No interpretation of probability*

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> *Probability is the most important concept in modern science, especially as nobody has the slightest notion what is means.*
>
> — Attributed to Bertrand Russell

1 Introduction

Probabilities play central role in many theories across science – in quantum physics, statistical mechanics, chemical kinetics, systems biology, evolutionary theory, and elsewhere. If we want to take seriously what science tells us about the world, we have to ask what these theories mean. What does it mean to say that there is a 50 percent probability that a tritium atom will decay within 12 years? What would the world have to be like for the claim to be true? How would it differ from a world in which the probability is 60 percent?

The *propensity interpretation*, defended for example in [Popper 1982] and [Gillies 2000], gives a straightforward answer. On that account, there is an irreducible probabilistic quantity of "chance" built into the very fabric of physical reality. Chance captures the propensity of physical systems to evolve in one way rather than another. Tritium atoms, for example, have a fundamental propensity for decaying into helium. To say that the probability of decay is 50 percent is simply to say that this propensity has a certain strength. If true, that is a brute fact of physical reality.

There are many problems with the propensity interpretation. For one thing, it is doubtful that there are physically fundamental propensities: the dynamical laws of standard quantum physics are deterministic. Even if there were a fundamental quantity of chance, how could we know about it? What would justify the assumption that the quantity manifests itself in relative frequencies? Most importantly, the propensity interpretation is tailor-made to supposedly dynamical probabilities in fundamental physics. Probability statements in genetics or evolutionary theory do not seem to talk about quantum-mechanical propensities. So what do these statements mean?

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Philosophers have come up with a large menu of alternatives to the propensity interpretation. An old classic is the *frequency interpretation* that identifies probabilities with actual or counterfactual frequencies. This, too, runs into well-known problems. Scientific practice clearly allows probabilities to deviate from actual frequencies, especially if the relevant conditions are rare. Turning to counterfactual frequencies only seems to makes things worse, since it is hard to give a coherent definition of counterfactual frequencies that preserves their status as a worthy object of empirical study (see e.g. [Hájek 2009]).

A more promising suggestion in the spirit of the frequency interpretation is the *best-systems interpretation*, first developed by David Lewis [1994]. Here the physical probability of an event is defined as the probability assigned to the event by whatever empirical theory best combines certain theoretical virtues such as simplicity, strength, and fit, where the latter measures the extent to which the theory assigns high probability to actual events. Lewis originally restricted the analysis to dynamical probabilities in fundamental physics, but it can easily be extended to other areas of science (see e.g. [Loewer 2004], [Schrenk 2007], [Hoefer 2007], [Cohen and Callender 2009]).

Another prominent idea is the *epistemic interpretation* that treats probability statements as statements about rational belief. Roughly speaking, to say that an event has a 50 percent probability is here taken to mean that it would be rational to be 50 percent confident that the event will occur. The epistemic interpretation has been especially influential in statistical mechanics (see e.g. [Uffink 2011]).

I will argue that none of these proposals is plausible as an interpretation of probabilistic theories in science. They all make the same mistake: they assume that probabilistic theories are in the business of making straightforward, apodictic claims about the world. I will argue that this is not the right way to understand probabilistic theories. The probability claims in scientific theories are not meant to be true or false, and thus do not need an interpretation. The idea may sound radical and revisionary, but it turns out to be quite ecumenical. We will see that the frequency account, the best-systems account, and the epistemic account all have a natural place in the resulting picture.

