this suggestion simply begs the question. If propensity does not have the structure of probability, then we cannot expect them to obey the law of large numbers, or otherwise use the laws of probability to derive information about frequencies from propensities.

III.i. *Conceptual/Definitional.* An alternative line holds that the connection between propensities and frequencies is conceptual: the strength of the propensity just is, by definition, the relative frequency that would result from a repeated sequence of trials. The finite frequency version of this suggestion will suffer from all of the usual defects of finite frequency theories of probability (see, e.g., Hàjek 1997). For example, if the propensity for heads on a coin toss is .5, and the coin will be tossed 50 times, then (if propensities are probabilities) the laws of probability tell us that the propenisty of getting exactly 25 heads is only .112. But on a finite frequency account, if the propensity of heads is .5, then by definition the coin *must* land heads up on exactly 25 out of 50 tosses.

More promising is to identify the propensity with the limiting relative frequency that *would* result in a purely hypothetical infinite sequence of tosses. But even this version of the theory is not without its problems. First, it should at least be *possible* that an infinite sequence of tosses of a fair coin would result in heads on every toss, or some other sequence incompatible with a limiting relative frequency of .5, although the probability of such an outcome is zero. Second, how are we to understand the counterfactual? What would the world have to be like in order for the coin to be tossed infinitely many times? The coin would have to be infinitely durable, there would have to remain a sufficient gravitational gradient in which to conduct the experiment, and so on. Such a world would be very, very different from our own. Who is to say what might happen if the coin were flipped an infinite number of times in such a world? Finally, it is not at all clear what talk of propensities would be adding to the story. That is, how would the resulting interpretation of probability differ from the limiting relative frequency conception of Von Mises (1957) and Reichenbach (1949)? At best, the appeal to propensities would indicate that it is the causal basis of the propensity that grounds the truth of the counterfactual about what would happen in an infinite sequence of tosses. But given how different that causal basis would have to be for an infinite sequence to really be possible, it's not clear that the propensity can do even this much work.

These worries about tying propensities to frequencies are familiar (se e.g. Hájek 2009). The situation is hardly hopeless, and I certainly encourage others to continue developing this approach, but the problems are sufficiently thick on the ground to motivate the exploration of another avenue.

II.iii. *Evidential Connections*. Another tack would be to make the claim between frequencies and propensities an evidential one. The idea is that a relative frequency of heads of m/n would most strongly support the hypothesis that the propensity is (close to) m/n, or that such a hypothesis provides the best fit with the data. In fact, I think that the approach discussed in part III below can be understood as a Bayesian version of this strategy. I leave it open whether it might be possible to develop a version of this strategy from within other confirmation-theoretic frameworks.

III. *Credences.* Mellor (1970), Skyrms (1980), and Lewis (1980) have all attempted to tie objective chance to credence. There are independent arguments, such as the Dutch Book arguments of Ramsey (1931) and De Finetti (1937), representation theorems for rational sets of preferences, and arguments based on scoring rules (e.g. Joyce 1998), that the degrees of belief of a certain sort of idealized rational agent should obey the rules of the probability calculus. These arguments are not completely problem-free, but as with frequencies and outcome spaces, they do seem to give us an independent foothold on the mathematical structure. Perhaps, then, propensities could inherit their mathematical structure from credences.

The most explicit form of the proposal is due to David Lewis. If *Ch* is a mathematical function, then a rational agent is treating this function as objective chance just in case her credences satisfy the *Principal Principle* (PP):

PP $\quad Cr(A \mid Ch(A) = r \wedge E) = r$

Where *Cr* is the agent's credence function, *A* is any proposition, *r* is a number, and *E* is any proposition that is *admissible* with respect to *Ch* and *A*. We will have a good deal more to say about admissibility shortly. For now, we will note inadmissible propositions typically contain information about happenings *after* the time at which the chances were evaluated. The proposition *A* itself is inadmissible (since $Cr(A \mid Ch(A) = r \wedge A) = 1$, regardless of *r*). Propositions about *later* chances are inadmissible. Partial information about the outcomes of chance processes will also be inadmissible; for example, if *A* is the proposition that the first toss of the coin lands heads, the proposition 'two of the first three tosses land heads' will be inadmissible.

Essentially, PP says that a rational agent will set her credences to what she believes the chances to be (assuming, as always, that she has no inadmissible information). In case she has no firm belief about what the chance of *A* is, it follows from the *PP* that her credence in *A* will be equal to her expectation of the chance of *A*. As Lewis (1980) shows, PP entails that the function *Ch* will inherit the mathematical structure of *Cr*.

PP, by plugging propositions about chances into the apparatus of Bayesian confirmation theory and decision theory, tells us how we can go about confirming and disconfirming hypotheses about the value of chance. In particular, it will tell us how frequencies of outcomes bear on hypotheses about chance. Thus *PP* can be thought of as a Bayesian formulation of the evidential connection between chances and frequencies. It also tells us how to use our knowledge of chances to make decisions. So *PP* provides an account of chance by delineating the role of beliefs about chances in our conceptual economy.

By substituting propensity for chance in PP (or a similar principle), we could establish a connection between propensities and credences that would allow propensities to inherit the mathematical structure of credences. But a couple of puzzles remain. One is how the distinctively causal considerations that figure in the propensity theory play a role in connecting propensities with credences. A second, already hinted at, is how to identify those propositions that are admissible and inadmissible. We will develop these questions in more detail in the next two sections.

7. Credence and Cause

The Principal Principle tells us that:

$$Cr(A \mid Ch(A) = r \wedge E) = r$$

Can PP tell us anything about the causal basis of $Ch(A)$? I think it can. PP is really telling us two different things about the way in which a rational agent's beliefs about $Ch(A)$ affect her degrees of belief. One is that the agent will set her degrees of belief in accordance with what she takes the chances to be. But PP also tells us that for a rational agent, propositions of the form $Ch(A) = r$ *screen off A* from admissible propositions E. I want to suggest that it is in this screening-off role that the action really lies.

Consider a meteorologist, who must issue a prediction as to whether it will rain. The meteorologist does not encounter a world full of dials, corresponding to mathematical functions *Ch*, *Ch'*, *Ch"*, etc., and face the challenge of determining which dial points to the *chance* of rain. Rather, the meteorologist collects information about the causal antecedents of rain and shine—low pressure systems, humidity levels, masses of warm air, prevailing wind directions, and so on. Call the totality of her information of this kind C. The meteorologist then combines this information with her understanding of the theoretical principles (T) underlying the evolution of weather patterns. In light of this information, she believes to degree r that it will rain today (R). That is:

$$Cr(R \mid C \wedge T \wedge E) = r$$

The meteorologist need not find anything in the world with magnitude r. Nor need her theory T contain or entail explicit principles to the effect that whenever conditions C obtain, there is a chance r of rain. Rather, it is her coming to have degree of belief r in rain that *constitutes* her taking $C \wedge T$ to entail a chance r of rain. But this can't be all there is to the story. *Any* two propositions C and T will be such that when she conditions on $C \wedge T \wedge E$ she will arrive at some degree of belief that it will rain. But not just any propositions C and T will be taken to entail a chance of rain. What distinguishes the chance-setting propositions is that the agent's degrees of belief will be *resilient* (in the terminology of Skyrms (1980)). That is, $Cr(A \mid C \wedge T \wedge E) = r$ when she conditions on all manner of propositions E, so long as E is admissible. It is because the proposition $C \wedge T$ screens off R from all admissible propositions that $C \wedge T$ can be understood as setting the value of chance for the agent.

8. Admissibility

We still have not got very far. For any proposition $C \wedge T$ will screen off A from *some* range of propositions *E*. For example, many propositions of the form $C \wedge T$ will screen off *A* from propositions *E* that are irrelevant to *A*. As an illustration of what can go wrong, observe that finite frequencies obey a *PP*-like principal. Suppose, for example, that I know nothing about Pat except that Pat is a Briton. Thanks to his or her androgynous name, I don't even know Pat's sex. Suppose, moreover, that I know that 15.7% of Britons are aged 65 or older. Then it seems that I ought to assign credence .157 to the proposition that Pat is aged 65 or older.[4] But this is not the right kind of probability to be a chance. There is no experimental set-up that has a causal disposition to produce a 65-year old Pat. So not just anything that satisfies a *PP*-like principal is chance. What must be distinctive about chance, is the range of propositions *E* that count as admissible. For finite frequencies, all sorts of propositions will count as inadmissible. In the case of Pat, Pat's gender, where in Britain Pat lives, Pat's hobbies – whether Pat likes to knit and play bridge or bungee jump and listen to Coldplay – and so on will all be inadmissible. All of these things might affect my credence in the proposition that Pat is 65 or older, even given the information about finite frequencies.

