No interpretation of probability*

Wolfgang Schwarz
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1 Chance: believers and skeptics

Tritium is a radioactive form of hydrogen, produced in the upper atmosphere by cosmic rays interacting with nitrogen and deuterium. As soon as a tritium atom is formed, it is at risk of decaying into helium. About 50 percent of tritium atoms decay within their first 12 years. But there seems to be no way of predicting in advance when any particular atom will decay. All we can say is that the atom has a certain *probability* of decaying within a given period of time.

Philosophers are divided over what this means. Some hold that there is an objective, physical type of probability, *chance*, built into the very structure of reality. It captures the tendency of physical systems to evolve in one way or another. Thus every tritium atom has a 50 percent chance of decaying within about 12 years. This is not a measure of our uncertainty or ignorance; it is a genuine physical quantity. Like mass and charge, it figures in causal explanations and physical laws.

Others are skeptical about primitive physical chance. For the most part, these skeptics don't dismiss all talk about chance as meaningless or false. Instead, a popular idea is that statements about chance express lack of information: to say that there's a 50 percent probability (or chance) of decay is to say that given the available information, it would be reasonable to give 50 percent credence to this event – where what counts as *available* depends on the conversational context. Thus chance is rational degree of belief conditional on (contextually determined) information about ordinary, non-chancy matters.

Also popular among skeptics are deflationist "Humean" interpretations that reduce chance facts to facts about occurrent events in the world. Most simply, chances might be identified with actual relative frequencies. A more sophisticated proposal in the same spirit is the *best-systems analysis*, which identifies chance with the probability function that figures in whatever scientific theory best combines certain theoretical virtues, including simplicity, strength and fit, where *fit* is a matter of assigning high probability to actual events (see [Lewis 1994], [Loewer 2004], [Hoefer 2007], [Cohen and Callender 2009]).[1]

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1 As with all such divisions, the line between believers and skeptics is not perfectly clear. Lewis [1980]

I am a skeptic. But I am also a scientific realist, and that creates a problem. The skeptical proposals just reviewed may be plausible as interpretations or regimentations of our ordinary conception of chance. But they are not at all plausible as interpretations of probabilistic theories in science. Let me explain.

2 Interpreting science

In the last 200 years, probability has infiltrated almost all areas of science. It plays a prominent role in various theories ranging from quantum mechanics and statistical mechanics to chemical kinetics, systems biology and evolutionary theory. If we want to take seriously what science tells us about the world, we have to ask what these theories mean.

Consider Boltzmann-style statistical mechanics. Here the object of study are isolated physical systems consisting of a large number N of particles. The possible micro-states of such a system correspond to a region Γ in a $6N$-dimensional state space each point of which specifies the precise locations and momenta of all particles. The basic postulate of statistical mechanics now states that the probability that the system's state lies in a subregion S of Γ (in the state space) is equal to the ratio $\mu(S)/\mu(\Gamma)$, where μ is a measure of volume (the Liouville measure) associated with the space.

We should note from the outset one thing this *doesn't* mean. While the postulate in some sense identifies the probability $P(S)$, that the system's state lies in region S, with the quantity $\mu(S)/\mu(\Gamma)$, the identification is not a stipulative definition: in statistical mechanics, '$P(S) = x$' is not just shorthand for '$\mu(S)/\mu(\Gamma) = x$'. This would turn the probabilistic claims of statistical mechanics into trivial analytic truths. But statistical mechanics is an empirical theory that is commonly taken to explain a large number of real-life phenomena such as the melting of ice cubes and the diffusion of milk in coffee. The formalism of statistical mechanics contains infinitely many functions all of which satisfy the mathematical conditions on a probability function; $\mu(S)/\mu(\Gamma)$ is special at least in part because it fits the empirical phenomena. In fact, it is widely held that in order to yield good empirical predictions, the standard measure μ should be replaced by another measure μ_p which gives zero weight to systems moving from a high entropy past to a low entropy future (see [Albert 2000: ch.4]).

So the identification of $P(S)$ with $\mu(S)/\mu(\Gamma)$ or $\mu_p(S)/\mu_p(\Gamma)$ is not just a definition. It seems to have empirical consequences. What exactly are these consequences? What does statistical mechanics say about the world when it says that that $P(S) = \mu_p(S)/\mu_p(\Gamma)$?

and Schaffer [2003, 2007], for example, should arguably be classified as believers although they endorse Humean accounts of chance. Paradigm believers are Popper [1982] and Gillies [2000]; paradigm skeptics are de Finetti [1974] and Jeffrey [1992]. For the present paper, it won't matter where exactly we draw the line.