2 Interpreting science

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

The postulate in some sense identifies the probability $P(S)$, that the system's state lies in region S, with the quantity $\mu(S)/\mu(\Gamma)$. But the identification is not a stipulative definition: '$P(S) = x$' is not just shorthand for '$\mu(S)/\mu(\Gamma) = x$'. That would turn the probabilistic predictions of statistical mechanics into trivial analytic truths. Statistical mechanics is an empirical theory. Its predictions explain real-life phenomena such as the melting of ice cubes and the diffusion of soy milk in coffee. This empirical success depends on the identification of $P(S)$ with $\mu(S)/\mu(\Gamma)$ rather than infinitely other probabilistic quantities that could be defined on the same state space. In fact, it is commonly held that in order to yield the right empirical predictions, $\mu(S)/\mu(\Gamma)$ is not quite right and that μ should be replaced by another measure μ_p that gives zero weight to systems moving from a high entropy past to a low entropy future (see e.g. [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 $P(S) = \mu_p(S)/\mu_p(\Gamma)$?

It does not seem make a claim about primitive propensity. My cup of coffee can hardly be said to have a propensity to be, right now, in one micro-state rather than another. Indeed, classical statistical mechanics assumes a deterministic micro-dynamics, while non-trivial propensities are usually taken to be incompatible with determinism.[1]

One might suggest that although the probabilities of statistical mechanics do not pick out primitive quantum-physical propensities, they pick out some other primitive probabilistic quantity – *statistical mechanical chance*. But there is no good reason to believe in such a quantity. Among other things, the quantity would seem to be epiphenomenal. For 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 other fundamental quantity.[2] Moreover, it is 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 are not fundamental.

1 David Albert [2000] suggests that the probabilities of Statistical Mechanics, and indeed of all higher-level scientific theories, might be reduced to those of GRW quantum physics. A friend of primitive propensities might therefore hold that all probabilistic theories in science are in fact – more or less indirectly – concerned with the primitive propensities of GRW. But even if we grant Albert's controversial reductionism about special science, GRW is a highly speculative theory. What if GRW is not the true theory of fundamental physics? Nobody thinks that this would undermine the basic principles of statistical mechanics or evolutionary theory. So there must be an interpretation of these theories on which they do not presuppose the truth of GRW.

2 The emphasis here is on *fundamental*. The future state of my coffee is certainly sensitive to its present temperature, 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.

The epistemic interpretation and the best-systems interpretation look more promising. Applied to statistical mechanics, the best-systems account would go roughly as follows (see e.g. [Loewer 2007]). First, imagine a list of possible theories dealing with thermodynamic phenomena. Given full information about the world, each theory on the list can be evaluated for accuracy, simplicity, strength, and other theoretical virtues. If a theory involves probabilities, let's also evaluate the extent to which it assigns high probability to actual events. Suppose some probabilistic theory T comes out best, on balance, by those criteria. The best-system account now defines the true probability of an event as the probability the "best system" T assigns to the event.

Spelling out the details would raise some interesting subtleties (see e.g. [Winsberg 2013]). By comparison, the problem I want to raise is very simple and common to all applications of the best-systems account. It is easiest to see in the application for which the account was originally developed: for stochastic dynamical theories in fundamental physics. So let me briefly switch examples.

The leading version of quantum physics that postulates a stochastic dynamics is the GRW theory of [Ghirardi et al. 1986] and [Pearle 1989]. GRW assigns probabilities to certain transitions between superposition states of physical systems. For our purpose, the details don't matter. Suppose GRW states that there is an 0.5 probability for systems in state S_1 to turn into state S_2. What does that mean? 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 accuracy, simplicity, strength, fit, etc. with respect to the quantum-physical events in w assigns probability 0.5 to transitions from S_1 to S_2*. This is not the kind of proposition I would expect to find in the basic laws of physics.

Why not? One reason is that I expect the basic laws of physics to specify relations between fundamental physical quantities. That is why it was never credible that the basic laws could assign a special role to measurements: *measurement* is a gerrymandered, anthropocentric, and not at all fundamental physical kind. But the same is true for *probability* as interpreted by the best-systems account. The theoretical virtues that go into the definition of a best system are not part of fundamental physical reality. Proponents of the best-systems account often emphasize the anthropocentric character of the interpretation, the fact that it reflects our contingent epistemic perspective. Moreover, there are many ways of spelling out the virtues, and of balancing them against each other. It is hard to believe that one of these ways is somehow objectively privileged. On the best-systems interpretation, the precise content of the GRW laws would therefore depend on arbitrary choices in the ranking of theories.

