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Epistemic Theories of Objective Chance

1 Introduction

In this paper I will examine and defend a type of propensity theory of objective chance that, while far from new, has been largely neglected in recent decades. I am not aware of a general term for views of this sort, but I will call it the *epistemic* view of chance. Physical chances, on this view, have all their generally-accepted properties, so that the view does not offer some mere epistemic surrogate for chance, but the real thing. After surveying the history of this approach to chance, I will advocate a particular version of it.

The epistemic view of chance has a long history, and (as shown in Section 6) it entails all our common beliefs about chance, so why is it often overlooked?[1] The main reason, as far as I can judge, is that it conflicts with accepted views in related areas such as causation, laws of nature, and the extent of rational constraints on subjective probability. In particular, the three problems of chance, causation, and natural laws are interlocked, like pieces of a jigsaw puzzle, so that they must be solved simultaneously. I believe that the epistemic approach to chance not only provides the best understanding of physical chance, but also clears a path toward more satisfactory views of causation and natural laws. Sections 4 and 5 provide an overview of this work.

A number of books and papers have either suggested or endorsed an epistemic view of chance, and these are summarised in Section 2. However, to my knowledge, no article devoted to articulating and defending the view in detail has previously been published. It is my hope that, in summarising all this work in an accessible form, the merits of the view will be brought to the attention of philosophers, who have unfortunately very little awareness of it at present.

[1] For example, when Hoefer (2007) surveys the existing views of chance, and finds them all wanting, he does not mention the epistemic view.

I realise that the epistemic view of chance will initially seem implausible to many, due to the basic fact that chances are 'in the world' rather than 'in the head'. Chances are invoked to *explain* empirical data, such as the presence of stable relative frequencies and algorithmic randomness in the outcome sequences of some experiments. Since these data are physical facts, the chances that explain them ought to be physical facts as well.[2] We certainly cannot explain physical facts by appeal to human knowledge or ignorance! I will refer to this reasoning as the 'no epistemic explanations' argument.[3]

My full response to the 'no epistemic explanations' argument cannot be given until Section 7, but I wish to make three quick clarifications. First, one must be careful to distinguish between propensities, which are physical tendencies, and chances which are the *numerical measures* of those tendencies. Consider, for example, the differences in stability between isotopes. If you buy some copper piping at a hardware store, it will be mostly composed of copper 63 and 65. It will contain very little copper 62, as this isotope decays so quickly. Copper 63, on the other hand, lasts indefinitely. We say that nuclei of copper 62 have a much stronger tendency to decay than those of copper 63. This difference in tendencies is certainly not epistemic, since it results somehow from a difference in the number of neutrons per nucleus: copper 63 has 34 neutrons, one more than copper 62 has. Chances are not propensities, however, but numerical *measures* of propensities. By 'measure' here I mean a real-valued function that represents the tendencies, which moreover satisfies the axiomatic requirements for a normalised measure, or probability function. This measure is indeed epistemic, in the sense that the numbers are rational degrees of belief.

An analogy may be helpful here. Suppose that, in a certain city that is prone to earthquakes, a team of competent structural engineers develops an earthquake rating for each building: the maximum magnitude of earthquake that the building would be expected to survive (with epistemic probability 90% say). Thus, a strong building might have a rating of 7.8, whereas a weaker one's rating might be only 6.1. These ratings are derived using error-free calculations of the building's performance under different scenarios, using all the available information, including the original building blueprints and geological surveys. The earthquake rating of a building is then epistemic, being based on expert opinion, and is distinct from the building's ordinary physical properties. For example, if a building collapses during an earthquake, then this was not (even partially) *caused* by its low earthquake rating.[4]

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[2] See for example Franklin (2009: 161-2) for a statement of this argument.

[3] Of course one can explain human behaviour in epistemic terms, but here we are talking about inanimate processes.

[4] Here I just mean that, had the building not been given any rating, its performance during the earthquake would not have been any different. The assignment of a low rating did not help to destroy it.

A second clarification is to distinguish between a quantity being observer-dependent and its *varying* between observers. For chance, on the epistemic view, is observer-dependent but does not vary between observers. How is this possible? It results from the "observer" in this case being a rather ideal one, who is not only (perfectly) rational but who also has maximal information about all the factors that might help to cause the event in question. In this way, the inevitable differences between human observers in knowledge and reasoning ability are beside the point, and the chance function supervenes on the physical facts alone. Given this supervenience relation, we should resist the thought that since chances are epistemic, they are therefore 'not real'. *Chances are both epistemic and real*. It is similar with the earthquake ratings in the example above. While these ratings are epistemic, they are also physical properties of the building in the sense that they supervene on the physical properties (such as the number, sizes and placement of columns). Two identical buildings, built upon exactly similar geological conditions, will have the same earthquake rating.

The epistemic view of chance does require, of course, that rationality be a necessary feature of the world, one that goes beyond human physiology and culture. The half-life of Copper 62 is about 9.74 minutes, and this value does not in any way depend on Western civilization or the layout of the cerebral cortex (beyond the choice of unit). Such a high view of rationality is not uncommon among analytic philosophers, however.[5] Logicians are still reluctant to see logical truths such as *modus ponens* or the theorems of the probability calculus as socially constructed, or dependent on human evolution. Thus, I do not see the requirement for such an independent rationality to be a drawback for the epistemic view. Instead, I see it as a useful argument against those who would make rationality a mere human construct. I am happy to say that such a claim is hard to square with the existence of objective chances.

A third quick response to the 'no epistemic explanations' argument is that, while we generally explain events by appeal to their causes, explanation also involves an *inference* of the explanandum E from the proposed cause C. Without this inference, there is no intellectual connection between the cause and the effect, and so we feel unsatisfied and ask, "But *why* did C

[5] A passionate defense of this view is given by Frege in his introduction to the *Grundlagen* (1884). For an opposing view see Cooper (2001) who proposes an evolutionary origin not just of human cognition, but *also of the laws of logic themselves*. He writes (p. 5) "The laws of logic are neither preexistent nor independent. They owe their very existence to evolutionary processes, their source and provenance." I agree with Frege (1884) that if views like Cooper's were correct then "… there would no longer be any possibility of getting to know anything about the world and everything would be plunged in confusion … this account makes everything subjective, and if we follow it through to the end, does away with truth." (p. VII).

cause E?" Furthermore, this inference from cause to effect does not always reason from a direct representation of the cause itself, but may use an indirect representation, especially if a direct representation is unavailable. In the earthquake example above, let's imagine that an earthquake of magnitude 7.0 hits the city, and it is found that buildings with high earthquake ratings generally perform much better than those with low ratings. In that case, even though the low earthquake rating of a building is clearly not a *cause* of its collapse during the earthquake, many will no doubt use it to *explain* the collapse. In effect, the explanation is: "The building fell because it had certain structural characteristics that justified expert opinion that it would collapse." This is not a fully satisfying explanation, even though it does refer to a cause of the collapse, since the reasoning used to infer the effect from the cause is hidden from view. Yet, if this is the only available explanation (perhaps since the building's blueprints are all lost or destroyed) then it is much better than nothing. In the case of physical chances, the epistemic view says that we can only represent the strength of a single-case propensity as a rational degree of belief. Then, since this is the only measure of propensity we have, we use the chance measure over the outcome space for an experiment to infer (and hence explain) the observed features of long sequences of outcomes, such as stable frequencies and algorithmic randomness.

2 A brief survey of epistemic theories of chance

The only fully-developed epistemic theory of chance is my own theory, presented in Johns (2002), but some key elements of this view were developed by Henri Poincaré, John Maynard Keynes, and Frank Ramsey. In particular, Keynes was the first to develop the required notion of epistemic probability as a rational degree of belief, and Poincaré held that physical probability could not arise merely from ignorance, but requires a certain kind of dynamical system whose behaviour is highly sensitive to the initial state, and Ramsey combined these elements together. We will begin this brief survey with Laplace, however, whose *Memoire sur les Probabilités* (1781) anticipated some key aspects of contemporary epistemic theories.

Laplace was of course a determinist, and hence rejected the idea of irreducibly chancy events. Events are ascribed to chance, Laplace held, only as a result of our ignorance of their true causes, and so one can only talk of probability as something that reflects our knowledge or ignorance of the world. Thus, although Laplace did not refer to partial belief or betting quotients, he in effect held that all probabilities are epistemic. Nevertheless, Laplace recognised a distinction between 'absolute' and 'relative' probability that strongly resembles the familiar distinction between chance and epistemic probability.

Laplace needed the distinction between absolute and relative probabilities in order to solve the problem of the coin of unknown bias. If one has no information about the possible bias of the coin, then it seems reasonable to assign equal probability 0.5 to both heads and tails. This is an example of a *relative* probability, in Laplace's terminology, as it is based on ignorance of the coin's true nature. Relative probabilities do not obey the product rule, as for example the relative probability of getting two heads in two tosses of this coin will not be ¼, but something a little greater, since if the coin lands heads on the first toss, this is slight evidence for a bias towards heads. The product rule does hold however for *absolute* probabilities, which are based on knowledge of the causes rather than ignorance, and so are viewed as real and objective. For example, some coins are objectively fair, and have an absolute probability ½ for heads whether anyone knows it or not. According to Laplace the frequency of heads, in a long run of tosses, will likely approximate this absolute probability.