Believers in a primitive physical quantity of chance might suggest that the identification of $P(S)$ with $\mu_p(S)/\mu_p(\Gamma)$ is simply a statement about that primitive quantity: it says that the relevant system has chance $\mu_p(S)/\mu_p(\Gamma)$ of being in state S. But most believers in primitive chance actually reject this interpretation, for it clashes with some popular assumptions about primitive chance. I mentioned that chance is supposed to be a dynamic quantity, reflecting the tendencies of physical systems to evolve in one way or another. Probabilities in statistical mechanics often lack this forward-looking dynamical nature. My cup of coffee hardly has a tendency to be, right now, in one micro-state rather than another. Classical statistical mechanics also assumes a deterministic micro-dynamics, while non-trivial primitive chance is usually taken to be incompatible with determinism.

Let me mention a few more arguments against the idea that statistical mechanics deals with primitive chance. First, even if the laws of quantum physics are probabilistic, the quantum physical probability that my cup is *right now* in micro-state S is plausibly either zero or one. (Only future events have interesting chances.) Yet statistical mechanics assigns non-trivial probability to this state of affairs. It is hard to see how a single, basic physical quantity could take several values for the same token event.

Second, the suggestion that the probabilities of statistical mechanics are fundamental quantities appears to render them epiphenomenal. The future state of a physical system is completely determined (to the extent that it is) by its present micro-state and the fundamental dynamical laws. It isn't sensitive to the values of any further fundamental physical quantities.[2] Relatedly, it is highly plausible that the laws of statistical mechanics supervene on the fundamental structure of the world, in the sense that a world with the very same distribution of micro-properties and the same micro-laws couldn't have different statistical mechanical probabilities. Again this suggests that these probabilities aren't fundamental.

Third, and perhaps most simply, the reason why physicists identify the probability of S with $\mu_p(S)/\mu_p(\Gamma)$ is patently not that this matches the value of some basic physical quantity. The choice of $\mu_p(S)/\mu_p(\Gamma)$ is justified in part by its formal simplicity and in part by the fact that it fits the empirical phenomena. This is true for probabilistic theories in all parts of science, including quantum mechanics. Consider the GRW theory of [Ghirardi et al. 1986] and [Pearle 1989], the main realist version of quantum mechanics that postulates probabilistic laws. GRW assumes that the deterministic Schrödinger dynamics for physical systems is occasionally punctured by random "jumps" in such a way that the probability of a jump within a given time span depends on the system's

2 The emphasis here is on *fundamental*. The future state of my coffee if certainly sensitive to its present temperature (say), although temperature does not figure in the fundamental laws. This is because temperature is realized by more fundamental physical properties. If the coffee's temperature had been different, then its micro-state would also have been different, which would have made a difference to the coffee's future.

mass. Again, this probability measure is only one among many equally real and objective candidates; it is chosen on grounds of theoretical simplicity and fit with empirical data.

In any case, even if the primitivist interpretation were plausible for probabilities in GRW quantum mechanics, I think we can safely conclude that it does not work for statistical mechanics and other "high-level" probabilistic theories. So our question is still largely open: what do *these* theories say about the world?

Skeptical accounts such as the best-systems analysis promise a unified interpretation of probabilistic theories. Recall that on the best-systems account, chance is defined as the probability function that figures in whatever empirical theory best combines the virtues of simplicity, strength and fit. This can be applied not only to fundamental theories in physics, but also to theories about the statistical behaviour of various macroscopic objects.

Unfortunately, the resulting interpretation looks again rather implausible. The problem I have in mind is especially striking for fundamental theories. For the sake of concreteness, let's pretend that GRW states that tritium atoms have a 50 percent probability of decaying within 12 years. What is the content of this statement? What is required for the statement to be true? If 'probability' is interpreted along the lines of the best-systems account, the statement is true at a world w iff *whichever physical theory best combines the virtues of simplicity, strength and fit with respect to the events in* w *assigns probability 0.5 to tritium atoms decaying within 12 years.* This doesn't sound like the kind of proposition I would have expected to find in the basic laws of physics.