Lewis never offers an explicit definition of admissibility, offering only illustrations and general guidelines. The general idea is that if *Ch* concerns the chances at time *t*, then any information about matters of fact at or before *t* is admissible (barring backward causation, crystal balls, and the like). Any factors at or before time *t* influence a rational agent's credence in proposition *A*, if at all, only via their influence on *Ch*(*A*). However, as we have seen in section 5, chances need not be manifested at one specific

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[4] Technically, this requires an assumption akin to exchangeability.

time. We will need, then, a more flexible criterion of admissibility. My proposal is to characterize admissibility in purely *causal* terms. The idea is that propositions E will be inadmissible if they concern factors that are causally downstream of the causal basis of chance.

9. Taking Stock

Let us take stock. We have two seemingly disparate approaches to chance: the propensity theory and the subjecitivist theory. And we have a collection of seemingly disparate problems for the two. More specifically, we are in search of a propensity theory of chance, where propensities are causal dispositions rooted in a set of causes, not necessarily all simultaneous. The mathematical structure of probability will be inherited from credences. Not all of the objective probabilities that can be defined will be identified with propensities, but the set of propensities will be rich enough to support a probabilistic representation. We need to specify what sorts of sets of causes suffice for determining chances, and we need to specify what kinds of propositions will be admissible in the formulation of the Principal Principle. My positive proposal will tie all of these loose ends together.

10. Causal Bayes Nets

My proposal will make use of Causal Bayes Nets, which are devices for representing a system of causal relations. We will say that a *causal structure* is an ordered pair $S = \langle \mathbf{V}, \mathbf{G} \rangle$ with the following elements:

V is a set of variables. If V is a variable in **V**, the values of V correspond to different factors, the sorts of things that can be causes, and outcomes of experimental set-ups. For example, a variable might represent the amount of force imparted to a coin, or the outcome of a coin toss.

G is a *directed acyclic graph* whose vertices correspond to the variables in **V**. Formally, **G** is a set of directed edges, or ordered pairs of distinct variables in **V**, $\langle V_1, V_2\rangle$. Directed edges are represented pictorially as arrows. If $\langle V_1, V_2\rangle \in \mathbf{G}$, then V_1 is said to be a *parent* of V_2, and **PA**(V) denotes the set of all the parents of V. A variable that has no parents is *exogenous*. A set of directed edges $\{\langle V_1, V_2\rangle, \langle V_2, V_3\rangle, \ldots, \langle V_{n-2}, V_{n-1}\rangle, \langle V_{n-1}, V_n\rangle\} \subseteq \mathbf{G}$ is called a *directed path* from V_1 to V_n. Pictorially, a directed path is a series of arrows aligned tip to tail. If such a directed path exists, V_n is said to be a *descendant* of V_1 (it will be convenient to extend this definition so that each variable is also a descendant of itself), and **DE**(V) is the set of all descendants of V; analogously, we will say that V_1 is an *ancestor* of V_n. A directed graph **G** is *acyclic* just in case there is no directed path from any variable to itself. The graph **G** represents the causal relationships among the variables in **V**. An arrow from V_1 to V_2 represents a causal influence of V_1 on V_2 that is not mediated by any of the other variables in **V**. If there is such an influence, we will say that V_1 is a *direct cause* of V_2 relative to **V**.

A *causal Bayes net* is a causal structure together with a probability function that satisfies the *Markov Condition*. Formally, a causal Bayes net is an ordered triple $\mathcal{N} = \langle \mathbf{V}, \mathbf{G}, Pr\rangle$, such that $\langle \mathbf{V}, \mathbf{G}\rangle$ is a causal structure, and Pr is a probability function defined over the variables in **V**. Specifically, we can define a formal language $\mathcal{L}_{\mathrm{V}}$ such that for every variable V in **V**, and every value v in the range of V, the proposition $V = v$ is in $\mathcal{L}_{\mathrm{V}}$, and $\mathcal{L}_V$ is closed under conjunction, disjunction, and negation.[5] *Pr* is then a probability function defined on the language $\mathcal{L}_V$. To keep the mathematics simple, we will assume that all the variables in **V** are discrete, and that *Pr* is regular (assigning 0 only to contradictions). In a Causal Bayes Net, the probability function *Pr* must satisfy a special constraint, called the *Markov Condition* (MC):

> For all $V \in \mathbf{V}$, all $\mathbf{W} \subseteq \mathbf{V}\backslash\mathbf{DE}(V)$; all v, in the range of V, all $\mathbf{w}$ in the range of $\mathbf{W}$; and all $\mathbf{p}$ in the range of $\mathbf{PA}(V)$:[6]
>
> $Pr(V = v \mid \mathbf{PA}(V) = \mathbf{p} \wedge \mathbf{W} = \mathbf{w}) = Pr(V = v \mid \mathbf{PA}(V) = \mathbf{p})$

In words, the values of $\mathbf{PA}(V)$ screen off the values of V from the values of any other variables, except for the descendants of V.

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[5] If the variables in **V** are continuous, then we also want to include in $\mathcal{L}_V$ propositions of the form $V \in (a, b)$ and close $\mathcal{L}_V$ under countable conjunctions and disjunctions.

[6] As a shorthand we will use vector notation to represent a conjunction of propositions about the values of variables. For instance, if $\langle V_1, V_2, \ldots, V_n \rangle$ is an ordered n-tuple or vector of variables, and $\langle v_1, v_2, \ldots, v_n \rangle$ is a vector of values, with each v_i in the range of V_i, we will write $\langle V_1, V_2, \ldots, V_n \rangle = \langle v_1, v_2, \ldots, v_n \rangle$ as shorthand for $V_1 = v_1 \wedge \ldots \wedge V_n = v_n$. Names of vectors appear in boldface, so for example we might write **V** for $\langle V_1, V_2, \ldots, V_n \rangle$ and **v** for $\langle v_1, v_2, \ldots, v_n \rangle$.

One important mathematical fact about causal Bayes nets is that the probability function *Pr* will satisfy the MC if and only if *Pr* factorizes in a particular way, namely:

$$Pr(V_1 = v_1 \wedge \ldots \wedge V_n = v_n) = \Pi_{i = 1,..,n}\, Pr(V_i = v_i \mid \mathbf{PA}(V_i) = \mathbf{p_i})$$

where $\mathbf{V} = \{V_1, \ldots, V_n\}$ and the vector $\mathbf{p_i}$ assigns to each variable V_j in $\mathbf{PA}(V_i)$ the value v_j. In other words, the complete joint probability distribution is determined by the conditional probabilities of the variables given their parents.

Causal Bayes nets have been widely and successfully used in the representation of causal systems, in the inference of causal relations from probabilistic correlations, and in the identification of causal magnitudes. (See for example, Pearl (2000) and Spirtes, Glymour and Scheines (2000).) The MC will not hold for certain quantum systems, and it will not hold for certain inappropriate variable sets **V**; for example, variable sets with omitted common causes, variables that are too coarse-grained, or variables that stand in logical relations to one another. In the latter cases, the MC is typically found to hold for macroscopic systems once we move to a more appropriate system of variables. Both Pearl (2000) and Spirtes, Glymour and Scheines (2000) formulate versions of a principle they call the *Causal Markov Condition*, which asserts that the MC holds for 'nice' variable sets (where 'niceness' entails, inter alia, freedom from the sorts of defects just canvassed). In what follows, I will assume that we are working with 'nice' variable sets, and that the MC holds of them.

11. A Preliminary Proposal

Let $\mathcal{N} = \langle \mathbf{V}, \mathbf{G}, Pr \rangle$ be a Causal Bayes Net that accurately represents some causal system. My preliminary proposal is that probabilities of the form:

$$Pr(V = v \mid \mathbf{PA}(V) = \mathbf{p})$$

correspond to propensities. $\mathbf{PA}(V) = \mathbf{p}$ is a suitable causal basis that gives rise to a certain propensity for $V = v$ to occur. Because of the factorization property, the conditional probabilities of this form suffice to fix the values of all the probabilities. Moreover, if a rational agent adopts credences that are in agreement with *Pr*, the screening off relations required in the definition of the Markov condition will be exactly those required by the Principal Principle.