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

Third, consider theories that don't just make outright probability statements. Suppose the best system states 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, such a statement 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 can't be right. Assume H is the true history of the world up to t. By modus ponens, it should follow that the best theory assigns probability x to A at t'. But by assumption, the best theory does *not* assign outright probability to A at t'; it only specifies history-to-chance conditionals.[3]

Fourth, the best-systems interpretation threatens to collapse the important difference between the hypothesis that something is merely true and the hypothesis that it is nomologically necessary. Newton's second law, for example, says that $F = ma$, not that it is nomologically necessary that $F = ma$. But if we interpret $P(A) = x$ as claiming that the best system assigns probability x to A, then the statement can't be true without also being part of the best system and hence a law (on the best-systems account of laws).[4]

Analogous problems arise for the epistemic interpretation. Suppose it is a fundamental law that systems in state S_1 evolve with probability 0.5 into state S_2. On the epistemic interpretation, the law says that one should have degree of belief 0.5 in transitions from S_1 to S_2. Normative psychological notions would figure in the fundamental laws of physics! That is incredible. Again, the relevant propositions also do not seem to be explanatory bedrock: if it is rational to have degree of belief 0.5 in certain events, and this is an epistemically contingent fact about the world (as physical laws are supposed to be), then surely there must be a further explanation of why that degree of belief would be adequate.

One might respond that the epistemic interpretation or the best-systems interpretation should not be understood as spelling out the truth-conditions of probability statements. Instead, they might be taken to "fix the reference": they identify physical probability by a certain role, without revealing the nature of the quantity that occupies the role. That kind of story is plausible for other theoretical terms. Consider inertial mass. Perhaps our

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

4 Note that the arguments I just gave are arguments against the best-systems interpretation of probability terms *as they figure in scientific laws*. Parallel arguments might cast doubt on the best-systems interpretation of the term 'law' as it figures in scientific laws – but I can't think of any realistic case where a scientific law essentially involves the notion of a law.

concept of inertial mass can be analyzed in terms that we don't expect to find in the laws of physics. The analysis might identify inertial mass by its role in our experience of the world – roughly, as the property responsible for the fact that we find some things harder to accelerate than others. The 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). The content of Newton's second law is arguably a proposition directly about that quantity. But if we assume that probability terms similarly refer to a fundamental physical quantity, we are back to the propensity interpretation. Alternatively, we could take the referent to be a non-fundamantal quantity such as $\mu(S)/\mu(\Gamma)$, but that would turn the relevant theories into empirically empty tautologies.

I have focused on statistical mechanics when arguing against the propensity interpretation and on GRW when arguing against the best-systems interpretation and the epistemic interpretation. So how about a mixed approach on which fundamental physics talks about primitive propensities while non-fundamental theories talk about best-systems probabilities or epistemic probabilities? I do not think such a mixed approach would fare significantly better than the unified proposals. Most of the problems I raised for the best-systems interpretation also arise at the non-fundamental level. For example, we could not allow for higher-level history-to-chance theories, we would collapse the distinction between p and *necessarily* p, and we would have to accept that the laws of genetics (say) involve strongly anthropocentric terms. In addition, we would face all the problems of the propensity account on the level of fundamental physics.

I do not claim to have refuted all extant interpretations of probabilistic theories in science. But I hope I have said enough to motivate trying something new.

3 Science without truth

I began with a question: what do 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? I want to suggest that we should reject the question. Probability statements in scientific theories do not express a special kind of fact. They are not meant to be true or false.