The obvious definition of 'absolute' probability would be probability based on absolute (i.e. maximal) knowledge of the causes, yet Laplace cannot use this definition on account of his commitment to determinism. In a Laplacian universe all such maximal-knowledge probabilities would be 0 or 1. On the other hand, if absolute probabilities are based on any degree of ignorance, then it would seem that they will vary according to the knowledge that is missing, and so lack objectivity. Faced with this difficulty, Laplace admits that "the distinction between absolute and relative probability may appear imaginary" (1781, p. 385).

Laplace's solution is to define absolute probabilities as those based on maximal knowledge of *constant* features of the experiment, such as "the tendency of the dice to fall on one face rather than the others" (p. 385), and ignore variable features such as the exact way the dice are thrown. Whether or not this approach solves the subjectivity problem need not concern us here. The most enduring aspect of Laplace's theory of absolute probability is his method for defining it on the basis of symmetries with respect to quasi-maximal knowledge of the causes. Laplace writes that absolute probabilities may be determined:

> … *a priori*, when just from the nature of the events one sees that they are possible in a given ratio; for example, in a game of heads and tails, if the coin that is flipped is homogeneous and its two faces are entirely alike, we judge that heads and tails are equally possible. (Laplace 1781, p. 384)

In other words, Laplace claims, equal absolute probability is assigned by virtue of symmetries in a quasi-maximal description of the causes. Then, if the space of all possible outcomes can be partitioned into N equally-probable events, the probability of each such event is just $1/N$. We will return to the idea that chances arise from symmetries in Section 8.2.

Poincaré's theory of physical probability (1896) has recently gained some attention, through such supporters such as Abrams (2012) and Strevens (1998). The history of this approach is described in detail by von Plato (1994), who calls it 'the method of arbitrary functions'. The key idea here is that, for some physical systems, there are microscopic changes in the initial state that will cause a significant change in the final outcome. The outcomes of such systems, while nomically pre-determined (and hence predictable by Laplace's demon) are not predictable by us. Poincaré identified a further feature of many such unpredictable systems, that the probability distribution over outcomes is approximately invariant under a wide range of distributions over the possible initial states.

A roulette wheel, for example, has about the same probability for black no matter how rapidly it is set in motion, as long as the input probability distribution is sufficiently 'smooth'. The possible starting speeds of the wheel can be viewed as points on the real number line, and these points can be coloured (red or black) according to the outcome determined by that input speed. This line will then have 'stripes', or alternating red and black regions, each such region being approximately equal in width to its close neighbours. The probability distribution over the starting speeds is then irrelevant, so long as it is approximately constant at the scale of these (very thin) stripe widths.

Poincare's approach is similar to the epistemic approach, in that chancy events are those that are *unpredictable*, in the sense of not being inferable from the best available knowledge of their causes. On the other hand, Poincaré does not define chance as a rational degree of belief – a fact that is unsurprising, since this concept had not yet been clearly articulated.

J. M. Keynes (1921) introduces the notion that rational degrees of belief are the fundamental kind of probability, and proposes "to treat all other relevant conceptions as derivative from this" (p. 322). Like Poincaré, Keynes views an event such as the outcome of a coin toss as being a matter of chance, "when it is brought about by the coincidence of forces and circumstances so numerous and complex that knowledge sufficient for its prediction is of a kind altogether out of our reach." (p. 337) Keynes also agrees with Poincaré's requirement for the objective chance be stable, or resilient, in the sense of our predictions not changing very much when we acquire new information about these generating conditions. An event is due to objective chance only if, "... the addition of a wide knowledge of general principles would be little use." (p. 330).

As far as I am aware, Keynes does not explicitly define objective chance functions, but writes only of events being due to objective chance. His Cambridge colleague Frank Ramsey (1931, pp. 206-11) goes a step further, however, and defines chances as degrees of belief. After rejecting frequency theories of chance for the usual sorts of reasons, he continues:

> (2) Hence chances must be defined by degrees of belief; but they do not correspond to anyone's actual degrees of belief…
>
> (3) Chances are degrees of belief within a certain system of beliefs and degrees of belief; not those of any actual person, but in a simplified system to which those of actual people, especially the speaker, in part approximate.
>
> (4) This system of beliefs consists, firstly, of natural laws, which are in it believed for certain, although, of course, people are not really certain of them… (Ramsey, 1931, p. 206)

At first sight this looks very similar to the epistemic view I advocate, but Ramsey's understanding of chance is part of his best-system account of laws, so that he does not see chance as a measure of propensity. (In fact, propensity theories had not yet been proposed, which perhaps explains his hasty inference in the first sentence.) Propensity theories attempt to explain some features of the world in terms of single-case chances, whereas best-system accounts take the mosaic of actual events as basic facts that cannot be explained. Hence, despite the identification of chance with an idealised epistemic probability, Ramsey does not support what I am calling the epistemic approach to chance.

David Lewis (1980) considers an epistemic theory of chance that is similar to mine, in that the chance of an event is analysed as the rational degree of belief, given maximal information of the laws and past history of the universe. Lewis quickly rejects this analysis, however, since it is inconsistent with his other metaphysical views, especially concerning causation and laws of nature. There is a discussion of what epistemic theories of chance should say about causation in Section 4, and about laws of nature in Section 5.

Brian Skyrms (1980, pp. 25-6) takes up Poincaré and Keynes's idea that propensities are epistemic probabilities that are "highly resilient", i.e. approximately invariant when conditionalising on a wide range of other facts about the world. Unlike Poincaré and Keynes, however, Skyrms does not see objective chance as a relation between cause and effect. Skyrms's notion of 'propensity' is therefore rather different from the standard one that I aim to analyse here, and I will not discuss it further.

Skyrms briefly mentions the epistemic approach taken in this paper, saying, "... we could define *probability maximally objectified according to causal antecedents* of an event as its probability conditional on the complete description of the entire backward light cone of that event." He gives a qualified endorsement of this approach, saying "There is at least one sense of *chance* for which [the above] provides a fair explication." Skyrms does not however regard this "ancient sense of chance" as the most useful one, and does not develop the theory in any detail.

My epistemic theory of chance, initially developed in Johns (2002), is similar to Lewis's idea in that I define the chance of an event as its logical probability, given a maximal specification of its possible causes (including the dynamical laws). The specification of the causes here is absolutely maximal, so that pre-determined events can only have the trivial chances 0 and 1 on this account, as with Giere's (1973) propensity view. I dubbed this view the 'causal theory of chance', intending to stress the reversed direction of analysis between my theory of chance and probabilistic (i.e. chancy) theories of causation. (Rather than taking chance as a primitive, and defining a cause as some kind of chance-raiser, I take causation as primitive and uses it to define chance.) As far as I am aware, this is the only detailed development of the epistemic approach to chance, and so one purpose of this paper is to summarise its main features in a much shorter and more accessible form. I will also attempt to correct and improve some aspects of my theory, and evaluate some criticisms of it, made by Eagle (2004), that I have not previously responded to.

Finally, Jon Williamson (2009) briefly outlines an epistemic view of chance similar to that of Lewis and me. As an Objective Bayesian, Williamson holds that epistemic probabilities are highly constrained by rational norms, and from this standpoint, "one can argue that chances are those degrees of belief fixed by some suitable all-encompassing background knowledge." (p. 503) Referring to this as the "ultimate belief" notion of chance, Williamson admits that, "much needs to be done to show that this type of approach is viable. One needs to show that this notion can capture our intuitions about chance." (p. 520) Proofs of this are given in Section 6 below, where it is shown that one epistemic analysis of chance entails all the known properties of objective chance, which are listed in the next section.

3 What do we know about objective chance?

There are some beliefs about physical chance that are very widely (and reasonably) held, by physicists as well as propensity theorists, so that any theory of chance might be expected to account for them. I will call them the Basic Assumptions. They are as follows:

(BA1) Chance is a normalised measure.

(BA2) Chance measures the strength of the tendency of a type of event to occur in a specific context. A chance of zero represents an (almost) physically impossible event, and a chance of one means that the event is (almost) physically necessary, in that context.

(BA3) The Principal Principle – see Lewis (1980). This says that the chance function is an authority, or expert probability, and this authority can only be defeated by information concerning events that occur after the experiment has begun.

(BA4) The chance of an event E is determined by its causal context, or "generating conditions", so that in identical experiments, where all the *possible* causes[6] of E are exactly replicated, it always has the same chance of occurring.

(BA5) Causally-independent events have chances that are probabilistically independent, at least for 'classical' systems. For quantum systems, there seem to be exceptions to this.

(BA6) The stability of long-run relative frequencies in repeated identical experiments (such as in quantum mechanics) and the algorithmic randomness of outcome sequences are correctly explained by positing independent and identical ('iid') single-case chances, from which the (probable) existence of stable long-run frequencies and algorithmic randomness can be mathematically derived, using the probability calculus.

(BA7) Chances can be empirically ascertained (approximately and fallibly) by measuring those stable long-run relative frequencies in repeated identical experiments.

These assumptions are not of course logically independent. David Lewis (1980) has shown, for example, that BA7 follows from BA1, BA3, BA4 and BA5 together.