Why not? Here is one reason. There is a general difference between a simple statement p and the statement that it is a law that p. Newton's second law, for example, says that $F = ma$, not that it is a law that $F = ma$. (Everyone who doesn't subscribe to a very naive regularity theory of lawhood will agree that these two claims are not at all equivalent.) On the present proposal this distinction would collapse, at least for probabilistic laws. The content of a law to the effect that under conditions C outcome A has probability x would be equivalent to the claim that this statement is part of the best physical theory and hence a law (on the best-systems account of laws).

Worse, consider theories that don't just make outright probability statements. Suppose the basic laws of physics take the form of what Lewis [1980] calls *history-to-chance conditionals*, saying that *if* H is the history of the world up to time t, *then* event A at t' has probability x. On the present proposal, this means that if H is the history of the world up to t, then the best theory assigns probability x to A at t'. But that's clearly wrong. By assumption, the best theory does not assign an outright probability to A at t', even if H happens to be the history of the world up to t.[3]

3 Does any serious scientific theory contain history-to-chance conditionals? Arguably not. Stochastic dynamical theories generally assign probabilities to future states only relative to past (or present) states, but arguably these "conditionals" are better understood as a special type of conditional

There is also the intuition that the basic laws of physics should specify quantitative relations between fundamental physical properties. Hence they should be expressible in a highly restricted language whose non-logical and non-mathematical terms stand for fundamental quantities. This is why the "Born rule" in quantum physics, for example, is not a credible law: 'measurement' does not pick out a fundamental physical kind. But the same is true for probability terms if they are interpreted along the lines of the best-systems account. Indeed, most advocates of this account admit that the theoretical virtues in terms of which probability is defined reflect our contingent epistemic interests and limitations. A concept like this shouldn't figure in objective physical laws.

Relatedly, we expect the basic laws of physics to be explanatory bedrock. Why do opposite charges attract? Perhaps there is no deeper, underlying explanation. That's just how things are. By contrast, if the best system assigns probability x to A, this is clearly not a brute fact. It is explained by patterns of occurrent events in the history of the world, together with the relevant standards for evaluating theoretical systems.

One might respond that the best-systems analysis needn't be understood as spelling out the truth-conditions of probability statements. Perhaps it only "fixes the reference": it identifies chance by a certain role, without revealing the nature of the quantity that occupies the role. This is plausible for other theoretical terms. Consider inertial mass. Perhaps our concept of inertial mass can be analyzed in terms that we don't expect to find in the laws of physics, including terms for conscious experience (as suggested in [Chalmers 2012]). The analysis would identify inertial mass by its role in our experience of the world – roughly, as the property responsible for the fact that some things are harder to accelerate and slow down than others. This role is realized by a fundamental physical quantity (as it turns out, by the very same quantity that also plays the role associated with the distinct concept of gravitational mass). Newton's second law can now be understood as a statement directly about that quantity.

But this route is not available to skeptics about chance. On the skeptical account, there is no fundamental physical quantity to be picked out by probability terms in physical theories. The only candidates in the vicinity are quantities like $\mu_p(S)/\mu_p(\Gamma)$, and we saw that probabilistic physical theories are not reasonably interpreted as talking about these quantities, as this would rob them of any empirical content.

I have focused on fundamental theories, but arguably the worries I have raised also apply to statistical mechanics or chemical kinetics. I conclude that the best-systems account fails as an interpretation of probabilistic theories in science. It can't tell us what these theories say about the world.

In section 1 I mentioned another skeptical proposal, according to which saying that the

probability. Still, versions of the present problem arise whenever a theory contains suitably complex statements involving probabilities. I would feel uncomfortable if I had to declare from the armchair that this can never happen.

chance of A is x means that conditional on the available information, it would be rational assign credence x to A. Clearly this fares no better as an interpretation of scientific theories. The laws of physics do not contain normative psychological terms, nor do they make reference to available information.

3 Meaningless theories

I asked what probabilistic theories in science say about the world. What would a world have to be like for it to be true that tritium atoms have a 50 percent probability of decaying within 12 years? What would a world have to be like for it to be true that a certain physical system has probability $\mu_p(S)/\mu_p(\Gamma)$ of being in state S? Perhaps the best answer is to reject these questions. Perhaps probability statements in scientific theories are not meant to represent a special kind of fact at all.