The idea is that we broaden our conception of scientific theories. On the traditional realist conception, scientific theories aim to register important truths about the world – interesting and robust patterns in the observable phenomena and in what lies behind these phenomena. The task is straightforward if the relevant patterns are crisp: all Fs are Gs, whenever a system is in state S_1 it will later be in S_2, whenever a phenotype has frequency x in one generation it has frequency y in the next generation. But what if the world is more complex? What if two quantities F and G are strongly and robustly 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 G is determined by F together with other salient features of the situation? We could simply refrain from saying anything about the connection between the quantities. But then we would 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 such a 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, perhaps by adding a noise term to an algebraic equation. The result is a probabilistic model, or theory. The point of the model is to capture the noisy, stochastic relationship between F and G. It is not to capture a crisp relationship between F, G, and third quantity P. This is why we could not find a sensible answer when we asked what that quantity might be: primitive propensity, best-systems probability, rational credence, or what have you. All these answers misunderstand the point of probabilistic models.

When a scientist puts forward a probabilistic model, she commits herself to the assumption that the model fares well, on balance, in terms of simplicity, strength, fit 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 purpose, it is enough that the model contains a probability function. The function does not need an interpretation.

Admittedly, this view of scientific theories may be unfamiliar and therefore somewhat counter-intuitive. We are used to thinking that respectable scientific theories simply represent the world as being a certain way, for example (as I said above) by stating relations between fundamental quantities. On the present account, this is not quite true for probabilistic theories. If a theory "states" a probabilistic connection between fundamental quantities, it doesn't really state anything, insofar as it does not make an apodictic, outright claim about the world.

As an analogy, it may help to imagine scientific theories as agents ("experts") with certain beliefs about the world. On the traditional conception of theories, those beliefs are always apodictic: the expert beliefs that all Fs are Gs, that whenever quantity A has value x, then B 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 B has value y if A has value x. These partial beliefs are not outright beliefs with a special 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: a good expert assigns high degree of belief to true propositions and low degree of belief to false ones. The same is true for probabilistic theories in science. A good theory assigns high probability to true propositions and low probability to false ones.

So that's my proposal. The probabilities in scientific theories do not have an interpretation. As a consequence, probabilistic theories cannot be true or false, except in their non-probabilistic parts. They can, however, be more or less close to the truth, as measured by the difference between the (uninterpreted) probabilities and the actual events in the world. And that is all we need. The point of probabilistic models in science is to provide a simple and informative systematization of noisy patterns in the world. To serve that purpose, the models do not need to be assessable for truth and falsity.

4 Theories, predictions, beliefs

At first glance, my proposal seems to create a host of problems. If probability statements don't have truth-conditional content, how can they be believed, disbelieved or conjectured? How can they be confirmed or disconfirmed by observation? How do we interpret complex sentences that embed statements about probability?

In response, I should first stress that my proposal does not concern the interpretation of probability statements in ordinary language. My topic is the interpretation of scientific models or theories. Arguably, such models are best understood not as linguistic constructions at all. If they are expressed in language, the relevant language will generally involve both terms from natural language and special-purpose technical vocabulary. On my proposal, probability terms should be treated as technical terms, and they should not be given an interpretation. I will say a little more on the interpretation of 'probability' in ordinary English below, but that is not the focus of my proposal.

So the problem with complex sentences only arises for complex sentences within a given scientific theory. That is, what if instead of assigning an outright probability to an event A, a theory merely states that the probability of A is *either x or y*? Or, returning to an earlier example, what if a theory says that *if H, then* $P(A) = x$? Now, on the present account probabilistic theories do not have a classical truth-conditional interpretation. They only need to be evaluated for simplicity, strength, probabilistic fit and other theoretical virtues. So we need to ask, for example, how to measure a theory's fit with respect to actual events in the world if it merely specifies that the probability of A is either x or y. This might be an interesting question to ponder, but it is not a terribly urgent question, since real theories rarely take that form (see note 3 above). Note also that to the extent that there is a problem here, it is equally a problem for the best-systems account, which also assumes that one can evaluate theories for probabilistic fit without yet assigning a meaning to the probability terms.