Looking at BA1-7 above, we can see the sense in which chance is an objective quantity, not a subjective one. For example, BA4 says that the chance of *E* depends only on the causal context, i.e. the physical events and laws that might cause *E*, and not on anyone's beliefs about those causes. Also, the fact that chances can be empirically measured (i.e. BA7) surely entails that it is an objective quantity. On the other hand, it is easy to derive all of BA1-7 from the epistemic

[6] The causal context includes not just the things that will *actually* cause *E*, but also all factors that *might* help to bring *E* about. For example, flipping two coins rather than one increases the chances of getting at least one head, even in cases where the extra coin lands tails.

view of chance, as is shown in Section 6 below. Thus the epistemic view perfectly accounts for the fact that chances exist 'in the world'.

BA4 and BA5 are not often stated explicitly, but I think they are accepted by virtually all physicists. The main difficulty in expressing them is that they involve the notion of causation, which is a cluster of different relations. One common meaning of 'causation' in an indeterministic context, that a cause is something that raises the chance of its effect, cannot be the sense of causation involved here. For if that were the meaning of 'cause' in this context then BA5 becomes nonsensical. The two senses of 'independent', causal and probabilistic, become almost synonymous, so that the first part of BA5 becomes a tautology and the second part a contradiction. Assumption BA4 also makes little sense from this point of view, as the causal context of an outcome *E* (the entire backward light cone, as Skyrms says) should surely include any event that has an *impact* on the chance of E, whether it raises chance of *E* or lowers it. The notion of causation needed for epistemic theories of chance is one that I call 'concrete causation', which is discussed in the next section.

4 Concrete causation and chance

It is now commonplace to hold that the term 'cause' has a number of meanings, and I do not wish to denigrate those that differ from the one that is relevant here. The purpose of this section is merely to identify the kind of causation that is needed to formulate an epistemic theory of chance. In order to distinguish this kind from others I will call it a 'concrete cause'[7].

Concrete causation is the kind that Elizabeth Anscombe drew attention to in her famous inaugural lecture. She writes, for instance:

> There is something to observe here, that lies under our noses. It is little attended to, and yet still so obvious as to seem trite. It is this: causality consists in the derivativeness of an effect from its causes. This is the core, the common feature, of causality in its various kinds. Effects derive from, arise out of, come of, their causes. For example, everyone will grant that physical parenthood is a causal relation. Here the derivation is material, by fission. Now analysis in terms of

[7] I am tempted to use the term *physical* cause from Dowe (2000), since Dowe's notion is essentially the same as mine, but I do not endorse Dowe's analysis of this relation. The term 'physical' is also rather ambiguous. In Johns (2002, Section 3.1) I referred to this relation as 'efficient causation'.

necessity or universality does not tell us of this derivedness of the effect; rather it forgets about that. (Anscombe 1971, p. 136)

This idea of the cause as the *source* of the effect implies a physical connection between the two, so concrete causation is at least very close to the target of "process" theories of causation, such as those of Salmon (1984) and Dowe (2000). On the other hand, concrete causation is quite different from the notion that probabilistic and counterfactual theories of causation aim to analyse, as is shown below.

I will not give a theory of concrete causation, since this would be too large an undertaking for a short paper, but rather lay down criteria that any theory of concrete causation would have to satisfy. These are as follows.

(i) Concrete causation is intimately connected to substance, or real existence. Causes and effects cannot be mere possible states of affairs, or bundles of properties, but must really (concretely) exist. As Anscombe (1971, p. 140) notes, "a thing hasn't been caused until it has happened". And the concrete reality of causes and effects is at the core of the relation, not a superficial feature to be covered by an *ad hoc* condition in the analysis. Concrete causation is the process of an object or event becoming real. For this reason, no doubt, Anscombe's favourite example of causation seems to be that a mother concretely causes a child. She gives the child real existence, so that the effect quite literally grows out of the cause.

(ii) There is no negative of concrete causation, as one event either helps to cause another or it doesn't. There is no such thing as negative existence, of course, so there can be no negative concrete causation.

(iii) Concrete causation is not a matter of degree, because existence is not a matter of degree. An event, such as a coin landing heads, or Alice telling the truth, either occurs or it doesn't, so concrete causation is all or nothing.[8]

(iv) Concrete causation has very little to do with determination. Determination is something like logical consequence relative to the laws of physics, and holds between abstract entities such

[8] Event C may 'partially' cause E in the sense that C may be merely part of the total cause of E. But a total cause cannot (concretely) cause E merely to a certain degree.

as propositions or possible states of affairs. One counterfactual state of affairs can determine another, which shows that determination has no essential connection with real existence. Also, future states of affairs can determine those in the past.

Many philosophers suppose that the only kind of causation that can exist in an indeterministic system is 'probabilistic causation'. Ramachandran (2003, p. 152) writes for example, "If we want to allow that there is causation even in indeterministic worlds, there is little alternative but to take causation as involving chance-raising." Now the notion of a 'probabilistic cause', or chance-raiser, is a useful one in its own right[9], but is best called a *positive causal factor* to avoid conflating it with causation in the concrete sense. To prove that a positive causal factor is very different from a concrete cause I will show that the former has none of the properties (i) – (iv) listed above.

(i) Since *C* can raise the chance of *E* without either of *C* or *E* actually occurring, there is no essential connection between positive causal factors and real existence. We can simply say that *C*, if it occurred, would raise the chance of *E*. There are also well-known cases where *C* and *E* both occur, and *C* raises the chance of *E*, yet *C* did nothing at all to help cause *E*. (These are cases where *C might* have helped to cause *E* but did not in fact.)

(ii) This relation of probabilistic causation has a negative, of course, are where an event *lowers* the chance of the effect. These are called inhibitors, or negative causal factors.

(iii) Probabilistic causation is a matter of degree – causal factors may be strong or weak, depending on the size of the chance increase.

(iv) Probabilistic causation is closely connected to determination, since the extreme case of probabilistic causation, where the occurrence of *C* raises the chance of *E* all the way to 1, is just determination – or virtually so.

Despite these differences, probabilistic and concrete causation are not entirely unrelated. It is rather obvious that chancy events require concrete causes, so chances can be raised only in systems where there is underlying concrete causation. Consider for example a physical

[9] In medicine, for example, double-blind experimental studies are designed to measure such chance increases and reductions.

experiment in which the chance of some outcome *E* is 0.64. If this experiment is repeated exactly, say a trillion times, then we are rightly confident that the proportion of *E*-type outcomes will be close to 0.64. Now suppose that each chancy outcome lacks a concrete cause. In other words, we are supposing that each outcome of the experiment appears all by itself, from nowhere, rather than arising from the generating conditions. It then follows that the aggregate of one trillion outcomes also appears from nowhere.[10] Yet this is absurd. An uncaused event could not be so *reliable*. After all, an uncaused event is not controlled by anything, so there would be no source for this relative frequency of 0.64 for *E*-type outcomes.

The silliness of the notion that chancy events are uncaused becomes even clearer when we consider that, by modifying the apparatus, the relative frequency of *E* might be reliably close to some other value, say 0.315. In the absence of a concrete causal connection, why should rotating a piece of the apparatus (for example) predictably change the frequencies of outcomes?

Using this concrete notion of causation, BA4 seems accurately to capture a belief among physicists that is universal, or virtually so. To suggest to physicists that one might alter the chance of an event *E* without changing the physical context in which *E* might arise is to invite incredulous stares. After all, physical events don't appear all by themselves – they are produced by earlier physical events (deterministically or otherwise). Thus, a change in the tendency for an event to arise equates to a change in its tendency to be caused. It seems absurd that a fixed complete set of all the possible concrete causes of E might vary in its tendency to produce *E*.

BA4 also sheds light on the design of experiments to measure chances. Why does one run the "same" experiment over and over again? And what constitutes the "same" experiment? The idea is to determine the tendency of a given set of possible causes to produce *E*, and it is taken for granted that if the possible causes of E remain the same, then so will the chance of E. The world inevitably changes as the experiments are performed: the earth rotates, a distant country gets a new government, and so on. But these will not affect the chance of *E* so long as they are not possibly involved in the production of *E*.

5 Chance and laws of nature

The "causal context" referred to in the epistemic view must obviously include the dynamical properties of the system in question, i.e. the system's tendency to undergo certain kinds of change

[10] I am aware that such composition inferences are not all valid, but this one surely is. It is no different from the following: If every person in this room is from Portugal, then the aggregate of the people is also from Portugal.

more readily than others. But how could knowledge of these properties be present in an epistemic state, if not in the form of a chancy law such as "Every *F* has chance *q* to cause *G*"? If the dynamical properties of an indeterministic system can only be specified using chances, then such chances are basic physical properties, and not degrees of belief.[11]

This question about the dynamics of indeterministic systems is part of a much larger question about laws of nature in general. The chance of an event is its degree of nomic necessity, so the relation of chances to laws of nature must be tackled as part of a general theory of how laws relate to nomic necessity.