If our goal were to produce a probabilistic theory of propensities, we would be nearly done (we might still want to add the material in section 14 below). That is, if we were given the objective probability function *Pr*, together with the causal structure $\langle \mathbf{V}, \mathbf{G} \rangle$, we could define propensities in the manner described. For example, Salmon (1984) maintains that probabilities cannot be interpreted in terms of propensities (citing the kinds of worries discussed in section 4 above), and opts instead for an interpretation in terms of limiting relative frequencies. Nonetheless, he maintains that 'propensity' is a useful concept for thinking about causation and probability. Someone coming at these issues from this sort of perspective could readily adopt the proposed definition of propensity. However, if we want to provide an interpretation of probability using propensities, we cannot take as given the probability distribution *Pr* that figures in the definition of a causal Bayes net. Rather, we will have to construct this distribution from the ground up.

12. From Propensities to Probabilities

Let us now re-formulate our preliminary solution, this time taking pains to do things in the appropriate logical order. Instead of taking a causal Bayes net $\mathcal{N} = \langle \mathbf{V}, \mathbf{G}, Pr \rangle$ as given, we will begin with a *propensity structure* and build the objective probability function *Pr* out of the causal propensities, via credences.

A propensity structure is an ordered triple $\mathcal{P} = \langle \mathbf{V}, \mathbf{G}, Prop \rangle$, where **V** is a 'nice' set of variables, and **G** is a directed acyclic graph on **V** that represents the causal relations among the variables in **V**. The language $\mathcal{L}_V$ is defined as above. For every variable $V \in \mathbf{V}$, $\mathbf{PA}(V)$ is a set of causes of V, such that any realization of these causes $\mathbf{PA}(V) = \mathbf{p}$ gives rise to a certain disposition or propensity yield the result $V = v$. Technically, *Prop* is a partial function on $\mathcal{L}_V \times \mathcal{L}_V$. $Prop(A, B)$ has a value when: (i) B is of the form $V = v$; (ii) if V is endogenous, then A is of the form $\mathbf{PA}(V) = \mathbf{p}$; and (iii) if V is exogenous, then A is a tautology**.** In section 14 below, we will extend the function to other arguments as well. $Prop(\mathbf{PA}(V) = \mathbf{p}, V = v)$ is the propensity of the system in state $\mathbf{PA}(V) = \mathbf{p}$ to yield the outcome $V = v$. The condition denoted by $\mathbf{PA}(V) = \mathbf{p}$ will be the *causal basis* for this propensity**.** (If V has no parents, then the causal basis will be denoted by any tautology $\top$. We will say more about exogenous variables in section 15 below.)

So far, there is nothing in our definitions that requires *Prop* to have anything like the structure of a probability distribution. Our next step is to calibrate the values of *Prop* via an analog of the PP. Let $\mathcal{P} = \langle \mathbf{V}, \mathbf{G}, Prop \rangle$ be a propensity structure. Let P be the proposition that $\mathcal{P}$ is an 'appropriate' propensity structure for representing some causal system. This requires, at a minimum, that **G** correctly represent the causal relations among the variables in **V**. If we are to be robust realists about propensities, then this will also require that *Prop* assign correct values to the propensities. But at this stage, I wish to remain open-minded about the metaphysics, hence the use of the hedging term 'appropriate'. Let *Cr* be the credences of an idealized rational agent defined on an extension of the language $\mathcal{L}_{\mathrm{V}}$ that includes *P*. Then we can state a version of the PP for propensities. For any variable $V \in \mathbf{V}$:

PP* $Cr(V = v \mid \mathbf{PA}(V) = \mathbf{p} \wedge P \wedge E) = r = Prop(\mathbf{PA}(V) = \mathbf{p}, V = v)$

for all admissible propositions *E.*

E is admissible just in case *E* is a Boolean combination of propositions of the form $\mathbf{W} = \mathbf{w}$, where $\mathbf{W} \subseteq \mathbf{V} \backslash \mathbf{DE}(V)$ (including tautologies, but excluding contradictions).

We make two technical observations:

(i) *E* will be admissible if *E* is a tautology, hence PP* entails that $Cr(V = v \mid \mathbf{PA}(V) = \mathbf{p} \wedge P) = Prop(\mathbf{PA}(V) = \mathbf{p}, V = v)$.

(ii) PP* entails that $\Sigma_v\, Prop(\mathbf{PA}(V) = \mathbf{p}, V = v) = 1$, but PP* otherwise imposes no constraints on the values of *Prop*. In particular, the values of $Prop(\mathbf{PA}(V) = \mathbf{p}, V = v)$ do not constrain the values of $Prop(\mathbf{PA}(V) = \mathbf{p}', V = v)$ of the values of $Prop(\mathbf{PA}(V') = \mathbf{p}'', V' = v')$.

In PP* the two propositions **PA**(V) = **p** and P together play the role of the proposition $Ch(V = v) = r$ in Lewis's version of PP. The state of the parents of V, together with values assigned by *Prop*, fix the chance of $V = v$. Unlike Lewis, however, I have given a strict causal interpretation of admissibility. Intuitively, E will be admissible if it contains information about goings-on that are not causally downstream of the causal basis **PA**(V) = **p** via V. In other words, inadmissible propositions give us information about how the chance set-up leading to V actually unfolded. This seems very much in the spirit of what Lewis had in mind. I interpret PP* as being constitutive of what it is to believe a propensity structure to be appropriate: to regard a structure as appropriate, is to allow one's credences to be guided by it.[7] We will consider further potential justifications of PP* in section 16 below.

Now let $Pr(\bullet) = Cr(\bullet \mid P)$. Since Cr has the structure of a probability function, Pr will as well. Moreover, it follows directly from PP* that for all $V \in \mathbf{V}$, $\mathbf{W} \subseteq \mathbf{V} \backslash \mathbf{DE}(V)$:

$$Pr(V = v \mid \mathbf{PA}(V) = \mathbf{p} \wedge \mathbf{W} = \mathbf{w}) = Pr(V = v \mid \mathbf{PA}(V) = \mathbf{p})$$

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[7] In particular, I interpret the proposition P in such a way that if an agent has already conditioned on information about the values of the variables in **V** in such a way that his credences in the values of the exogenous variables differs from those required by PP* -- that is, if they differ from the values assigned by *Prop*, or if they are not independent – then the agent does not take $\mathcal{P}$ to be an appropriate propensity structure. Thus we do not need to worry that the equality described in technical observation (i), which is a corollary of PP*, will fail because Cr already incorporates inadmissible information.

$= Prop(\mathbf{PA}(V) = \mathbf{p}, V = v)$.

But this is just to say that *Pr* satisfies the MC with respect to the causal structure ⟨**V**, **G**⟩. It follows that *Pr* will factorize in the usual way, namely:

$$Pr(V_1 = v_1 \wedge \ldots \wedge V_n = v_n) = \Pi_{i = 1,.., n}\, Pr(V_i = v_i \mid \mathbf{PA}(V_i) = \mathbf{p_i})$$

$$= \Pi_{i = 1,.., n}\, Prop(\mathbf{PA}(V_i) = \mathbf{p}_i, V_i = v_i)$$

where $\mathbf{p_i}$ is a vector in which each variable V_j in $\mathbf{PA}(V_i)$ has the value v_j.

So we can start with a propensity structure ⟨**V**, **G**, *Prop*⟩*,* and if PP* is satisfied, we can construct *Pr* directly as follows:

$$Pr(V_1 = v_1 \wedge \ldots \wedge V_n = v_n) = \Pi_{i = 1,.., n}\, Prop(\mathbf{PA}(V_i) = \mathbf{p}_i, V_i = v_i).$$

Now ⟨**V**, **G**, *Pr*⟩ is a causal Bayes net in which $Pr(V = v \mid \mathbf{PA}(V) = \mathbf{p}) = Prop(\mathbf{PA}(V) = \mathbf{p}, V = v)$. In other words, probabilities of the form $Pr(V = v \mid \mathbf{PA}(V) = \mathbf{p})$ are directly interpreted as propensities, while other probabilities are constructed out of propensities.

We will want to add some refinements, but let us first review how this proposal addresses the problems of sections 4 through 8.

13. Puzzles Resolved

I. *Humphreys' Paradox.* Probabilities of the form $Pr(V = v \mid \mathbf{PA}(V) = \mathbf{p})$ correspond to propensities. These certainly have the right causal direction to be propensities: they are the probabilities conferred upon values of V by the values of the direct causes of V in the causal model. But what about other conditional probabilities? All conditional probabilities, including the troublesome backward conditional probabilities, are uniquely determined by the unconditional probabilities assigned by *Pr*. And these, in turn, are uniquely determined by the values of the propensities, according to the factorization $Pr(V_1 = v_1 \wedge \ldots \wedge V_n = v_n) = \Pi_{i = 1,..,n} Prop(\mathbf{PA}(V_i) = \mathbf{p}_i, V_i = v_i)$. This factorization gives us the desired representation theorem: the values of the propensities suffice to fix all of the conditional and unconditional probabilities.