The suggestion is that we should broaden our conception of scientific theories. On the traditional realist conception, a good scientific theory registers important truths about the world – interesting and robust patterns in the history of occurrent events. But what if these patterns defy any simple, comprehensive description? Suppose two quantities F and G are strongly correlated, but the value of G on any given occasion is not completely determined by the value of F, nor is there a simple formula for how it is determined by F together with other salient features of the situation. A theory could simply refrain from saying anything about the connection between the quantities. But then it might fail to capture an important fact about the world. What is a scientist supposed to do if she notes (or suspects) an interesting, robust, but *noisy* relationship between two quantities? How can she express this relationship in a scientific theory?

This is where probability enters the picture. Let's allow our scientist to specify a probabilistic relationship between F and G, say by adding a noise term to an algebraic equation, with the understanding that a probabilistic model of this kind mirrors a noisy pattern of dependence in the world. By formulating a probabilistic law, the scientist does not posit a *third* physical quantity besides F and G. The point is to model the noisy relationship between F and G, not a non-noisy relationship between F, G and a further quantity P.

Probabilities are an aid to provide simple and informative models of noisy regularities. When a scientist puts forward a probabilistic model, she commits herself to the claim that it fares well, on balance, in terms of simplicity, informativeness and other relevant virtues. But this is not the *content* of her model. Her model doesn't say of itself that it maximizes theoretical virtues, or that it captures noisy relationships in the world. In order to serve its role, it is enough that the model contains a probability function. The function doesn't need an interpretation.

In a sense, the idea is to drop the last step in the best-systems account. The account begins with a competition between possible scientific theories. At this stage, the probability statements in these theories remain uninterpreted. Only the non-probabilistic parts of a theory can therefore be evaluated for truth or falsity. The probabilistic parts are evaluated for other virtues such as simplicity, strength and fit. It is important that this can be done without assigning any interpretation to the probabilities. Roughly, *simplicity* measures a theory's syntactic complexity, *strength* the number of relevant actual events in its scope, and *fit* the extent to which the theory assigns high probability to actual events. The best-systems account now goes on to define chance as the probability function that figures in whatever theory wins the competition. This step is of course crucial if we seek an interpretation of 'chance'. But that is not our topic. Interesting scientific theories do not contain the term 'chance'. Arguably scientific theories are best understood not as linguistic constructions at all, but as more abstract models. And even if a theory is written out in language, with probabilistic equations, we don't have to understand these as statements about chance, or about anything else.

A useful analogy is to imagine scientific theories as agents ("experts") with special beliefs about the world. On the traditional conception of theories, these beliefs are always apodictic: the expert beliefs that all Fs are Gs, that whenever F has value x, then G has value y, and so on. Now we also allow partial beliefs. The expert can be more or less confident that something is G given that it is F, or that G has value y if F has value x. These partial beliefs are not outright beliefs with probabilistic content. Believing something to a given degree is not to have a full belief about a physical quantity or about one's own state of mind. Accordingly, a system of partial beliefs is in the first place not true or false, but more or less *close* to the truth (more or less *accurate*): a good expert assigns high degree of belief to true propositions and low degree of belief to false ones.

How do we to interpret complex statements like 'either $P(A) = x$ or $P(A) = y$', or (returning to an earlier example) 'if H, then $P(A) = x$'? Now it isn't obvious that statements like these ever occur in reasonable scientific theories (see note 3 above). If they do occur, all that matters is that the relevant theory can be evaluated for simplicity, strength, fit and other theoretical virtues, for that is the only "interpretation" we require. So we need to ask, for example, how to measure a theory's probabilistic fit if it says that $P(A)$ equals either x or y. The question is not entirely trivial, but it also doesn't look unanswerable. In any case, if there is a problem here, then it is equally a problem for the standard best-systems account, which also assumes that one can evaluate theories for simplicity, strength and fit without first assigning a meaning to the probability terms.

I am not going to spell out precise standards for measuring a theory's virtues. I very much doubt that there are such precise and universal standards. If one theory is a little simpler, another a little more informative, and a third a little better integrated into other parts of science, we don't need to assume that there is a fact of the matter about which of them is objectively best. What actual scientists value in their models varies from discipline to discipline, from school to school, and even from person to person. I see no reason to regard this as a flaw. (Perhaps there would be more of a problem here if the definition of a central scientific term had depended on the choice of standards, as on the best-systems account.)