The issue of confirmation and belief is more serious. However, there is a simple and natural answer. 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. So the scientist commits herself to the truth not of T itself, but of a derivative proposition $\Box T$: that T fares well in terms of accuracy, simplicity, strength, fit and other theoretical virtues. Unlike T, $\Box T$ is an ordinary (albeit vague) proposition. It can be true or false. It can be believed, disbelieved, conjectured, and denied. It can be confirmed and disconfirmed by empirical observations.

So what appear to be propositional attitudes towards a probabilistic theory T are really attitudes towards an associated proposition $\Box T$ – roughly, the proposition that T provides the best systematization of the relevant patterns in the world.

The "relevant patterns" are not just patterns in the phenomena. 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 the entities it postulates are real and whether they display the patterns suggested by the theory. In contrast, to really endorse (say) GRW quantum mechanics, you have to believe (roughly) that the true state of an isolated physical system is accurately and completely characterized by its wavefunction, that the state mostly evolves in accordance with the Schrödinger equation, but that this evolution is occasionally punctured by collapse events 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.[5]

In general, $\Box T$ is closely related to propositions about randomness (disorder) 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? Well, it depends on the precise meaning of the box. For concreteness, let's assume that $\Box T$ states that T is the best systematization of the sequence as measured by Lewis's [1994] criteria of simplicity, strength, and fit. $\Box T$ then entails that about 80 percent of the tosses actually come up heads. For suppose the actual frequency is only 70 percent. Then T provides a significantly worse systematization of the sequence than a rival theory T' that assigns probability 0.7 to heads on each toss. Specifically, T' has much greater fit – the probability of 70 percent heads is approximately 8.7×10^{-4} according to T', but 8.4×10^{-12237} according to T – while it fares 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

5 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 truth in certain respects) of our best theories T, but the truth (etc.) of the associated propositions $\Box T$.

to probabilities at all. Finally, $\Box T$ plausibly entails that right after a heads outcome, the relative frequency of another heads is not too far from 80 percent; otherwise a theory that doesn't treat successive tosses as independent would have greater fit without too much a cost in simplicity.

So there is a tight connection between probabilistic theories and claims about relative frequency and disorder. If a scientist accepts our coin model T, she will 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 attitudes towards T. It is therefore understandable that many science textbooks endorse some form of the frequency interpretation on which probability claims simply are claims about relative frequency.[6]

We can also see what is right about the epistemic interpretation. On the supposition that a theory T provides a good systematization of the relevant patterns in the world (i.e., on the supposition that $\Box T$ is true), a rational agent should generally align her credence with the theory's probabilities. To illustrate, suppose you know that the best systematization of our coin toss sequence is the theory that 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, it is highly plausible that 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. (See [Schwarz 2014] for more details and generalizations of these observations.)[7]

How could you have come to know $\Box T$, without having surveyed the entire sequence? The short answer is: by induction. Perhaps you have witnessed the first 10000 tosses, and found an irregular pattern of heads and tails with about 80 percent heads. All else equal, you would then be justified to assume that the same noisy regularities obtain in the unobserved parts of the sequence.[8]

6 The connection between scientific probability and relative frequency is a substantive fact that any account of scientific probability must explain. It is not a consequence of the laws of large numbers. These laws are theorems of the probability calculus that hold for every probability function whatsoever, no matter how out of touch it is with the frequencies in the world.

7 Note that on the present account, the probabilities most central to statistical testing, the *likelihoods*, are neither frequentist probabilities nor epistemic probabilities, but they are closely related to both. This might have the potential to bridge some of the gap between Bayesianism and frequentism in statistics – but I have to leave further investigations into this matter for another occasion.

8 As Hume pointed out, 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 physical 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]).