II. *The Causal Basis.* In probabilities of the form $Pr(V = v \mid \mathbf{PA}(V) = \mathbf{p})$, the proposition $\mathbf{PA}(V) = \mathbf{p}$ characterizes the causal basis for the chance of $V = v$. The definition of $\mathbf{PA}(V)$ requires that each variable in $\mathbf{PA}(V)$ be a direct cause of V relative to the variable set $\mathbf{V}$. Nothing in this definition requires that all of these variables pertain to goings-on at the same time. Moreover, in section 14 below, we will be able to liberalize even further the types of sets of causes that can serve as the causal basis for chances.

Note also that our proposal is compatible with non-extreme chances even if determinism is true. Our causal Bayes net $\mathcal{N}$ need not include a complete set of causes, nor is there any need for the probabilities of the form $Pr(V = v \mid \mathbf{PA}(V) = \mathbf{p})$ to be irreducible probabilities. Let us suppose that the value of each variable V_i in $\mathbf{V}$ is deterministically caused by the value $\mathbf{p_i}$ of $\mathbf{PA}(V)$ together with the values $\mathbf{u_i}$ of some set $\mathbf{U_i}$ of variables that are excluded from the model, perhaps because they are unknown or unobservable. There is a probability distribution Pr' on the values of the $\mathbf{U_i}$'s (perhaps representing our ignorance of the values of the unknown causes), which induces a probability distribution *Pr* on $\mathcal{L}_V$. Then *Pr* will satisfy the MC if the $\mathbf{U_i}$'s are probabilistically independent in *Pr′* (Pearl and Verma 1991). Thus our causal model will be 'complete enough' if the omitted causes are independent of one another.

III. *The Problem of Structure.* Our version of the Principal Principle, PP*, connects propensities with the degrees of belief of an idealized rational agent. There are independent arguments that such degrees of belief have the mathematical structure of probabilities. PP* guarantees that the function *Pr*, constructed from the propensities, inherits this structure.

IV. *Credence and Cause*. In our version of the Principal Principle, the rational agent's degrees of belief are not fixed directly by beliefs about the chances, but indirectly by beliefs about the causal antecedents of some event, together with beliefs about which propensity structures appropriately represent a causal system. What is distinctive about fixing degrees of belief in this way is that such degrees of belief are highly resilient. Admissible information about the values of variables that are not causally downstream from the causal basis can not affect this degree of belief.

V. *Admissibility*. We have given a precise and rigorous definition of admissibility in terms of causal structure. We will be able to give a more liberal (but equally precise and rigorous) definition in the next section.

We turn our attention now to a collection of loose ends.

14. Liberalizing Propensities

Consider the causal structure shown in figure 1. Suppose that the measure *Pr* is defined on this structure, and satisfies the MC. As defined above, conditional probabilities of the form $Pr(V = v \mid X_2 = x_2 \wedge Y_2 = y_2 \wedge Z_2 = z_2)$ can be directly interpreted as causal propensities. But it seems intuitively that any conditional probability of the form $Pr(V = v \mid X_i = x_i \wedge Y_j = y_j \wedge Z_k = z_k)$, i, j, k = 1, 2 should count as a propensity. Any conditioning set of this form seems like a sufficiently complete set of causes to confer a propensity upon $V = v$. With the conditioning set $\{X_1, Y_1, Z_1\}$, conditioning on information about the values of X_2, Y_2, Z_2, or W will affect the probability that V takes a particular information. But this seems alright, as this is information about how the chance set-ups evolve toward V from the selected causal basis. On the other hand, the set $\{Y_2, Z_2\}$ is not sufficient to give us a causal basis, since information about X_1, X_2 or W will give us further information about V, and these are not causally downstream of Y_2, or Z_2.

The guiding idea is that a set of causes is sufficient to ground a chance if it gives rise to a version of the PP with the right kind of admissibility relations. I suggest the following, more liberal definition of admissibility. Let $\langle \mathbf{V}, \mathbf{G} \rangle$ be a causal structure, and let $\mathbf{U} \subseteq \mathbf{V}$ be a candidate causal basis for $V \in \mathbf{V}$. We will say that proposition E is *generally admissible* in $\langle \mathbf{V}, \mathbf{G} \rangle$ with respect to $\mathbf{U}$ and V just in case E is a Boolean combination (including tautologies, but excluding contradictions) of propositions of the form $W = w$, where $W \in \mathbf{V}$, and

> W is not a descendant of any variable $X \in \mathbf{V}$ such that:
>
> (i) X is not in $\mathbf{U}$, and
>
> (ii) X lies on a directed path from a variable in $\mathbf{U}$ to V.

Note that V itself is excluded, unless $V \in \mathbf{U}$. The idea is that a proposition is inadmissible if it gives information about the outcomes of chance processes proceeding from $\mathbf{U}$ to V.

Let us temporarily get ahead of ourselves, and assume that we already have our causal Bayes net $\langle \mathbf{V}, \mathbf{G}, Pr \rangle$. Then we will want to say that $\mathbf{U} = \mathbf{u}$ confers a propensity upon $V = v$, just in case:

$$Pr(V = v \mid \mathbf{U} = \mathbf{u} \wedge E) = Pr(V = v \mid \mathbf{U} = \mathbf{u})$$

when E is admissible relative to $\mathbf{U}$ and V. The set $\mathbf{U}$ will be called an *appropriate conditioning set* for V if it meets this condition.

Here is one fairly simple condition, expressible in terms of the causal structure $\langle \mathbf{V}, \mathbf{G} \rangle$ alone, that is sufficient for a set of variables $\mathbf{U}$ to be an appropriate conditioning set for V:

Sufficient condition I.

1. Every directed path from an exogenous variable to V includes at least one variable in $\mathbf{U}$;
2. If $U \in \mathbf{U}$ and $U \in \mathbf{DE}(X)$ then $X \in \mathbf{U}$ (i.e., $\mathbf{U}$ is closed under ancestry).

Sets satisfying these two conditions will often contain more variables than strictly necessary. For example, in figure 1, the set $\mathbf{U} = \{X_2, Y_2, Z_2\}$ does not meet condition 2; we must also add X_1, Y_1, and Z_1. Here is a slightly less intuitive sufficient condition:

Sufficient condition II.

1. Every directed path from an exogenous variable to V includes at least one variable in $\mathbf{U}$;

2. If any descendant of V is in $\mathbf{U}$, then V is in $\mathbf{U}$;

3. If $U \in \mathbf{U}$ and $U \in \mathbf{DE}(X)$, then every directed path from X to V has at least one variable that is in $\mathbf{U}$.

The reader can verify that in figure 1, any set of the form $\{X_i, Y_j, Z_k)$, i, j, k = 1, 2 satisfies this condition. The proof that these two conditions are sufficient, and further discussion, are contained in the appendix. No doubt there are weaker sets of sufficient conditions, but sufficient condition II will suffice for the present.

We can then extend our definition of a propensity structure so that the function *Prop* is defined on this broader class of causal bases. That is, a propensity structure is triple $\mathcal{P} = \langle \mathbf{V}, \mathbf{G}, Prop\rangle$, where (a) $\langle \mathbf{V}, \mathbf{G}\rangle$ is a causal structure; and (b) *Prop* is a partial function on $\mathcal{L}_\mathrm{V} \times \mathcal{L}_\mathrm{V}$ such that $Prop(A, B)$ is defined when: (i) B is of the form $V = v$; (ii) if V is endogenous, then A is of the form $\mathbf{U} = \mathbf{u}$, where $\mathbf{U}$ satisfies sufficient condition II; and (iii) if V is exogenous, then A is a tautology; and finally, $\langle \mathbf{V}, \mathbf{G}, Prop\rangle$ is consistent with with an appropriately emended version of PP:

PP** For any variable $V \in \mathbf{V}$, if $\mathbf{U}$ satisfies sufficient condition II with respect to V, then:

$Cr(V = v \mid \mathbf{U} = \mathbf{u} \wedge P \wedge E) = r = Prop(\mathbf{U} = \mathbf{u}, V = v)$

for any proposition E that is generally admissible with respect to $\mathbf{U}$ and V.

PP* follows as a special case, where $\mathbf{U} = \mathbf{PA}(V)$. Thus, if *Pr* is constructed from *Prop* as before, PP** entails that *Pr* will satisfy the MC, and $Pr(V = v \mid \mathbf{U} = \mathbf{u}) = Prop(\mathbf{U} = \mathbf{u}, V = v)$ whenever the latter is defined.