4 Theories, predictions, beliefs

I have argued that the point of probabilistic models in science is to provide a simple and informative systematization of noisy patterns in the world, and that to serve this purpose, such models don't need to be assessable for truth or falsehood. At first glance, this may seem to create a host of problems. If probabilistic theories don't have truth-conditional content, how can they be believed, disbelieved or conjectured? How can they be confirmed or disconfirmed by observation?

Fortunately, there is a simple and natural answer to these worries. In fact, we will see that the present account makes straightforward sense of how probabilistic hypotheses are tested in science. We will also see why both the best-systems interpretation and the epistemic interpretation come out basically right about probability talk in ordinary language.

Suppose a scientist proposes or endorses a probabilistic theory T. On the account I suggested, she thereby commits herself to the hypothesis that T provides a good systematization of the relevant patterns in the world. It needn't be the absolutely *best* systematization, since the terms of comparison are vague, but at least T shouldn't be clearly worse than any alternative. So the scientist commits herself to the truth not of T itself, but of the derivative proposition $\Box T$ that T fares comparatively well in terms of simplicity, strength, fit and whatever other theoretical virtues are salient in the context. Unlike T, $\Box T$ is an ordinary (albeit vague) proposition. It can be true or false. It can be believed, disbelieved, conjectured or denied. It can be confirmed or disconfirmed by empirical observations.

So the simple answer to the above worries is that what appear to be propositional attitudes towards a probabilistic theory T are really attitudes towards the associated proposition $\Box T$. While $\Box T$ is not plausible as the content of T, it is quite plausible as something you're committed to if you endorse T.

It is important to be clear about the content of $\Box T$. First of all, $\Box T$ is *not* the hypothesis that T provides a good systematization of our evidence, or of the evidence we could possibly gather. To be sure, a scientist might only half-heartedly and instrumentally "accept" a theory, confident that it captures interesting patterns in past and future observations, but agnostic about whether, say, the entities it postulates are real, and whether they stand in the noisy regularities suggested by the theory. To truly endorse GRW quantum mechanics, for example, you have to believe (roughly) that the state of isolated physical systems is accurately and completely characterized by their wavefunctions, and that these states evolve in accordance with the Schrödinger equation except for occasional and irregular jumps whose frequency and outcome displays statistical regularities to which the probabilities in GRW are a good approximation. This is the content of $\Box$GRW. It goes far beyond the hypothesis that GRW is a useful tool for predicting measurement outcomes.[4]

In general, $\Box T$ is closely related to propositions about randomness and relative frequency. Consider a toy example. Suppose a coin is tossed a million times, and let T be a theory that assigns probability 0.8 to heads on each toss, independent of the other outcomes. T itself can't be true or false, but $\Box T$ can. What does $\Box T$ entail about the sequence of outcomes? First of all, it entails that about 80 percent of the tosses actually come up heads. For suppose the actual frequency is, say, 70 percent. Then T provides a significantly worse systematization of the sequence than the alternative theory T' that assigns probability 0.7 to heads on each toss. Specifically, T' has much better fit (it assigns much higher probability to the actual outcomes[5]), while doing equally well in terms of simplicity and strength. $\Box T$ also entails that the sequence of outcomes does not have any conspicuous patterns. For example, it can't be 200000 heads followed by 800000 tails, or 200000 repetitions of HHHHT; in either case, it would be easy to specify the exact sequence, so a good systematization of the outcomes would not resort to probabilities at all. Plausibly, $\Box T$ also entails that right after a heads outcome, the relative frequency of another heads is again not too far from 80 percent, since otherwise a theory that doesn't treat successive tosses as independent would have greater fit without too much a cost in simplicity.

We thus have a natural explanation of how and why probability is tied to randomness (irregularity) and relative frequency. A scientist who endorses the probabilistic theory T of our coin toss sequence should expect an irregular sequence with about 80 percent heads and 20 percent tails. If the sequence turns out to be more regular or the frequencies different, the scientist will have to revise her attitude towards T.

Given these observations, it is unsurprising that many scientists sympathize with the frequency interpretation on which probability statements are analyzed as statements about relative frequencies in irregular sequences of outcomes. This isn't quite right. Scientific practice clearly allows probabilities to deviate to some extent from the actual frequencies, especially if the relevant conditions are rare. Turning to counterfactual frequencies only makes things worse, since it is virtually impossible to (a) give a coherent

4 So there is still an important contrast between scientific realism and anti-realism. It's just that what is at issue is strictly speaking not the truth (or approximate truth, or truth in certain respects) of our best theories T, but the truth (etc.) of the associated propositions $\Box T$.