The question of what the laws of nature really are is rather vexed, yet the basic point I need to make is very simple. Among views that posit nomic necessity as an objective feature of the world (i.e. one that explains the pervasive regularities that we observe)[12] there are some like Armstrong's (1983) that take it to be primitive and unsuitable for analysis, and others that analyze it in terms of some better-understood relation such as logical consequence or metaphysical necessity. It is only primitivist views of nomic necessity, like Armstrong's, that are inconsistent with epistemic theories of chance. The latter views, which include for example Bigelow, Ellis and Lierse's (1992) "essentialist" view, and Bird's (2005) "dispositionalist" view, dovetail nicely with epistemic theories of chance. I will present and discuss these alternatives in turn.

Armstrong's primitivist view says that a statement such as "All *F* are *G*" is a law just in case the relation *N*(*F*, *G*) holds, where *N* is the primitive relation of nomic necessitation. For an indeterministic system, *N* expresses chance relations, which are primitive relations of partial nomic necessitation, so chances cannot be analysed as rational degrees of belief.

Primitivist views of nomic necessary face a serious difficulty, however, in addition to being incompatible with epistemic theories of chance, which Van Fraassen (1989) calls the "inference problem". By taking nomic necessity as primitive, the relation is only named, and not analysed in any way. And yet, we are calling it 'necessity', which presupposes that it licenses an inference of *G* from the conjunction of *N*(*F*, *G*) with *F*. This seems to be a verbal trick.[13]

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[11] This was one of the objections made in Lewis (1980) to his own (quickly dismissed) epistemic theory of chance.

[12] "In this paper I do not consider regularity or "best system" accounts of laws, or the theories of chance developed along similar (Humean) lines. The conceptual chasm between these views and mine so wide that there is little common ground on which to base an argument. This is not to say that there can be no rational grounds for choosing one camp or another, for one see can how successful (fertile, intellectually satisfying, etc.) each approach is and judge between them on those grounds.

[13] In making this point, David Lewis (1983, p. 366) nicely quipped that being called 'Armstrong' doesn't give a person big biceps.

If nomic necessity is not taken as primitive, it is usually analyzed in terms of some better-understood necessity relation, such as logical necessity. On such a view a natural law L is not intrinsically necessary, in the way that a tautology is, but must be necessitated *by* another state of affairs, which for now we can call *D* (for 'the dynamics'). Thus, the necessity of a law *L* consists in the fact that it is a logical consequence of *D*. As far as I know there is no term that applies to exactly this approach to laws of nature, so I propose the term *inferentialist*.[14] An inferentialist believes that laws of nature are necessary (or probable, in the indeterministic case) by virtue of being inferable, either deductively or with some degree of probability, from the "natures" or "essential properties" of the relevant kinds of matter.

This inferentialist approach to laws is exactly what the epistemic view of chance requires. In fact, the epistemic theory of chance could be called the 'inferentialist theory of chance', since it merely generalises the inferentialist view of laws to the indeterministic case. The inferentialist approach locates the necessity of deterministic laws, and the probability of stochastic laws, in an inferential relation, and logical inference is a matter of degree. Thus, the objection that indeterministic processes must be governed by laws that involve primitive chances is entirely avoided here.

It may appear that inferentialism is burdened with the implausible consequence that *D* itself is a logically necessary state of affairs, which would make the laws of physics necessary as well. After all, if *D* is contingent, then the entailment of some conditional ($F \to G$) by *D* does not make the conditional necessary. For example, letting *D* be the contingent fact that all the coins in my pocket are now quarters, it follows from *D* that *if this is a coin in my pocket then this is a quarter*. Yet that conditional is hardly a necessary truth, as I could easily have a dime in my pocket, and it does not strike us as a law-like regularity. This example shows that *D* has to meet certain requirements, in order for its consequences to count as laws.

Yet the requirement that *D* itself be necessary is much too strict. *D* can in fact be contingent, as long as it has some of the properties of a necessary truth, namely:

(i) *D* is *impervious*. (Nothing that we can do has any effect on *D*.)

(ii) *D* is *invariant*. (*D* is constant across time and space.)

These properties are sufficient to account for the apparent 'necessity' of laws, as understood by common sense. For an analogy, suppose we are humble peasants in some developing country,

[14] 'Consequentialist' might be better, but this word is too strongly associated with a view about ethics.

and a contingent decision is made in a corporate boardroom, in some rich and powerful country. While the decision could have been otherwise, we had no influence over it, and now have no power to change it.[15] Moreover suppose that, even if the decision was not specifically about us, it entails that every *F* will be a *G* in our country. Then, from our perspective, there can be no *F* without *G* as well. We can assert counterfactuals of the form "had this been an *F*, then it would have been a *G*". Now let us also suppose that the board that made this decision never changes its mind, and that its decisions are binding in all the places we can experience. This decision, though contingent, will stand for all time once it is made, and apparently apply everywhere. The statement "every *F* is a *G*" now has, from our perspective, all the characteristics of a law of nature, even though it is logically contingent.[16]

The argument above shows that an inferentialist view of laws does not require *D* to be necessary, but only invariant and impervious, in order to account for the so-called 'necessity' of laws. We have not said, of course, what *D* is exactly, nor shown that *D* is in fact invariant and impervious. There has certainly been no attempt to explain *why D* should have these features. These are all good questions, but this is not the place to try to answer them.

In the remainder of this paper I shall just refer to *D* using my term from Johns (2002), the 'dynamical nature', of the system (see pp. 66-69). I am not able to say too much there about dynamical natures, but argue that the existence of such properties is presupposed by the general practice of physics, which respects something like Aristotle's basic distinction between natural and compulsory motions. A closed system, one that is free of external interactions, behaves in a way that can be inferred from its Lagrangian (or its Hamiltonian[17]) together with its initial state. The Lagrangian thus captures the system's intrinsic tendencies to undergo certain kinds of change, so I infer that such tendencies exist, and dub them the 'dynamical nature' of the system.

6 The fundamental principle for chance

David Lewis has rightly drawn attention to the Principal Principle (PP) as being of central importance to our understanding of chance, yet there is a related principle that (remarkably) not only entails PP but all of the other Basic Assumptions as well. Moreover, this principle is a

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[15] Impervious events are often misidentified as being necessary. Consider, for example, the so-called "necessity" of the past, which is really just the causal independence of the past from present events.
[16] In a similar way, the inferentialist view of laws is consistent with theological voluntarism concerning natural laws, as espoused by William of Ockham for example.
[17] The Lagrangian and Hamiltonian are different mathematical functions that express essentially the same information about the system, in the sense that either can be derived from the other.

consequence of the Basic Assumptions, so it neatly condenses them into a single statement. For this reason I will call it the *Fundamental Principle* for chance (FP).

(FP) The chance of an event E is numerically equal to the rational degree of belief in E, given maximal knowledge of the causal context of E (and nothing else).

Lewis (1980, p. 277) derived something very much like FP, describing it as a 'reformulation' of the Principal Principle. FP is slightly different from Lewis's reformulated PP, however, being stronger than PP. (To prove FP requires BA4 in addition to PP.)

The fundamental principle merely states that chances are numerically equal to certain rational credences, and so is not itself an analysis of chance. FP does not claim that chances are *identical* to those credences. If one does take that extra step of treating FP as an analysis, one arrives at an epistemic theory of chance. Later in this section we will consider the merits of using FP as an analysis, but first we must show that (as claimed above) FP both entails, and is entailed by, the Basic Assumptions.

In proving these results it will be convenient to use an abbreviation for the epistemic probability function referred to in FP, namely the one generated by maximal knowledge of the experimental conditions, or causal context (and nothing else) for the events in question. We will call this the *causal credence* function. The causal credence function represents perfectly rational degrees of belief, so we can think of it as the credence function for Laplace's demon, when the demon has maximal knowledge of the causal context. Using this term, FP becomes the very simple claim that the chance of an event equals its causal credence.

First we will prove that FP is a consequence of the Basic Assumptions. Let the proposition M be a maximal description of the causal context, and X a proposition that says the chance of event E is x. FP then follows from two premises: (i) M is 'admissible' in Lewis's sense, since it describes only states of affairs prior to the start of the experiment[18], and (ii) The chances of events are logical consequences of M, so that $M \Rightarrow X$ for example. Starting with BA3, the Principal Principle, and writing P_K for epistemic probability in the (admissible) epistemic state K, we have:

$$P_K(E \mid X) = x$$

[18] It should be noted that the epistemic view of chance is one according to which, as Lewis (1980, p. 112) put it, "the complete theory of chance for every world, and all the conditionals that comprise it, are necessary". (Thus, Lewis's problem of undermining futures does not arise for the epistemic view.) The causal context M consists of the past history of the world together with D (the dynamical nature), and all chances logically follow from M.

Then since M is admissible, as noted above, it follows that:

$$P_K (E \mid X \,\&\, M) = x.$$

Now we need to show that $M \Rightarrow X$, which follows from BA4, i.e. the assumption that the chance of an event is determined by its causal context. If M does not entail X, then there is a possible state of affairs in which M is true but X is false, so that the chance of E takes some value other than x. But in that case, the chance can vary while the causal context is fixed, contrary to BA4, which proves that $M \Rightarrow X$. Hence $(X \,\&\, M) \Leftrightarrow M$, so that the chance of E equals $P_K (E \mid M)$, as stated by FP.

We will now show that the fundamental principle (FP) entails the Basic Assumptions.