Note that PP** imposes far more constraints on the values of *Prop* than PP*. Suppose, for example, that we have a simple causal model in which X is a direct cause of Y, which in turn is a direct cause of Z. Then propensities of the form $Prop(X = x, Z = z)$ will be uniquely determined by propensities of the form $Prop(X = x, Y = y)$ and $Prop(Y = y, Z = z)$.

15. Exogenous Variables

We have suggested that probabilities of the form $Pr(V = v \mid \mathbf{PA}(V) = \mathbf{p})$ can naturally be interpreted as propensities. This appears to be problematic, however, in the case of exogenous variables. Recall that for a causal graph **G** over a variable set **V**, a variable is exogenous just in case it has no parents in **G**. But then, when we construct our objective probability measure *Pr* via the formula $Pr(V_1 = v_1 \wedge \ldots \wedge V_n = v_n) = \Pi_{i = 1,.., n} Prop(\mathbf{PA}(V_i) = \mathbf{p}_i, V_i = v_i)$, how can we understand the terms of the form $Prop(\top, V_i = v_i)$ when V_i is an exogenous variable? What grounds the propensity? If this is not a propensity, where does $Pr(V_i = v_i)$ come from?

I do not think that there is one uniform solution to this problem, but rather that there are a variety of possible cases.

(i) Typically, we do not model the entire causal structure of the world at once, but rather do it piecemeal. Thus a variable that is exogenous in one causal model may well be endogenous in another. Thus if V is an exogenous variable in one causal model, it may be that $Pr(V = v)$ can be recovered from another model where V appears as an endogenous variable, and can be constructed from propensities in that model.

(ii) Some variables will be the sort over which we have direct control. This may be the case for the parameters of certain kinds of chance set-ups that we create. For a very simple example, an exogenous variable may represent whether a particular coin is flipped, or a particular die is rolled. Here we can set the probability of the relevant value of the variable equal to 1.

(iii) In some chance set-ups, there may be variables whose probability distribution makes very little difference to the probability of further variables. The approach to deterministic games of chance proposed originally by Poincaré (1896) and developed in Suppes (1987), and Strevens (2003) fit this description. Suppose, for example, that we are modeling a coin toss. In addition to the constitution of the coin (which is fixed and so has probability 1), let us assume that there are three parameters that determine the outcome of the coin toss: the upward force imparted to the coin, the torque applied to the coin, and its height above the surface where it will land. The precise values of these parameters will determine whether the coin will land heads or tails. However, if regions of the three parameter state space that lead to heads are very close to those that lead to tails, and if the proportion of the volume of largish regions of the state space occupied by states leading to heads is roughly the same in different parts of the state space, then any probability distribution over the state space that is reasonably smooth and spread out will yield approximately the same probability for heads. If we assume, for example, that the person flipping the coin can't exercise very fine control over the values of the parameters, that even if she deliberately tries to flip the coin exactly the same way on repeated trials, she produces a diverse range of initial conditions, then it may not matter precisely what probabilities we assign to those parameters. The consequences for the probabilities of later events will be essentially the same, no matter which distribution we choose.

(iv) Some variables may be such that we know nothing about what causes them to take particular values; or some may have so many diverse causes that no one cause or small handful of causes make much difference. Nonetheless, we may observe that the frequencies with which these variables take certain values are stable in certain kinds of system or causal structure. These variables will essentially appear to be random. There may well be causes that confer propensities upon values of the variable, but these will be excluded from our models. Here the probabilities for exogenous variables can be thought of as degenerate propensities: propensities whose causal basis is empty.

(v) For any exogenous variables that do not fall into one of these categories, it may simply turn out that the probabilistic representation is not uniquely determined by features of the world that can reasonably be interpreted as propensities. That is, we may have a class of probability functions Pr' that all agree on the values of conditional probabilities of the form $Pr'(V = v \mid \mathbf{PA}(V) = \mathbf{p})$ (or $Pr'(V = v \mid \mathbf{U} = \mathbf{u})$), which are determined by the propensities, but which differ in the unconditional probabilities they assign to the exogenous variables.

16. What Do the Numbers Mean?

In section 6, we noted that the question of where the mathematical structure of probability comes from has two parts. The second part concerns why propensities should have the mathematical structure of probabilities. The answer developed here is that they inherit this structure from the credences of a rational agent. The first part concerns the interpretation of the numbers themselves. What feature of the world determines that the propensity that the causal basis $\mathbf{U} = \mathbf{u}$ confers upon $V = v$ is equal to r, rather than to r'? PP* (or PP**) by itself does not resolve this question. Suppose, for example, that there are two propensity structures, $\mathcal{P} = \langle \mathbf{V}, \mathbf{G}, \mathit{Prop} \rangle$ and $\mathcal{P}' = \langle \mathbf{V}, \mathbf{G}, \mathit{Prop}' \rangle$. One agent, with credence Cr, takes $\mathcal{P}$ to be an appropriate representation, while another, with credence Cr', takes $\mathcal{P}'$ to be an appropriate representation. Assuming that neither is conditioning on any inadmissible information, we might have $Cr(V = v \mid \mathbf{U} = \mathbf{u}) \neq Cr'(V = v \mid \mathbf{U} = \mathbf{u})$, where $\mathbf{U} = \mathbf{u}$ is a suitable causal basis for $V = v$. Which value, if either, is the actual propensity of $\mathbf{U} = \mathbf{u}$ to yield $V = v$? What makes one value, rather than another, correct? PP* (PP**) tells us what it is for an agent to *believe* that a propensity structure is an appropriate representation, but it does not tell us what it is for a propensity structure to *be* an appropriate representation. It seems clear that a propensity interpretation of chance must ultimately answer this question if it is to supply the meaning of statements about chance.

I will not offer a definitive answer to this question. One reason is that I intend the approach developed here, and in particular, the solution to the problems raised in sections 4 through 8, to be compatible with a number of different views about the metaphysics of chance. A second reason is that I suspect that the correct answer to this question will vary depending upon the nature of the causal system being investigated. One desideratum of an answer to this question is that it be able to make sense of, or even justify, PP* (or PP**). In this section, I will canvass a variety of potential answers to this question. Some of these options will already be familiar. Others, I think, are attractive options that have been under-developed.

(i) One option is to be a robust realist about propensities. Propensities really exist in the world as irreducible causal magnitudes, and a propensity structure is an appropriate representation if it accurately specifies the value of the those propensities. On this view, PP* might be seen as a kind of external epistemic norm. By analogy, consider the case of simple beliefs. These are subject to both internal and external epistemic norms. Consistency is an internal norm – it is a feature intrinsic to a body of beliefs, independent of any way the world might be. By contrast, truth is an external norm: it is better (epistemically) to have true beliefs than false beliefs. We might tell a similar story for degrees of belief. Probabilistic coherence would be an internal epistemic norm. Hájek (MS) has argued that conformity with actual chances is an external norm for degrees of belief analogous to the norm of truth for simple beliefs. The existence of such a norm would present a simple explanation for why PP* holds: an agent seeking to conform to this norm would naturally set her degrees of belief in accord with what she takes the chances to be.[8]

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[8] Although it could be problematic if the agent has positive credences for different values of chance. Suppose, for example, that an agent knows that of the two coins in front of her, one is biased 2-to-1 for heads and the other is biased 2-to-1 for tails. She picks one at random, and is about to toss it. Her credence is equally split between two possible propensity structures. The result is that her credence that the coin will land heads is .5. But this seems like the wrong thing to do from the perspective of the external norm, since

One drawback with this approach is that it would undermine the solution to the second problem about mathematical structure. If propensities have the structure they do independently of the beliefs of agents, then the question remains as to why we should expect them to have the structure of probabilities. Of course, if they do not have the structure of probabilities, then the norm of conforming one's degrees of belief to propensities becomes highly problematic. But it seems to me that this is a genuine problem for those who maintain that there is such a norm, rather than a reason for thinking that propensities are probabilities. This problem is even more acute if one wants to liberalize propensities along the lines suggested in section 14 above. As we have seen, PP* imposes relatively minor constraints upon the values of propensities. Perhaps the robust realist about propensities could interpret PP* as a means for imposing a numerical scale upon propensities.[9] By contrast, PP** imposes more severe consistency constraints upon the values of propensities. If propensities really are just naturally occurring magnitudes, it would remain to be explained why we should expect them to satisfy these consistency constraints.