5 The probability of 700 000 heads in 1 000 000 tosses is approximately 8.7×10^{-4} according to T', but 8.4×10^{-12237} according to T.

definition of these quantities that (b) retains their status as a worthy object of empirical study. So frequentism doesn't work.[6] But this isn't the end of the story. To echo a well-known passage from [Armstrong 1968: 68], whatever theory of scientific probability is true, it has a debt to pay, and a peace to be made, with frequentism. Any theory of scientific probability must explain the connection between probability, randomness and frequencies. The proposal I have offered meets this demand.[7]

We can also vindicate another popular idea in science: that the probabilities in physical theories are epistemic, representing appropriate degrees of belief given imperfect information. Return to the coin toss example. Suppose you know that the best systematization of the outcomes is the theory T which treats the tosses as independent with a fixed probability 0.8 of heads. As we saw, this entails that the sequence is irregular with about 80 percent heads and 20 percent tails. Now consider, say, toss number 512. How confident should you be that this particular toss results in heads? In the absence of further relevant information, surely your credence should be about 0.8. Moreover, your credence should be fairly insensitive to information about other outcomes. For example, conditional on the assumption that toss number 511 lands tails, your degree of belief in heads on toss number 512 should still be about 0.8.

These observations generalize: conditional on the assumption that a theory provides a good systematization of the relevant patterns in the world (i.e., given $\Box T$), a rational agent without unusual information should generally align her credence with the theory's probabilities (see [Schwarz forthcoming]).

Of course a further question is how you could come to know $\Box T$, without having surveyed the entire sequence. The short answer is: by induction. For example, if you have witnessed the first 10000 tosses, and found an irregular pattern of heads and tails with about 80 percent heads, you would normally be justified to assume that the same noisy regularities obtain in the unobserved parts of the sequence.[8]

Similarly, one might suggest that our experience of the world reveals statistical patterns in the distribution of microstates among macroscopic systems: the proportion of reasonably isolated systems whose state lies in a region S of their state space is close to the ratio

6 Further arguments against the frequentist interpretation of probabilistic theories can be found e.g. in [Hájek 1997] and [Hájek 2009]. However, note that the main topic of these papers is not the interpretation of scientific theories, but the analysis of Hájek's intuitive concept of chance. We shouldn't take it for granted that this concept plays a role in our best scientific theories.

7 Some authors seem to think the demand can be met by simply pointing at the laws of large numbers. But these laws are valid for every mathematical probability function, no matter how out of touch it is with actual frequencies and no matter how random or non-random the actual events.

8 To be sure, there is no logical guarantee that the patterns will continue, and thus no logical refutation of an inductive skeptic who claims that they won't. In the literature on scientific probability, it is sometimes assumed that in order to explain the connection between probability and rational belief, one must also refute the inductive skeptic (see e.g. [Salmon 1967], [Strevens 1999]). Unsurprisingly, the verdict then is that this can't be done.

$\mu_p(S)\mu_p(M)$. We also have indirect reason to believe in this kind of regularity: given what we know about the microphysical laws and the age and beginnings of the universe, it is reasonable to expect that those statistical regularities should arise. (This may be part of what makes them *robust* regularities: regularities that would have persisted if things had been a little different.) So we are justified to believe that statistical mechanics captures interesting statistical patterns in the world, and therefore to align our degrees of belief with the theory's probabilities. Again, the epistemic interpretation is not far from the truth. Any reason to endorse statistical mechanics is at the same time a reason to regard its probabilities as good prescriptions for belief.[9]

So far I have said nothing about the interpretation of probability statements in natural language. Suppose a scientist endorses a probabilistic theory T that assigns high probability to some event A. She might then go on to assert that A has high probability. I do *not* claim that this assertion would lack truth-conditional content. My proposal only concerns the interpretation of probabilistic theories in science, not the semantics of probability statements in natural language.