BA1 Since causal credences are epistemic probabilities, one can use the usual arguments (e.g. Dutch book arguments) that they must conform to the probability calculus.

BA2 A causal credence of 1 means that Laplace's demon is certain[19] that the event will occur. In other words, the event's occurrence is logically entailed by a maximal description of the causes, so that the event is physically determined. In a similar way, an event with zero causal credence is (almost) determined not to occur. Events whose causal credences are between zero and one therefore have some degree of determination by their causes. The *degree of determination* by a causal context is, I think, the only reasonable interpretation of the 'strength of causal tendency' within that context.

BA3 The Principal Principle is a fairly straightforward consequence of the epistemic view, since PP says that chance is an authority (or expert) probability. In general, the rational credence of a person who knows everything I do about a certain subject, and more besides, is an authority for me.[20] This means that my degrees of belief should equal theirs, when they are known. Now suppose that I know for certain the causal credence q of an event E, and perhaps also have some information about the causal context of E, but have no knowledge concerning

[19] Or almost certain. It is shown in Johns (2002, pp. 31-32) that the rational number (quotient) 1 represents actual certainty, whereas the real number (Dedekind cut or Cauchy sequence) 1 represents a degree of belief that is either certain or very close to certainty. The same distinction applies to probability zero.
[20] This understanding of deference to experts agrees with the account given in Joyce (2007).

events after the start of the experiment. In that case, I know that Laplace's demon believes to degree *q* that *E* occurs. Since the demon's knowledge of the causal context of *E* is maximal it must include mine, so he is an epistemic authority for me. Hence I should defer to the demon, in which case my degree of belief in the occurrence of *E* will be *q* well.

BA4 That the causal credence of an event is determined by its causal context follows rather trivially from the epistemic view, provided one has a sufficiently high view of rationality. All that is required is that there be precisely one maximal (true) proposition describing the causal context of the event in question. (This is easily shown. There cannot be more than one maximum, and the maximal proposition can be constructed as the conjunction of all the true propositions describing the causal context.)

BA5 My proof in Johns (2002, pp. 114-121) that outcomes of separate experiments are probabilistically independent with respect to causal credence, but in the classical case only, is too lengthy to be repeated here. The 'classical' assumption made there is that the maximal description of a pair of systems <*X*, *Y*> is always equivalent to a logical conjunction of descriptions, one concerning *X* only and the other *Y* only.[21] The derivation of independence in this classical case also makes crucial use of the fact that the causal credence for a trial is based on a *maximal* specification of the causal context for that trial, so that learning the outcomes of other trials cannot increase one's knowledge about the causes on that trial. I also use a Humean assumption that separate objects have no *a priori* epistemic relevance to each other.

BA6 It is clear from the points above that causal credences are objective, single-case probabilities. The probability calculus then entails that in repeated identical experiments, which are probabilistically independent from BA5, the long-run relative frequency for an outcome *E* has a high causal credence for being close to the causal credence for *E*. Then, since causal credences are expert probabilities, we can reasonably infer that the relative frequency for *E* will be close to the causal credence. Thus we have inferred the relative frequency of *E* from

[21] I note that an *incomplete* description of a pair <X, Y> will not factorise into a conjunction, in some cases, and argue that this fact can account for the existence of quantum-mechanical correlations, such as in the famous EPR experiments. My view is that the quantum state vector represents maximal-yet-incomplete information about the system. Generally speaking, epistemic views of chance go hand-in-hand with epistemic interpretations of quantum states, such as those of Spekkens (2007) and Bub (2007).

a hypothesis about the causes of *E*, so we have explained this relative frequency.[22] The algorithmic randomness of outcomes is similarly explained by the fact (derivable from the independence of outcomes) that different outcomes with the same overall frequencies have equal causal credence. Since the vast majority of sequences of a given set of terms are algorithmically random, and these sequences have equal causal credence, it follows that the causal credence of getting some random outcome or other is very close to 1.

BA7 I will not argue in detail that causal credences are empirically measurable, according to the epistemic view, since Lewis (1980, pp. 285-7) has already shown this. The premises used are Basic Assumptions 1, 3, 4 and 5.

7 Should FP be taken as an *analysis* of chance?

It was shown in the previous section that FP captures all the general knowledge that we have about chance. Given this fact, the case for taking FP as an *analysis* of chance may seem overwhelming, but alternative viewpoints must be considered. For the truth of FP is consistent with conventional propensity views, which maintain that propensity values are physical quantities that are not degrees of belief, but which nevertheless serve as a *template* (we might say) for subjective probability. To say that propensity values serve as a 'template' for partial belief means that knowledge of propensity values (in most cases) authorises numerically equal degrees of belief, as stipulated by the Principal Principle. Such standard views about propensity are held for example by Popper (1957), Gillies (1973) and Giere (1974).

On this standard propensity view, if Laplace's demon were to examine the start of a physical experiment, he would see that the template-measure of event *E* is some value *q*, and *from this* would infer to degree *q* that *E* will occur. An example of such a template is provided by some early interpretations of quantum mechanics, according to which the wavefunction represents a physical field of some kind. In that case the Born measure (the squared amplitude of the wavefunction) is also a physical quantity, but the Born measure of an outcome is equal to its chance, so it provides a template for the causal credence. According to the epistemic view, by contrast, the maximal physical description of an experiment does not include a template for the causal credence function, so that the chance values arise 'for the first time' as inferred degrees of belief. Of course, *for a given experiment* there will surely be a physically-definable measure whose values equal those

[22] As stated in the introduction, I take the view that to explain an event is to infer it (to some degree, not necessarily with certainty) from a hypothesis about the concrete causes of the event.

of the chance function[23], but such measures do not tell us what chance really *is*. An analysis of chance must give the defining characteristic of chance, and be applicable to all experiments.

Now we come the important question: Why should the epistemic view, rather than any "template" view, be seen as the better approach to propensity? I will offer three arguments here.

The first argument simply notes that the epistemic view, that chance is causal credence, does account for all of our beliefs about chance (BA 1-7). In particular, the 'no epistemic explanations' argument has been shown to be incorrect, since all the intuitions that underlie the belief that chance is non-epistemic turn out to be *consequences* of the epistemic view.

The second argument is that no physical quantity capable of acting as a template has ever been identified, and there is apparently no prospect of one. It is important to realise that the template (i.e. some non-epistemic measure of propensity) must be a highly 'portable' physical quantity, i.e. one that exists in all experiments where chancy events occur. It must therefore be a highly abstract and general feature of the experiment, so that it will exist in contexts as varied as stock markets, allele frequencies in a population, radioactive nuclei, the Stern-Gerlach apparatus, etc.

In view of the strong intuitive connection between probability and frequency, it seems natural to define the template as some kind of proportion. Proportions do after all form normalised measures, and are extremely portable. It also seems very reasonable that, if event *E* occurs in 5 out of every 6 cases, then the right credence to have for *E* is 5/6. The "cases" here might be actual outcomes of the experiment, hypothetical outcomes in a long (or even infinite) series of outcomes, or (more abstractly) regions of the system's space of possible outcomes.

The use of outcome-frequency as a template was proposed by Popper, and later endorsed by Gillies and others, in varying versions. I shall not discuss these views in detail, since there is now a strong consensus that proposals of this sort will never work. The other idea, that chance is some sort of frequency in the abstract space of possible outcomes, is similar to Laplace's theory summarised in Section 2. I will not argue against this classical view either since, like the outcome-frequency views, it has been generally abandoned.[24] The great difficulty with this classical approach is to define "equally possible" in some clear and objective way – which cannot of course involve probability, on pain of circularity. (Bertrand's paradox of the chord illustrates this difficulty well.) In Section 8.2 I will argue that this view may actually be workable, but only as a component of an epistemic theory, since "equally possible" must be defined in epistemic terms.

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[23] The description of a particular experiment will involve various parameters such as lengths and angles, and the chance of a given outcome in that experiment may be determined by some algebraic expression involving these parameters.

[24] See van Fraassen (1989, pp. 78-81) for a summary of the difficulties.

After showing that propensity cannot be understood in terms of frequency, Giere (1973) concludes that it must be taken as primitive. On this view, we cannot say anything about propensity except that it measures the disposition, tendency or inclination for the experiment to produce a given outcome in a single case. Moreover, in contrast with the epistemic view (which defines this measure as the causal credence) this undefined propensity measure is supposed to be straightforward physical property, not a credence of any kind.

The difficulties of such a primitivist view of propensity are largely the same as those faced by Armstrong's primitivist theory of nomic necessity. (This is not surprising, since a propensity value is surely just a degree of nomic necessity.) In other words, having left the propensity measure unidentified, the inference problem arises: it is impossible to show that knowledge of propensity constrains rational belief in the right way. (See van Fraassen 1989, p. 81.) Thus Giere (1973, p. 418, n. 12) says, for example,

> I assume that the correct degree of expectation (expressible as a betting rate) in an outcome equals the propensity for that outcome. Moreover this identification seems to me not to need any further explanation—just as one need not explain why it is correct to believe that any causally necessary result of a causal process will occur.

Giere is right that the authority of the propensity measure is exactly like the authority of nomic necessity, but I would agree with van Fraassen that *both* of these inferences require explanation, rather than neither. Moreover, a theory that provides such a justification, as the epistemic theory does, is surely preferable to one that is incapable in principle of doing so.