(ii) A second approach is that adopted (at least some of the time) by David Lewis. Lewis offers a modified frequency theory of chance. Chances are determined by the 'best system' of laws, where some laws may involve chance. The best system is the one that achieves the optimum trade-off between fit with actual frequencies (as measured by

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the agent knows that the objective chance of heads is not .5.

[9] Although if propensities naturally lie on a ratio scale, and are unbounded, it is not obvious how one ought to re-scale them so that they will fit on a [0, 1] scale.

likelihood),[10] simplicity, and strength or informativeness. This approach seems reasonably attractive for the kinds of probabilities that are derived from mathematical theories such as quantum mechanics. Quantum mechanics provides a unified theory that accounts for a great many natural phenomena. Although the very itself is, arguably, not very simple, the probabilities for measurement outcomes are derived from the theory in a natural and mathematically elegant way (discussed in section 6 above). And, finally, these probability values have fit very well with the frequency data in repeated experimental tests. So we have, here, a theory that scores highly along each of Lewis's three criteria. In particular, the theory is sufficiently powerful and elegant, that it seems reasonable to prefer it over any *ad hoc* theory we might construct to yield a slightly better fit with frequency data. On the other hand, it is much harder to see how one might apply this apparatus in other domains, to yield values for the chance of rain tomorrow, the chance that a coin will land heads when tossed, or the chance that Shaquille O'Neal will make his next free throw.

A challenge for the best systems approach is to explain why chances should be constrained by anything like PP. Lewis claims to be able to "see, dimly, how it might be rational to conform [one's] credences about outcomes to [one's] credences about history, symmetries, and frequencies" (Lewis 1986, p. xv). I can see at least two different prospects for accounting for this rationality. One would involve an appeal to general principles of scientific methodology; roughly, it is constitutive of scientific rationality to

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[10] This entails that deterministic laws have a fit of zero unless they are true, which presumably disqualifies false deterministic laws.

invest one's credence in those theories that best fit the available data, and that satisfy other desiderata such as simplicity and informativeness. A second would be a modification of the argument offered by Howson and Urbach (2006) that a Von Mises-style frequentism entails a version of PP. The argument is roughly as follows: To set one's credence in *A* to *r* is to find fair a bet on *A* that cost *r* units and paid one unit if *A* obtains. If, in a long sequence of identical trials, the frequency with which *A* occurs is equal to *f*, then the only bet that would break even over this sequence of trials would be a bet on (or against) *A* with cost *f* and unit payout. Hence if one knows that the relative frequency will be *f*, one must set one's credence to *f*. Hoefer (2007) suggests a modified version of this argument to suggest that the edicts of the 'best system' of chances ought to guide credences in a similar way. In both approaches, however, it is hard to see exactly how simplicity, in particular, is supposed to play a role in guiding one's credences.

(iii) For some kinds of mechanical chance set-ups – coin tosses, roulette wheels, and the like – the strategy discussed in part (iii) of the previous section will work well. The dynamics of these systems ensure that (for anyone who understands them) a broad range of credence functions over the initial conditions will give rise to nearly the same credence for the outcomes. In such cases, the dynamics make such credences highly appropriate, and these credences can naturally be interpreted as propensities.

(iv) Another approach -- one that has not, to my knowledge, been pursued in any detail -- is to develop an analogy with sentimentalist theories of meta-ethics (e.g. Gibbard 1990, d'Arms 2000). These views maintain, for example, that act *A* is wrong if it is appropriate or fitting to react to *A* with characteristic emotions such as anger or disgust. The parallel would be that a propensity structure in which $Prop(\mathbf{U} = \mathbf{u}, V = v) = r$ would be appropriate just in case it would be appropriate or fitting to have a degree of belief of *r* in the proposition $V = v$ upon learning that $\mathbf{U} = \mathbf{u}$. This proposal would have the advantage of establishing an immediate conceptual link between propensities and credences. PP* would essentially follow from the definition of an appropriate propensity structure. This would make it easy to explain why propensities have the structure of probabilities. In particular, the fittingness of a degree of belief of *r* need not be grounded in some feature of the world that itself has the magnitude *r*, and hence we need not be burdened with the puzzle of why this magnitude behaves in accordance with the laws of probability. As with sentimalist theories in meta-ethics, however, a question remains as to what would ground the appropriateness or fittingness of the response: what makes having a credence of *r* in the proposition $V = v$ is a fitting response to learning that $\mathbf{U} = \mathbf{u}$? One approach, simple if unsatisfying, is to say that facts about the fittingess of degrees of belief are basic.

(v) One might try to supplement the approach described in part (iv) by grounding the fittingness of certain degrees of belief in pragmatic considerations, in the spirit of the Dutch Book arguments for probabilism about degrees of belief. This approach is suggested by Mellor (1970). Imagine a situation in which you will be forced by an intelligent demon to bet on a repeated series of trials. Your degrees of belief will determine the betting odds, but the demon will determine which side of the bet you will take. The demon is fully informed about the physics underlying the chance set-up. The best that you can hope to do is break even (unless you are very lucky). The propensity for the outcome on which you will bet can then be identified with the degree of belief that you ought (prudentially) to have. This approach has the advantage of providing an account of the normative force of the 'ought', but it still does not help us identify the features of the world that make one degree of belief, rather than another, the normatively appropriate one to have.

(vi) A final possibility, quite attractive in my view, is a moderate anti-realism about propensities. We may make an analogy with the interpretation of probability as rational degrees of belief. Beliefs are real, and some can be stronger than others. As I write this sentence, my belief that it will be sunny tomorrow (an August day) in Southern California is stronger than my belief that it will rain. But it is an idealization to assign precise numbers and a mathematical structure to degrees of belief. Similarly, the causal basis underlying a given propensity is real, and there is a genuine propensity or tendency for that basis to produce various outcomes. Moreover, some such causal tendencies are stronger than others. But, we might maintain, it is an idealization to assign precise numerical magnitudes to these propensities. There are, however, facts about how well certain propensity structures fit with frequency data, and how well they serve as guides; these are articulated, within a Bayesian framework, by PP* (or PP**). This makes the idealized propensity models highly useful. This seems to be something like the view of De Finetti (1937) and van Fraassen (1980, chapter 6), although the latter would not even recognize the causal basis or the dispositions as real. This also seems to be the position of David Lewis in at least some moods, for example when he claims that the Principal Principle "capture[s] all we know about chance" (1980, cited from 1986, p. 86). Sober (Forthcoming) and Spohn (Forthcoming) also articulate theories of objective chance that might be catalogued as species of moderate anti-realism.

This approach fits well with the way in which probabilistic models are actually used to represent complex systems. It would also be a way of reconciling Lewis's program of Humean Supervenience with well-known problems for analyzing chance in terms of the Humean basis (described in Lewis 1994). The idea is that while chance itself could not be analyzed in purely Humean terms, this would not pose a threat to Humean supervenience, since numerical chances are not extra ingredients of the world. They are, rather, convenient fictions: features of the models that we use to represent the world.

17. Quantum Mechanics

Popper introduced the propensity interpretation to make sense of the probabilities that appear in quantum mechanics. Indeed, Popper went so far as to claim that propensities took the mystery out of quantum mechanics (1957, 1959). Most commentators believe that this is asking too much of the propensity theory. Nonetheless, quantum mechanics does seem to be one domain where a propensity theory of chance ought to apply.

This is problematic for the present account, which makes use of causal Bayes nets to represent causal systems. Certain quantum systems seem to involve violations of the Markov Condition that relates probabilities to causation. For example, suppose that we have a two particle system prepared in the singlet state:

$$\psi = (1/\sqrt{2})(|+\rangle_1|-\rangle_2 - |-\rangle_1|+\rangle_2)$$

If we measure the spin of particle 1 in any direction, we will find that there is a .5 chance that the result will be +, and a .5 chance that the result will be –. Similarly if we measure the spin of particle 2 in the same direction. However, if we measure the spin of both particles, we will always get the opposite results. Thus the two measurement outcomes are correlated. Writing $+_1$ to represent that the outcome of the measurement on particle 1 is +, and likewise for – and particle 2, we have:

$$Pr(+_1, -_2) = .5 > Pr(+_1)Pr(-_2) = .25$$

Moreover, in standard quantum mechanics, there is no common cause that screens the results off from one another. Nor does it seem that one outcome causes the other: indeed, we can conduct the measurements at spacelike separation and still obtain the correlation.