On the other hand, I have argued that by putting forward or endorsing a probabilistic theory T the scientist commits herself to the hypothesis that T provides the best systematization of relevant patterns in the world. Then it is plausible to think that by asserting that A has high probability she commits herself to the weaker hypothesis that whatever theory provides the best systematization of the relevant patterns assigns high probability to A. So while the best-systems interpretation is wrong about the content of probabilistic theories, it is essentially right about the relevant probability statements in natural language. The same is true for the epistemic interpretation, given that endorsing T generally entails believing that it would be rational to align one's beliefs with T's probabilities.[10]

5 Probability and actuality

Humeanism in metaphysics is the rejection of primitive modality. Humeans believe that truths about what *must* or *could* or *would probably* be the case are ultimately grounded in truths concerning what actually *is*. The suggestion I have defended does not require

9 I am glossing over a large number of subtleties here concerning the interpretation of statistical mechanics; see e.g. [Winsberg 2008], [Frigg 2008] and [Callender 2011] for some of these issues. It may also be worth noting that scientific theories often assign probabilities to "outcomes" A under "conditions" C, where it is much easier to find out whether C obtains than whether A obtains, which fits the idea that objective probability is the credence one ought to have given easily available information.

10 On some occasions, statements about probability in the context of developing or applying probabilistic theories might also be statements about a quantity like $\mu_p(S)/\mu_p(\Gamma)$ with which probability is identified in the relevant theory.

a Humean metaphysics. It is compatible with the idea that what is systematized by probabilistic theories are regularities not just in the actual world but in a larger sphere of nomically possible world. It is also compatible with the existence of fundamental powers and other non-Humean whatnots. But it does support the Humean cause insofar as it entails that the probabilities in science do not stand for any such whatnots.

It may also support Humeanism in another respect, by helping to answer what is often regarded as the most serious problem for Humeanism. The problem is this. Imagine a possible world that consists of nothing but a million atomic "coin toss" events. As before, the events form an irregular pattern with about 80 percent heads and 20 percent tails. On Humean accounts of chance, the chance of heads on each toss must be determined by the actual sequence of outcomes – in this type of case, it is presumably identical to the relative frequency. Hence the assumption that the chance of heads on each toss is 0.8 *entails* that about 80 percent of the tosses result in heads. Intuitively however, the assumption about chance entails nothing at all about actual outcomes. Given that the chance of heads is 0.8, it could still happen that the coins land all tails, or all heads, or 50 percent heads and 50 percent tails.

Arguably, this intuition is driven by a simple chance-credence principle according to which one's rational credence ought to match the known chances. The reasoning might go as follows: if the tosses are independent and the chance of heads is 0.8, then every sequence of heads and tails has non-zero chance; so every sequence *might come about*; all tails, for example, has a miniscule but positive chance of $0.2^{1000000}$, and deserves a corresponding amount of credence.

Above we saw that endorsing a probabilistic theory typically goes hand in hand with accepting the theory's probabilities as one's degree of belief. When a scientist asserts that the probability of a given outcome is x, she typically commits herself to the claim that it would be reasonable to assign credence x to the outcome. So it is not surprising that some of us should have developed a conception of objective probability as an empirical quantity that is tied to rational belief by the simple chance-credence principle.

However, when we consider very large events or very small worlds, the usual alignment between theoretical probability and rational belief breaks apart. If you know that the best systematization of a sequence of coin tosses assigns 80 percent probability to heads, your credence in heads on toss number 512 should be 0.8, but your credence in heads on *every* toss should be 0, not $0.8^{1000000}$. (More sophisticated formulations of the alignment, such as the "New Principle" of [Lewis 1994] and [Hall 1994], may hold universally, but they no longer licence the inference from 'positive chance' to 'might happen'.)

You might insist that your concept of chance is that of quantity tied to the simple chance-credence principle, which is incompatible with the Humean interpretation. But why should we make room for such a quantity in serious physics or metaphysics? Given that no such quantity is required for a realist interpretation of science, wouldn't it be rather presumptuous for philosophers to insist that physics has overlooked a fundamental quantity because none of the quantities it acknowledges plays the intuitive chance role?

That said, I think there is even something deeply *right* about the anti-Humean intuitions. Again consider a theory that assigns probability 0.8 to heads. On the account I defended, this entails nothing about actual outcomes. It does not entail that around 80 percent of the tosses come up heads. Indeed, there is a good and important sense in which worlds where the relative frequency of heads is 0.5 or even 0 count as models (in the model-theoretic sense) of the theory: according to the theory, these are possible, albeit unlikely ways things could be. That the frequency of heads is around 0.8 is entailed not by the theory itself, but by the assumption that it provides the best systematization of the actual outcomes. And *this* entailment is hardly controversial.