Taking propensity as primitive also faces the objection known as Humphrey's paradox, that so-called 'inverse' probabilities (where the probability of E is conditional on events that occur after E) seem to be objective and physical, yet they cannot be measures of a causal relation, since causes precede their effects. Epistemic theories, according to which chances are epistemic probabilities anyway, have no problem here.

In summary, the primitivist view of propensity is only attractive under the assumption that it is the only theory of single-case propensity in town. Arguments for this view unfortunately ignore the possibility of defining the propensity measure as causal credence, and they lose all force once this option is recognised.

My third argument against template views of chance is that there is no obvious need for a template, so it should be dispensed with according to Ockham's razor. Suppose for example we are told that some proposition A is the conclusion of an inference from premise set Φ. It would be gratuitous to infer from this that A is a member of Φ. It is true that A follows from A, so it is certainly possible that A is among the premises, but it is not the only possibility. Logical inference is more rich and surprising than that, as shown by mathematical patterns like the Mandelbrot set that are derived from very simple rules. Also, physicists are able to infer (either by hand or with a computer simulation) interesting structures like Bénard cells and mushroom clouds, from simple physical laws and statements of initial conditions that do not explicitly contain such patterns. In a similar way, there is no reason to suppose that Laplace's demon needs a template measure for his causal credences. This question will be examined further in Section 8.2.

8 Objections to the epistemic view

Some criticisms have been levelled at the epistemic view of chance, especially by Antony Eagle (2004) in his review of Johns (2002). Some of these criticisms arise from weaknesses in the way I develop or present the view, so I will attempt to make some improvements here. Other criticisms are apparently mistaken.

8.1 Criticisms of 'dynamical nature'

In his review of Johns (2002), Eagle (2004) attacks the notion of a dynamical nature in three ways. First he suggests that the concept is out of step with contemporary physics, on the grounds that physics recognises no such 'teleological' notion. Eagle's reference to dynamical nature as 'teleological' is based on some of my statements (p. 66) which suggest that Lagrangian mechanics involves teleology in some way. However, it is generally recognised that the principle of least action has only a superficial flavour of teleology, so my use of the word is misleading. In fact, the dynamical nature is postulated as an efficient cause of the system's motion, not a final cause, so this dispute between Eagle and me is merely verbal.

Eagle's second criticism (p. 885) is that the dynamical nature has not been properly identified, so that this account faces the same identification problem as primitivist accounts of nomic necessity.[25] This objection, it seems to me, is correct up to a point but nevertheless unhelpful. As an illustration, let us compare the dynamical nature to the 'dormitive virtue' of opium. The appeal to *virtus dormitiva* to explain the soporific effect of opium is of course notorious, and we tend to regard such explanations having no scientific merit at all, since they merely name, and fail

[25] See van Fraassen (1989).

to identify, the virtue in question. Nevertheless, *I think that it really is helpful to assert such theories*, in some contexts.

Imagine a scholarly community in which various theories are proposed to account for peoples' sleepiness after taking opium. One group says that opium does not *cause* drowsiness; the correlation exists because being in a pre-soporific state causes people to take opium. Another popular theory is that there is a pre-established harmony between opium taking and drowsiness. A third group says that the observed correlation between taking opium and getting drowsy is a basic regularity with no possible explanation. In such a context, is it not a significant step forward to claim that opium has some unknown physical attributes that act on the human nervous system to cause drowsiness? While the physical attributes are not yet identified, the foundation is then laid to investigate what those attributes are, and uncover their mechanism of operation.

In a similar way, Eagle is correct to the extent that coining the term 'dynamical nature' does not give much insight into what these intrinsic qualities are, or how they combine with an initial state to cause the actual history. Yet, in the present context of theorising about laws of nature, I believe that recognising its existence is a positive step. Returning to the imaginary scholarly debate about opium, the failure to identify *virtus dormitiva* shows only that the theory is incomplete, not incorrect. In a similar way, when physicists claim that electrons have a property of 'negative charge' that causes a mutual repulsion between them, critics can rightly say that this property of 'charge' has not been identified, and we have no mechanism for its operation. Thankfully, however, physicists are not put off by such criticism, since they have good grounds to believe that *some* property exists that causes the particles to be mutually repelled, and this is enough for physics to develop electromagnetic theory in its present form.

Eagle's third criticism of dynamical nature concerns the grounding of modal properties in my account of laws. Eagle writes (p. 888),

> … if the dynamical nature ... is to mention no modal facts, then that makes [that nature] a fact about the actual system **X**. But this makes it impossible to see how [dynamical nature] could support modal properties except as brute facts introduced as a metaphysical posit.

This objection apparently results from a failure to understand that epistemic theories of laws are inferentialist, as defined in Section 5, so that nomic necessity and chance are logical relations between the causal context (the dynamical nature and initial state) and its possible effects. Modal properties are *not* brute facts on this account, as they are on primitivist views like Armstrong's and Giere's.

8.2 The problem of logical probabilities

A fairly common view about epistemic probability, held by Subjective Bayesians like Bruno de Finetti, is that it is constrained only by the axioms of probability, together with an updating rule. This view entails that rationality does not provide any probabilities by itself, but only generates probabilities from other probabilities. There are, it is said, no "logical probabilities". If this is true, then the epistemic view of chance is false.

A good case can be made, however, for Objective Bayesianism, the view that there are rational constraints on epistemic probabilities in addition the probability calculus. The Principal Principle is one such constraint, as are expert principles generally, and intuitive judgements of irrelevance. It is also hard to deny that symmetries constrain rational belief, at least in some cases, such as in the 'proportional syllogism' discussed in Franklin (2001, p. 281). For example, if all we know about Fred is that he is an Albertan, and all we know about Albertans is that 72.3% of them eat beef, then rationality requires assigning a subjective probability 0.723 to the proposition that Fred eats beef.

Well-known problems arise, however, when one tries to formulate an acceptable symmetry principle that will yield such probabilities, where they exist. Principles of this sort, historically known as principles of 'Indifference' or 'Sufficient Reason', can apparently be applied in more than one way to the same problem, generating inconsistent probability assignments. I will argue, however, that these historical principles have erred by conflating two distinct epistemic situations, which we might call *symmetry* and *non-preference*, and that paradoxes arise only from assigning equi-probability on the basis of non-preference.

To define this claim more precisely, let g and h be arbitrary goods with positive subjective value. In general, if '$\geq$' represents subjective weak preference, there are four possible preference relations between g and h:

(i) g is strictly preferred to h ($g \geq h$ but not vice-versa),
(ii) h is strictly preferred to g ($h \geq g$ but not vice-versa),
(iii) g and h are preference equivalent ($g \geq h$ and $h \geq g$),
(iv) g and h are preference-incommensurable (neither $g \geq h$ nor $h \geq g$).

I show in Johns (2002, pp. 30-33) that the symmetry of propositions *A* and *B* in an epistemic state *K* entails the preference equivalence of the gambles [$1 if *A*] with [$1 if *B*], from which the equi-probability of *A* and *B* in *K* follows (given other assumptions). On the other hand, the mere absence of grounds to regard either *A* or *B* as the more probable is consistent with the gambles [$1 if *A*] and [$1 if *B*] being incommensurable, which does not entail the equi-probability of *A* and *B*. Indeed, incommensurability is not even an equivalence relation.

Thus, the paradoxes of indifference can be avoided if we are careful to assign equal probability only on the basis of symmetry, rather than mere non-preference. But what is meant by 'symmetry' here? Here is the account given in Bartha and Johns (2001, p. S116):

> a symmetry, in one's epistemic state, between A and B means that there are no resources within that epistemic state for "pointing to" or distinguishing one of the outcomes in the pair {A, B}

Here it is supposed that an epistemic state consists of objects possessing properties and relations. The 'objects' here are internal (mental) objects rather than external (real) ones, since some objects that exist in an epistemic state, such as Zeus, do not exist in reality.[26] Also, two different internal objects may represent the same external object, as with the Morning Star and Evening Star, which are distinct in some epistemic states. The subjective properties of an object in an epistemic state may also be indeterminate (or uncertain) in some respects, as for example when you have no idea where your keys are.

In some epistemic states, there may be two objects that are distinct (since they are two, not one) yet their properties in your epistemic state are identical, so that you are unable mentally single one out, and think about it separately from the other. Any description that applies to one of the objects, using properties and relations that exist in the epistemic state, also applies to the other. This is a case where you cannot mentally "point to" either of the objects singly, even though you can identify the pair together. For example, you may have visited a park earlier in the day, and remember seeing two similar spaniels there, playing together. In your epistemic state, neither dog has any distinctive characteristics that would separate it from the other, making the objects symmetric.