How are we to think about the propensities in this case? The problem is that the outcome of the measurement of particle 1 does not seem to belong to the causal basis for outcome of the measurement of particle 2: there is no causal relationship between the two. Yet information about the outcome of the measurement on particle 1 makes a difference to the probability of the outcome of the measurement on particle 2, even though it is the sort of thing that is supposed to be screened off by the causal basis. The solution to this will depend upon one's interpretation of quantum mechanics. It seems to me that there are at least three different possibilities, each one of which locates the quantum weirdness in a different place.

(i) In Bohm's (1952) interpretation of quantum mechanics, there is a hidden variable that uniquely determines measurement outcomes. Nevertheless, this variable is very *deeply* hidden. We can never succeed in discovering its value. Hence even though the theory is deterministic, the probabilities are in practice ineliminable. If ever there were a case for retaining non-extreme chances under determinism, this would be it. Bohm's interpretation is characterized by non-local causal influences. Relativity's ban on superluminal causal signals is interpreted instrumentally as a restriction on the kinds of signals that we can send. There is, in fact, a preferred simultaneity relation. On this interpretation, one measurement outcome really does cause the other: which ever one is performed first is the cause. Thus, either the outcome of the measurement on particle 1 is a cause of the outcome of the measurement on particle 2, in which case it is appropriate to include it in causal basis; or the outcome of the measurement on particle 2 is a cause of the outcome of the measurement on particle 1, in which case the latter outcome is just the sort of thing that is normally considered inadmissible. On this interpretation, the weirdness of quantum mechanics lies in its nonlocal causal influences.

(ii) A second approach would be to simply accept that the outcome of the measurement on particle 1 is part of the basis of the propensity for the outcome of the measurement on particle 2, even though it is not a cause. On this view, the weirdness of quantum mechanics lies in the fact that quantum propensities can have a noncausal basis. (Shimony (1983) suggested the colorful phrase 'passion at a distance' – in contrast to action at a distance – to describe this type of noncausal determination.)

(iii) A third approach, suggested by Hausman (1998) and Hausman and Woodward (1999), and one that I favor, is to deny that the two measurement outcomes are distinct events in the appropriate sense. By analogy, suppose we were to toss a coin, and construct a little model of the possible outcomes. If our model included two separate variables, one for 'heads' (taking values 0 or 1), and one for 'tails', we would run into trouble. We would find that these variables are correlated: 'heads' is 1 just in case 'tails' is 0. Yet neither is a cause of the other (they are simultaneous), and if the toss is genuinely chancy, the correlation is not screened off by the state of the common cause. The problem is that these two variables do not really represent distinct outcomes of the coin toss. There is, rather, only one outcome variable, whose values correspond to the two possible results of the coin toss. Returning to the quantum mechanical case, the two particles are in an entangled state. This means that one can't meaningfully talk about the state of particle 1 in isolation, or the state of particle 2 in isolation. So even though the two measurements are separated by a considerable distance, they are not really distinct. Rather, the two measurements together should be thought of as a joint measurement of the state of the two particle system. On this view, the causal basis is the state of the system (including the preparation of the particles in the singlet state), and the outcomes are the joint results of the two measurements. So on this view, quantum propensities have a normal causal basis. The weirdness of quantum mechanics on this view, lies in the fact that widely separated events can fail to be appropriately independent of one another.

In any event, these problems with distant correlation phenomena will not be unique to my account. They present problems for Lewis's PP, since the sort of information we normally consider to be admissible turns out to be inadmissible in these cases. And they present problems for any propensity theory, since the probability of a measurement outcome seems to be fixed by something other than its causes. Quantum mysteries are everyone's mysteries.

Appendix

In this appendix, we prove that the two sets described in section 14 have the appropriate screening off properties.

Theorem: Let $\langle \mathbf{V}, \mathbf{G}, Pr\rangle$ be a causal Bayes net. Let $V \in \mathbf{V}$. We will say that $\mathbf{U} \subseteq \mathbf{V}$ is an appropriate conditioning set for V just in case for all v in the range of V, $\mathbf{u}$ in the range of $\mathbf{U}$, $W \in \mathbf{V}$, and w in the range of W:

$$Pr(V = v \mid \mathbf{U} = \mathbf{u} \wedge E) = Pr(V = v \mid \mathbf{U} = \mathbf{u})$$

where E is a Boolean combination (including tautologies, but excluding contradictions) of propositions of the form $W = w$, where $W \in \mathbf{V}$, and

> W is not a descendant of any variable $X \in \mathbf{V}$ such that:
>
> (i) X is not in $\mathbf{U}$, and
>
> (ii) X lies on a directed path from a variable in $\mathbf{U}$ to V.

Then the following are sufficient conditions for $\mathbf{U}$ to be an appropriate conditioning set:

I. 1. Every directed path from an exogenous variable to V includes at least one variable in $\mathbf{U}$.

2. If $U \in \mathbf{U}$ and $U \in \mathbf{DE}(X)$ then $X \in \mathbf{U}$.

II. 1. Every directed path from an exogenous variable to V includes at least one variable in **U**.

2. If any descendant of V is in **U**, then V is in **U**.

3. If $U \in \mathbf{U}$ and $U \in \mathbf{DE}(X)$, then every directed path from X to V has at least one variable that is in **U**.

Proof: Our proof will make use of a fact about causal Bayes nets. A *path* in **G** is a sequence of variables in **V**, $\langle V_1, \ldots, V_k \rangle$, such that for any two consecutive variables in the sequence V_i, V_{i+1}, there is either an arrow from V_i to V_{i+1} or an arrow from V_{i+1} to V_i. Such a path is said to be a path from V_1 to V_k. A variable V_i on this path is said to be a *collider* just in case $i \neq 1, k$ and there are arrows from both V_{i-1} and V_{i+1} into V_i. Intuitively, V_i is a collider just in case the arrows converge on V_i in the path. For any two variables V, W in **V** and any subset **U** of **V**, we define the relation of *d-separation* as follows:

d-sep **U** d-separates V and W just in case every path $\langle V = V_1, \ldots, V_k = W \rangle$ from V to W contains at least one variable V_i such that either:

(i) V_i is a collider, and no descendant of V_i (including V_i itself) is in **U**; or

(ii) V_i is not a collider, and V_i is in **U**.

Let $\mathbf{W} \subseteq \mathbf{V}$. The MC entails that V and **W** are probabilistically independent conditional upon **U**, just in case for every $W \in \mathbf{W}$, **U** d-separates V and W in **G** (Pearl 1988). Now let E be a Boolean combination of propositions of the form $W = w$, where W meets the conditions described in the theorem. We will show that if **U** satisfies either condition I or condition II, then **U** d-separates V from all such W.

I. Suppose that **U**, satisfying I, does not d-separate W and V. Let $\langle W = V_1, \ldots, V_k =$

$V\rangle$ be a path from W to V satisfying neither condition (i) nor (ii) of d-sep. This path cannot contain a collider. For if V_i is a collider, then V_i must have a descendant in **U**. Then by property 2 of set **U**, V_{i-1}, V_i, V_{i+1} must all be in **U**. But V_{i-1} and V_{i+1} are not colliders, since they each have an arrow pointing to V_i. Therefore, the path contains a variable that is not a collider and is in **U**, contrary to the assumption that the path does not satisfy condition (ii) of d-sep. Since none of the variables on the path are colliders, none can be in **U**. This leave only 3 possibilities: either V is a descendant of W, W is a descendant of V, or they have a common cause on the path. If W is a descendant of V, then W is a descendant of a variable that is not in **U** and is on a directed path from **U** to V. In either of the other two cases, W is a descendant of a variable X (perhaps W itself), such that there is a directed path from X to V that does not contain any member of **U**. By property 1 of I, X must be a descendant of a variable in **U**. *QED*.

Note that condition 2, while unintuitive, is needed. In figure 2, the set $\{U_1, U_2\}$ satisfies 1 but not 2 of I. Given the values of the variables in the set, the value of W will give information about the value of V, since W and X are correlated conditional upon the value of U_1.

II. Suppose that **U**, satisfying II, does not d-separate W and V. Let $\langle W = V_1, \ldots, V_k = V\rangle$ be a path from W to V satisfying neither condition (i) nor (ii) of d-sep. If $V \in$ **U**, then **U** d-separates W and V, for V is not a collider on the path from W to V. Hence, by property 2 of II, no descendant of V can be in **U**. We now show that the path $\langle W = V_1, \ldots, V_k = V\rangle$ can have no colliders. For suppose that V_i is the collider on this path that is closest to V. Then either V_i is a descendant of V, or V and V_i have a common ancestor on the path. Suppose V_i is a descendant of V. Then, since no descendant of V is in **U**, no descendant of V_i can be in **U** either, and the path will satisfy condition (i) of d-sep, contra hypothesis. So suppose V and V_i have a common ancestor on the path, V_j. In order for condition (i) of d-sep to be violated, V_i must have a descendant in **U**. This descendant must also be a descendant of V_j, so by condition 3 of II, the directed path from V_j to V that is on our path must contain a variable that is in **U**. But this variable will not be a collider on the path, so condition (ii) of d-sep will be met. The rest of the proof then proceeds as with the proof for I. *QED*.