6 Conclusion

None of the currently popular accounts of objective chance yields a plausible interpretation of probabilistic theories in science. I have argued that such theories should not be understood as hypotheses about a special subject matter, objective probability. Their point is rather to provide an efficient systematization of noisy relationships in the world.

On this picture, probabilistic theories can't be true or false; they can only be evaluated for simplicity, strength, fit and other theoretical virtues. To endorse a theory is not to regard it as true. It is to regard it as a good systematization of the relevant facts, and consequently to adopt its probabilities as one's own degrees of belief.

My proposal does not entail that objective chance, as conceived by believers, does not exist. But it entails that current science gives no reason to believe in any such thing. Lewis [1980: 83] was wrong when he claimed that the practice and analysis of science requires a concept of objective chance.

References

David Albert [2000]: *Time and Chance*. Cambridge (Mass.): Harvard University Press

David M. Armstrong [1968]: *A Materialist Theory of the Mind*. London: Routledge

Craig Callender [2011]: "The past histories of molecules". In C. Beisbart and S. Hartmann (Eds.) *Probabilities in Physics,* Oxford: Oxford University Press, 83–113

David J. Chalmers [2012]: *Constructing the World*. Oxford: Oxford University Press

Jonathan Cohen and Craig Callender [2009]: "A Better Best System Account of Lawhood". *Philosophical Studies*, 145(1): 1–34

Bruno De Finetti [1974]: “Bayesianism: its unifying role for both the foundations and applications of statistics”. *International Statistical Review*, 42: 117–130

Roman Frigg [2008]: “Chance in Boltzmannian Statistical Mechanics”. *Philosophy of Science*, 75: 670–681

G. Ghirardi, A. Rimini and T. Weber [1986]: “Unified dynamics for micro and macro systems”. *Physical Review D*, 34: 470–491

Donald Gillies [2000]: *Philosophical Theories of Probability*. London: Routledge

Alan Hájek [1997]: ““Mises redux” – redux: Fifteen arguments against finite frequentism”. *Erkenntnis*, 45(2-3): 209–227

— [2009]: “Fifteen Arguments Against Hypothetical Frequentism”. *Erkenntnis*, 70(2): 211–235

Ned Hall [1994]: “Correcting the Guide to Objective Chance”. *Mind*, 103: 505–517

Carl Hoefer [2007]: “The Third Way on Objective Probability: A Skeptic’s Guide to Objective Chance”. *Mind*, 116: 549–596

Richard Jeffrey [1992]: *Probability and the Art of Judgment*. Cambridge: Cambridge University Press

David Lewis [1980]: “A Subjectivist’s Guide to Objective Chance”. In Richard Jeffrey (Ed.), *Studies in Inductive Logic and Probability* Vol. 2, University of California Press. Reprinted in Lewis’s *Philosophical Papers*, Vol. 2, 1986.

— [1994]: “Humean Supervenience Debugged”. *Mind*, 103: 473–490

Barry Loewer [2004]: “David Lewis’s Humean Theory of Objective Chance”. *Philosophy of Science*, 71: 1115–1125

P. Pearle [1989]: “Combining stochastic dynamical state-vector reduction with spontaneous localization”. *Physical Review*, 39A: 2277–2289

Karl Popper [1982]: *Quantum theory and the schism in physics*. Totowa, NJ: Rowman and Littlefield

Wesley Salmon [1967]: *The Foundation of Scientific Inference*. Pittsburgh: University of Pittsburgh Press

Jonathan Schaffer [2003]: “Principled Chances”. *British Journal for the Philosophy of Science*, 54: 27–41

— [2007]: “Deterministic Chance?” *British Journal for the Philosophy of Science*, 58: 113–140

Wolfgang Schwarz [forthcoming]: “Proving the Principal Principle”. In A. Wilson (ed.), *Asymmetries of Chance and Time*

Michael Strevens [1999]: “Objective Probability as a Guide to the World”. *Philosophical Studies*, 95: 243–275

Eric Winsberg [2008]: “Laws and chances in statistical mechanics”. *Studies in History and Philosophy of Modern Physics*, 39: 872–888