Where (monadic) properties are concerned, the symmetry of two or more objects is straightforward – they simply have the same set of properties, in the epistemic state. However, an

[26] The 'properties' of objects in an epistemic state are also subjective, as internal objects may have magical powers, be made of the celestial aether, and so on.

epistemic state will generally include relations as well as properties, and these relations may not reduce to individual properties, as for example when we do not know where two objects are, but only that they are close to each other. With relations in the picture, the symmetry of two objects *a* and *b* requires that there be some function *f* that maps each object in the epistemic state to another object, and maps *a* to *b*, and leaves the epistemic state as a whole unchanged in the sense that any property of *x* is also a property of *fx*, and any relation that holds between *x* and *y* also holds between *fx* and *fy*. A certain cube, for example, may be symmetric in one's epistemic state, so that the six faces all have the same subjective properties, and a rotation of the cube about its centre maps each face to another face in such a way that the relations between faces are also preserved. In that epistemic state, it will be impossible to mentally single out any one face.

The definition of symmetry above concerns propositions rather than internal objects, but the general notion is the same. For the subjectively-symmetric cube, for example, one can conceive that each face may be the one that is uppermost, so there are six different propositions concerning the uppermost face. Yet none of these six propositions can be isolated in thought from the others.

It is clear that if *A* and *B* are symmetric then the gambles [$1 if *A*] and [$1 if *B*] are preference-equivalent, since one cannot even tell them apart. I use this to show in Johns (2002, p. 44) that symmetric propositions in *K* are also equiprobable in *K* under some circumstances. Eagle (2004, p. 887) accepts these results as proof that the paradoxes are avoided, but thinks that my notion of symmetry is too narrow in scope, and so too rarely applicable, to be of much use. He therefore maintains (p. 888) that rational (e.g. symmetry) constraints, in the epistemic state of maximal knowledge of the causal context, "are too weak" to determine precise epistemic probabilities of events.

Eagle does not give an argument for this claim, but it can be supported by considering the paradoxes of indifference, such as Bertrand's paradox, where there is more than one way to partition the outcome space into (apparently) equally possible alternatives, and the different partitions yield different probabilities. My strict definition of symmetry does avoid this paradox, but only by rejecting all three of Bertrand's arguments! In other words, none of the three proposed partitions satisfies my requirement for symmetry. With the bar for true symmetry being set so high, one might easily conclude that symmetry arguments are rarely if ever applicable.

This argument neglects an important issue, however, that causal credences are based on a maximal specification of the conditions of the experiment, i.e. the causal context. Let *M* be the maximal description of the conditions of some experiment, and E_1 and E_2 be two possible outcomes. Is it a requirement, if E_1 and E_2 are to have the same causal credence, that they be perfectly symmetric in the outcome space? By no means! The relevant question is whether E_1 and E_2 are symmetric *with respect to M*. Even van Fraassen, a staunch opponent of logical probability, acknowledges this point (1989, p. 304) in his discussion of the factory making cubes of unknown size. Having shown that there are incompatible partitions of the possible cube sizes into supposedly equiprobable sets, according to the parameter used in the Uniform Distribution Principle, he comments that, "More information about the factory could improve the situation".

Information about the factory is essential here – in fact, the relevant symmetries are the ones (if any) that exist in the epistemic state of *maximal* knowledge about the factory (the cause of the cube in this case). The same response applies to Bertrand's case of the random chord. Given the details of the experiment, or causal context, one or other of the partitions defined by Bertrand might well provide truly symmetric alternatives, and hence the correct propensity values. For example, if one drops long straws from a great height above a floor on which a circle is drawn, the frequency distribution over chord lengths is found to match the probability distribution obtained using Bertrand's 'random radius' partition.

How might adding knowledge of the causal context create epistemic symmetry where there was none before? In general, adding information will break symmetries rather than create them, as more information gives additional resources for identifying a given object. The basic point is that, when the full causal context is known, then many symmetry-breaking differences between outcomes become logically irrelevant, and hence many be ignored. For example, suppose an urn contains black and white balls in an unknown proportion. Is the epistemic probability of drawing a black ball equal to that of a white ball? Not in this epistemic state, since the two propositions are easily distinguished.[27] However, if we know more about how the balls are selected then this colour difference may become irrelevant, so that symmetry is created. For example, we might learn that the balls are drawn by a blindfolded person, or that the balls are all made of steel, and drawn by a magnet. In a similar way, in Bertrand's paradox of the chord, knowledge of how the chord got there can make one of Bertrand's three arguments valid, as I show in Johns (2002, pp. 51-52).

If I were right that physical chances are actually epistemic, and determined for example by symmetries in the causal context, then would it not be possible in principle to derive *a priori* the actual physical probabilities that exist in real experiments? This idea seems wholly unrealistic. Nevertheless, I am not committed to the possibility of such *a priori* physics, since it would require knowledge that only Laplace's demon possesses, namely maximal knowledge of the dynamical nature of the system, as well as the initial state. Unfortunately, as noted in Section 8.1, we have no direct access to dynamical natures. They are rather like the mental states of animals: we observe

[27] That is the point of the following joke: A farmer had two horses, and for years he couldn't tell them apart, until one day he noticed that the black one was an inch taller than the white one.

their effects, and so can reasonably infer their existence, but we can never directly observe them. Our best hope is to try to reverse-engineer what the dynamical nature might be like, based on the *behaviour* of the systems. For example, the Bose-Einstein chance distribution that applies to some collections of particles can be derived from the assumption that the particles are "indistinguishable", in a special sense used by physicists to mean that the they have no individual identities. (Rather like the two spaniels in the epistemic state discussed above.)

Michael Strevens (1998) holds that, in some experiments, chances actually can be derived *a priori* from physical symmetries, adapting the argument of Poincare (1886) that was mentioned in Section 2. Unfortunately, Strevens' argument seems to be rather limited in application, only showing that exact probabilities arise in systems like dice and roulette wheels, where the outcomes are both symmetric and very sensitive to the initial state. (It is very doubtful whether anything similar occurs in a silver atom as it passes through the Stern-Gerlach apparatus.) Nevertheless, Strevens' argument is further support for the view that chances can, as a matter of principle, be determined by epistemic symmetries.

In summary, I would say that it is very hard to deny that logical probabilities exist in some epistemic states. It is unfortunately impossible to prove *a priori* that precise causal credences exist in, for example, quantum mechanical experiments. Nevertheless, I see no good reason to reject the epistemic approach to chance on this basis.

8.3 Theories of chance should be compatible with determinism

The epistemic theory of chance developed here is incompatible with determinism, in the sense that all chances would be either zero or one in a deterministic world. Since chances on this view are based on maximal information about the causal context, all chances in a deterministic world are trivial.

In recent decades some authors (e.g. Howson and Urbach 1993, p. 341, Hoefer 2007, p. 557) have argued generally against any account of chance that is inconsistent with determinism in this sense. The basic premises of this argument are that (i) We know that non-trivial objective chances exist, while (ii) we do not know that determinism is false. Now suppose some theory of chance *H* says that, if determinism is true, then (non-trivial) chances do not exist. Combining this with premise (ii), we infer that we do not know objective chances to exist, which contradicts premise (i). Hence the theory *H* is false. As Hoefer (2007, p. 557) puts it, "Any view of chance that implies that there may or may not be such a thing after all—it depends on what the laws of nature turn out to be—must be mistaken".

This argument strikes me as circular. For suppose there are good arguments for such a theory of chance *H*. In that case, any evidence for the existence of chances will be (indirect) evidence that determinism is false. In other words, once premise (i) is accepted, premise (ii) of the argument simply begs the question against theory *H*. Also the inference that, if determinism were true then chances would not exist, seems bizarre. Surely we should say instead that, if determinism were true then theory *H* would be false? For an analogy, suppose that an advocate of continental drift argued in 1940 that colliding continental plates are a necessary condition for fold mountains to exist. A critic might have objected that, since drift is not known to occur, the colliding-continent theory of mountain formation entails that fold mountains might not exist. "Any view of mountains that implies that there may or may not be such things after all—it depends on whether the continents move—must be mistaken". This would obviously be silly, since the right conclusion to draw, if mobilism were refuted, would be that mountains are formed some other way.

In making this objection I am not claiming that, according to the epistemic approach, chances *are* incompatible with determinism. That is another issue, about which I have no firm opinion at present (although I lean in that direction and shall present two arguments in support of it). Keynes and Poincaré were at least close to saying that objective probabilities could be understood as epistemic probabilities in a quasi-maximal state of knowledge – one that is merely about the best that humans can achieve. We might call these *imperfect chances*. Such views have been a locus of debate in the past decade, with important contributions having been made by (for example) Shaffer (2007), Glynn (2010), Eagle (2011), and Handfield and Wilson (2014). The question here is whether we can account for all of the 'chancy data' – the empirical data that chances are invoked to explain – in terms of imperfect chances. In the remainder of this section I shall summarise two arguments that imperfect chances cannot explain the chancy data.