Note again that the set $\{U_1, U_2\}$ in figure 2 violates condition 3 of II.

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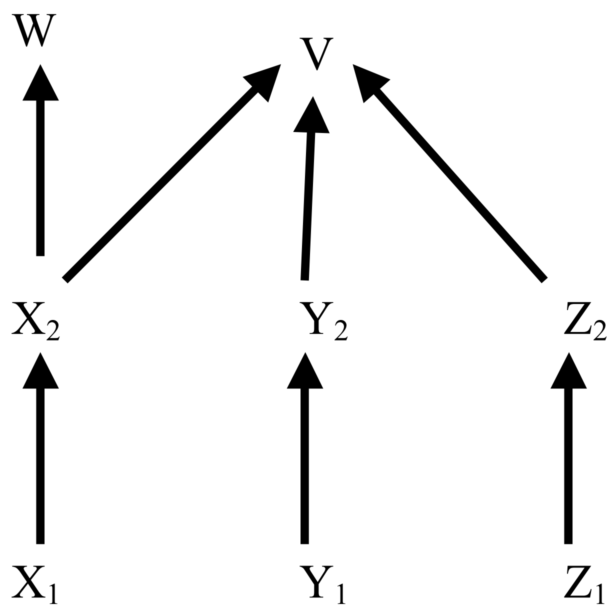

Figure 1

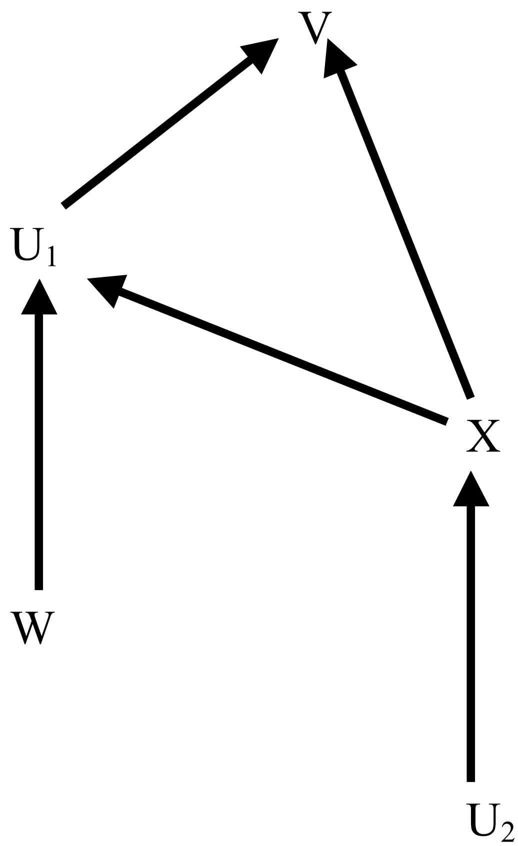

Figure 2

Cause and Chance

Précis

This paper addresses some seemingly unrelated problems in the theory of objective chance. According to Popper, Giere, Miller, Fetzer, and others, objective chances are to be understood as *propensities*, where propensities are causal dispositions. This raises three questions:

> 1. As Humphreys has noted, there are ‘backward’ conditional probabilities that are not plausibly understood as propensities. If a smoker has a probability of developing lung cancer, then a lung cancer patient has a probability of being a smoker. But if at least some probabilities must be understood in terms other than propensities, why not understand all objective probabilities in this way?
>
> 2. What kinds of (sets of) causes give rise to chances? For example, if I smoke, does that confer upon me a chance of developing lung cancer?
>
> 3. Where does the mathematical structure of chance come from? Popper and Giere sometimes describe propensities as physical magnitudes akin to charge. But it is surely an empirical matter, e.g. that charge comes in discrete units, that there are positive and negative charges, etc. But it does not seem to be an empirical matter, e.g. that probabilities take values between 0 and 1, that probabilities are additive, etc.

Let's start with the third problem. One promising line of solution to this problem is to make the mathematical structure of chance parasitic upon the mathematical structure of rational credence (or degree of belief). There are a number of arguments that purport to show that rational credences ought to obey the probability calculus (such as Dutch Book arguments, calibration arguments, and Joyce's 'non-pragmatic' argument). A number of authors, especially Mellor, Skyrms, and Lewis, have argued for a systematic link between chances and rational credences. The most explicit of these is Lewis's 'principal principle' (PP). This principle states that:

$$Cr\ (A \mid Ch(A) = p\ \&\ E) = p$$

where '*Cr*' is an agent's rational credence, *A* is any proposition, *Ch* is objective chance, and *E* is any proposition that is 'admissible' with respect to *A*. PP makes two assertions: (i) that beliefs or suppositions about the chance of *A* tends to set the agent's credence in A equal to the value of the chance believed or supposed; and that the chance of *A* screens off *A* from all admissible propositions. But PP raises its own questions:

3a. How is it connected to the idea that chances are causal propensities?

3b. Which propositions are admissible? While Lewis provides a great deal of informal discussion, and several examples and special cases, no rigorous criteria have been offered.

These two problems are inter-related. For example, it seems plausible that finite frequencies obey a PP-like principle. Given only the information that Pat is a Briton, and that 15.7% of Britons are aged 65 or older, a rational agent ought to assign credence .157 that Pat is aged 65 or older. But being British does not seem to create a causal disposition or propensity to be 65 or older. Moreover, for this PP-like principle, all sorts of propositions will count as inadmissible: Pat's gender, where in Britain Pat lives, whether Pat plays bridge or listens to Coldplay, etc.

I propose that all of these problems can be addressed using the apparatus of *Causal Bayes Nets* (CBNs). A CBN consists of an *directed acyclic graph* and a *probability function*. The nodes on the graph represents variables that stand in causal relations to one another, and a directed edge (or 'arrow') from X to Y represents that X has a causal influence on Y that is not mediated by any of the other variables in the graph. The set of *parents* of a variable X ($\mathbf{PA}(X)$) is the set of variables with an arrow pointing directly into X. *Descendants* are then defined in the obvious way (although it will be convenient to add that any variable X is considered a descendant of itself). The probability function satisfies the *Causal Markov Condition* (CMC):

$$P(X \mid \mathbf{PA}(X) \ \&\ \mathbf{Y}) = P(X \mid \mathbf{PA}(X))$$

so long as $\mathbf{Y}$ is any set of variables that are not descendants of X; i.e., the parents of X screen X off from all of its non-descendants. There is strong evidence that actual causal systems obey CMC so long as the set of variables included in the graph is sufficiently rich (in a way that can be made precise).

I propose that conditional probabilities of the form $P(X \mid \mathbf{PA}(X))$ can be directly interpreted as propensities. This account resolves all of the problems raised above.

1. It follows from CMC that the joint probability distribution over all variables can be factored into conditional probabilities of the form $P(X \mid \mathbf{PA}(X))$. Specifically, $P(X_1, \ldots, X_n) = \Pi_{\mathrm{i}}\, P(X_i \mid \mathbf{PA}(X_i))$. Thus all probabilities can be constructed out propensities.

2. It tells us that the set of direct causes of X suffice to confer a propensity for X.

3. This proposal establishes a connection between the causal structure of propensities. and Lewis's PP. Suppose that a rational agent believes (or supposes) that a given CBN accurately describes some causal system, and sets her credences in accordance with the probability in that CBN. Then the conditional probabilities of the form $P(X \mid \mathbf{PA}(X))$, together with the actual values of $\mathbf{PA}(X)$ will function as an objective chance function for that agent. That is, the agent's credence in X, given $P(X \mid \mathbf{PA}(X))$ & $\mathbf{PA}(X)$, will equal $P(X \mid \mathbf{PA}(X))$. Moreover, this credence will remain the same if the agent conditionalizes on any further information about the values of variables, so long as these do not include X or any descendants of X. This allows us to give a purely causal characterization of admissibility.

Finally, it is possible to generalize this approach to allow other sets of causes of *X* to confer chances upon *X*, thus capturing the idea that the chance of *X* can evolve over time. CMC entails a graphical criterion for screening-off called D-separation. Using D-separation, we can identify sets of causes that suffice to screen *X* off from other variables in the right way to count as objective chance.