In its artificial simplicity, the coin toss example is a little misleading. Real scientific theories typically aim for more than a compact statistical summary of certain events. They try to shed light not only on how the events are distributed, but also on why they are distributed the way they are. Hence the probabilities in scientific theories are often motivated by underlying explanatory assumptions, often about how the relevant events come about. For a simple example, think of the binomial probabilities in the Wright-Fisher model of neutral evolution. These are not based on inductive generalizations from observed frequencies. Rather, they are motivated and explained by internal assumptions of the model. In many areas of science, the probabilities can arguably be motivated by the "method of arbitrary functions". Paradigm examples are models of gambling devices such as roulette wheels and dice. These devices are built in such a way that any reasonably smooth frequency distribution over initial conditions is mapped by the dynamics of the systems to approximately the same distribution over outcomes. The characteristic patterns in the observed outcomes can therefore be explained by the absence of certain patterns in the input conditions. Several authors have recently suggested that considerations along these lines might also justify the probabilities in statistical mechanics and other scientific theories (see e.g. [Strevens 2003], [Myrvold 2012]).

What's important for our present topic is not so much how this or that probabilistic model can be justified, but the more general fact that we expect the probabilities in a model to have some such underlying justification. Among other things, this explains why we tend to hold fixed the adequacy of the models under counterfactual supposition: on the supposition that a fair coin were tossed a million times, we deem it unlikely there would be an unusual pattern in the initial conditions – and so we expect the relative frequency of heads to be approximately 1/2.

5 Conclusion

I have argued that none of the currently popular interpretations of probability yields an adequate understanding of probabilistic theories in science. They all assume that probability claims in science are claims about a particular probabilistic quantity, but it is hard to see what that quantity could be. I have suggested that we should stop looking for that quantity. The point of probabilistic theories is not to capture facts about some probabilistic quantity out in the world but rather to capture noisy relationships between ordinary, non-probabilistic quantities.

On the resulting picture, probabilistic theories cannot be true or false (except in their non-probabilistic parts), but they can still be evaluated for simplicity, strength, fit and other theoretical virtues. To fully accept a theory is to regard it as a good systematization of the relevant facts. Under normal conditions, this implies expecting a close fit between

Unsurprisingly, the verdict then is that this can't be done.

the theory's probabilities and actual (as well as counterfactual) frequencies. It also implies adopting the theory's probabilities as one's own degrees of belief.

So the best-systems interpretation, the frequency interpretation, and the epistemic interpretation are not entirely off the mark. They all misrepresent the content of probabilistic theories, but they capture important aspects of what a rational agent must believe who accepts a probabilistic theory. Probabilistic laws do not say of themselves that they have various theoretical virtues, but accepting the laws plausibly involves believing that they do.

Various problems for the frequency interpretation, the epistemic interpretation, and the best-systems interpretation disappear on the present perspective. For example, we are no longer under pressure to spell out precise standards for measuring and comparing a theory's virtues. We can allow that what scientists value in their theories to some extent varies from discipline to discipline, from school to school, and even from person to person. On the best-systems account, these differences would affect the *content* of probabilistic theories. On the present account, they only affect the content of the associated propositions $\Box T$ that captures what a theorist believes by accepting a theory. There is no reason to think that this has to be a fully precise and objective matter.

What about probability statements in ordinary language? Officially my proposal is neutral on this question. I have some sympathy for the view (defended e.g. in [Maher 2010]) that most ordinary statements about probability are normative epistemic statements. That is, when I say that there is a 90 percent probability of rain, I endorse and recommend a corresponding strength of belief. Since accepting a theory goes hand in hand with taking the corresponding degrees of beliefs to be rationally adequate, we can then see how the ordinary sense of 'probability' relates to the probabilities in scientific theories – and thus why the latter are called 'probabilities'.[9]

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9 Evidently, these remarks are no more than a sketch of the beginnings of an actual semantics for probability statements in natural language; the details are hard (see e.g. [Lassiter 2011]).

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