One chancy datum is the existence of stable relative frequencies of outcomes in repeated experiments. Can a theory of imperfect chances predict[28] such stable frequencies? A general difficulty that arises here, which we can call the *problem of the initial distribution*. In a deterministic model of the system, one needs a probability distribution over the possible initial states in order to derive any probability distribution at all over the outcomes. Moreover, as stated in Section 2 above, getting the right frequency predictions requires an assumption that the initial distribution is reasonably smooth, i.e. that the probability density is approximately constant over small variations in the initial state. (In other words, gambling devices are mere *amplifiers* of uncertainty rather than creators of it, so a small uncertainty in the input is needed to produce a

[28] As mentioned above, I take it that explaining a datum requires predicting to some degree from a theory about its causes.

highly uncertain output.) In a deterministic model there are no physical probabilities, so it seems that the initial distribution must also be an epistemic probability, i.e. an (allegedly) reasonable smooth credence distribution over the possible initial states. So far so good, but then two problems arise concerning this initial distribution. First, there is the question of whether an *explanation* of a physical phenomenon can appeal to sub-maximal epistemic states in its prediction of the phenomenon. My rejection of the 'no epistemic explanations' argument in Sections 1 and 7 applied only to the special case of *maximal* knowledge (of the causal context), so that the resulting probability function is determined by the physical facts. Probabilities that depend on sub-maximal knowledge are not based on physical facts alone, and so are apparently not eligible for use in physical explanations. Second, it is not clear to me what would justify the smoothness of the initial distribution. While I have argued above that epistemic probabilities may be determined by rational constraints of symmetry and irrelevance, such constraints do not seem applicable here, and I have no idea what other constraints may be of service.

A second chancy datum is the so-called 'arrow of time', i.e. the empirical fact that many physical processes (such as diffusion) only occur in one direction. For example, you can stir cream into your coffee, but you cannot stir it out again. The arrow of time is a 'chancy datum', a phenomenon that chances are invoked to explain, since the simplest (and very common[29]) explanation for it presupposes indeterminism. The simplest explanation for the arrow of time is that the actual behaviour of a physical system has two separate causes: its dynamical nature (or the laws of physics), and the initial condition. In other words, the initial state of the system is fixed by factors external to the system, and from that point the system creates its own history according to its own dynamical nature. Now, the term 'initial condition' may seem to smuggle in a temporal arrow, but in fact there is no need to *assume* that the externally-fixed state is the initial state. Rather, if we suppose that the state of the system at some time t^* is externally fixed, then one can *show* that the temporal direction that points away from t^* has all the observed properties of the forward direction of time. I show in Johns (2002, pp. 137-147), for example, that four temporal arrows, including diffusion, occur only[30] in the temporal direction leading away from t^*.

My second argument for an indeterministic view of chances then arises from the fact that this explanation of the arrow of time requires indeterminism. In a deterministic world, fixing the state of the world at time t_1 has exactly the same effect as a suitable condition imposed at time t_2, since every part of a deterministic history follows logically from every other part (given the laws of physics). (In fact, it is not clear that the notion of a single-time constraint on a deterministic

[29] See for example Feynman (1967, Ch. 5), Sklar (1986) and Lebowitz (1993: 36).
[30] More precisely, diffusion is *far* more likely to occur in that direction of time than in the reverse direction.

process even makes sense.) In an indeterministic world, by contrast, there is no condition imposed at t_2 that is equivalent to any condition imposed at t_1. For example, if the state is fixed at t_1, then the transition probabilities in the temporal direction from t_1 to t_2 are time independent, whereas if the state at t_2 is fixed then the time-independent transition probabilities run from t_2 to t_1. Thus, in a deterministic world, an externally-imposed constraint does not produce any temporal arrows, and so this (simplest) explanation of the observed temporal arrows in our world cannot be used.

I realise that these two arguments for perfect, indeterministic chances will not convince everyone, for these issues (especially the arrow of time) are hotly contested. Nevertheless, I believe they provide some grounds for preferring indeterministic versions of the epistemic approach to chance.

9 Conclusion

The epistemic view of chance is the very simple idea that physical probabilities are a special case of epistemic probability, where the epistemic state is one of maximal (or close to maximal) knowledge of the causal context. This idea has occurred to many authors, but has failed to gain wide acceptance due to its conflict with existing views about related areas. However, this cluster of topics (causation, chance, determinism, epistemic probability, laws of nature, etc.) is presently very problematic; many different approaches to them are currently being pursued, and each one is beset with serious difficulties. I believe that the epistemic view of chance is a key piece of this puzzle that, once in place, allows all the other pieces to fit together into a new and coherent way. At the very least, the epistemic approach to chance deserves more consideration than it has received until now.

References

Abrams, M. (2012). Mechanistic Probability. *Synthese* 187: 343–375.

Anscombe, G.E.M. (1971). *Causality and Determination*, Cambridge University Press.

Armstrong, D. M. (1983). *What is a Law of Nature?* Cambridge University Press.

Bartha, P. and Johns, R. (2001). Probability and Symmetry. *Philosophy of Science*, 68 (Proceedings): S109-S122.

Bigelow, J., Ellis, B. and Lierse, C. (1992). The World as One of a Kind: Natural Necessity and Laws of Nature. *British Journal for the Philosophy of Science*, 43, (3), 371-388.

Bird, A. (2005) The Dispositionalist Conception of Laws. *Foundations of Science*, 10, 353–370.

Bub, J. (2007). Quantum Probabilities as Degrees of Belief. *Studies in History and Philosophy of Modern Physics* 38, 232 – 54.

Cooper, W. S. (2001) *The Evolution of Reason: Logic as a Branch of Psychology*, Cambridge University Press.

Dowe, P. (2000). *Physical Causation*, Cambridge University Press.

Eagle, A. (2004). A causal theory of chance? *Studies in History and Philosophy of Science A*, 35 (4), 883–890.

------------- (2011). Deterministic Chance. *Noûs* 45 (2), 269–299.

Feynman, R. (1967) *The Character of Physical Law*, Cambridge, Mass: MIT Press.

---------------- (1969). What is Science? *The Physics Teacher*, 7 (6), 313-20.

Franklin, J. (2001). Resurrecting Logical Probability. *Erkenntnis 55:* 277–305.

-------------- (2009) *What Science Knows: And How It Knows It*, Encounter Books.

Frege, G. (1884) *Die Grundlagen der Arithmetik*, trans. J. L Austin, Blackwell 1950.

Giere, R. N. (1973). Objective Single-Case Probabilities and the Foundations of Statistics. In P. Suppes et al. (Eds.) *Logic, Methodology and Philosophy of Science IV* (Amsterdam: North Holland), 468-83.

Gillies, D. A. (1973). *An Objective Theory of Probability*, London: Methuen.

Glynn, L. (2010). Deterministic Chance. *Brit. J. Phil. Sci.* 61, 51–80.

Handfield, T. and Wilson, A. (2014). Chance and Context. In Wilson, A. (ed.) *Chance and Temporal Asymmetry*, Oxford University Press.

Hoefer, C. (2007). The Third Way on Objective Probability: A Sceptic's Guide to Objective Chance. *Mind*, 116 (463), 549-596.

Howson, C. and Urbach, P. (1993). *Scientific Reasoning: The Bayesian Approach*, 2nd ed. Las Salle, Ill.: Open Court.

Johns, R. (2002). *A Theory of Physical Probability*, Toronto: University of Toronto Press.

Joyce, J. M. (2007) Epistemic Deference: The Case of Chance. *Proceedings of the Aristotelian Society* 107(2), 1-20.

Keynes, J. M. (1921). *A Treatise on Probability*. London: Macmillan.

Laplace, P-S. de (1781) *Mémoire sur les Probabilités*, reprinted in Laplace's *Oeuvres Complètes* vol. 9, 383-485. (Available from Google Books.)

Lebowitz, J. L. (1993). Boltzmann's entropy and time's arrow, *Physics Today*, September, 32-38.

Lewis, D. (1980). A subjectivist's guide to objective chance. In Lewis, *Philosophical Papers Volume II* (New York: Oxford University Press, 1986) 83-113.

-------- (1983). "New Work for a Theory of Universals," *Australasian Journal of Philosophy*, 61: 343–377.

Poincaré H. C. (1896). *Calcul des Probabilités*, Paris: Gauthier-Villars. (Page numbers from the 2nd edition, 1912.)

Popper, K. (1957). The Propensity Interpretation of the Calculus of Probability and the Quantum Theory. In S. Körner (Ed.), *The Colston Papers*, 9: 65–70.

Ramachandran, M. (2003). Indeterministic causation and varieties of chance-raising. In P. Dowe and P. Noordhof (eds.), *Cause and Chance* (Routledge, 2003) 152-62.

Ramsey, F. (1931) *Foundations of Mathematics and other Logical Essays*, Routledge and Kegan Paul.

Salmon, W. (1984) *Scientific Explanation and the Causal Structure of the World*, Princeton, N.J.: Princeton University Press.

Schaffer, J. (2007). Deterministic Chance? *Brit. J. Phil. Sci.* 58(2), 113–140.

Sklar, L. (1986) The Elusive Object of Desire: In Pursuit of the Kinetic Equations and the Second Law, *PSA Proceedings*, vol. 2, 209-225.

Skyrms, B. (1980) *Causal Necessity*, Yale University Press.

Spekkens, R. W. (2007). In defense of the epistemic view of quantum states: a toy theory. *Physics Review A* 75, 032110.

Strevens, M. (1998). Inferring probabilities from symmetries. *Noûs, 32*, 231–246.

van Fraassen, B. (1989). *Laws and Symmetry*, Oxford University Press.

von Plato, J. (1994). *Creating modern probability*. New York: Cambridge University Press.

Williamson, J. (2009). Philosophies of Probability. In A. Irvine (ed.) *Philosophy of Mathematics*, North Holland, 493-533.