No interpretation of probability

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1 Chance: believers and skeptics

On the issue of objective chance, there are believers and skeptics. Believers (including, among many others, Popper [1982], Mellor [1971], Lewis [1980], Schaffer [2007], Hájek [1997] and Hall [1994]) hold that there is an objective, physical type of probability, *chance*, which measures the tendencies of physical systems to evolve in one way rather than another. On this view, every carbon-14 atom, for example, has a specific probability of decaying within the next thousand years. This probability is not a measure of our uncertainty or ignorance. It is a genuine physical property of the atom, not unlike its mass and charge, figuring in causal explanations and physical laws.

Skeptics (including, among many others, de Finetti [1937], Jaynes [1957], Skyrms [1980], Jeffrey [1983: ch.6], Hoefer [2007] and Handfield [2012]) do not believe in such a physical quantity. A popular idea among skeptics is that statements about chance simply express lack of information: when we say that there's a 50 percent chance of rain, we are not speculating about the value of a basic physical quantity; rather, we express that given the available information, it would be reasonable to give 50 percent credence to rain. What counts as "available" depends on the conversational context; sometimes the available information is the information we actually have, but sometimes it includes information that could easily be gathered, in some sense of 'easily'. Thus chance is rational degree of belief conditional on (contextually determined) information about ordinary, non-chancy matters.

Also popular among skeptics are deflationist Humean analyses 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* (see [Lewis 1994], [Loewer 2004], [Hoefer 2007], [Cohen and Callender 2009]), 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.

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 statements in scientific theories. Let me explain.

2 Interpreting science

Probabilities play a central role in many theories all across science, 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 the basic postulate of statistical mechanics, which (in a simple formulation) states that for any isolated system in macrostate M, the probability that the system's microstate lies in a subset S of M in the system's phase space is equal to the ratio $\mu(S)/\mu(M)$, where μ is the Lebesgue measure of the phase space. One might think that this tells us quite clearly how the probability statements in statistical mechanics are to be interpreted, namely as statements about ratios of Lebesgue measures. Thus $P(S/M) = x$ simply *means* that $\mu(S)/\mu(M) = x$. But this can't be right. One way to see this is to note that there are countless other measures besides μ that could be used to specify the probability for S given M. In other words, the formalism of statistical mechanics contains infinitely many functions (infinitely many "physical quantities") all of which satisfy the mathematical conditions on a probability function. There are good reasons why statistical mechanics identifies probability (*extensionally*) with a particular one of these functions. The reasons mainly turn on the way statistical mechanics is used to make empirical predictions. If you set up a large number of systems in macrostate M and check how many of them are in S, it turns out that the observed frequency ratio is in general very close to $\mu(S)/\mu(M)$. This empirical fact supports the claim that $P(S/M) = \mu(S)/\mu(M)$, which would make no sense if the claim were true by definition. (In fact, it is often argued that *in order to yield the right empirical predictions*, the measure μ should be conditionalized on the hypothesis that the relevant system started out in certain low-entropy conditions; see e.g. [Albert 2000: ch.4].)

Believers in primitive objective chance might suggest that $P(S/M) = \mu(S)/\mu(M)$ says that the *objective chance* of S given M is $\mu(S)/\mu(M)$. But this is not very plausible, for a number of reasons. First, the probabilities of statistical mechanics lack the forward-looking dynamical aspect allegedly characteristic of chance. A cup of coffee hardly has a propensity to be, right now, in one micro-state rather than another. Second, most formulations of statistical mechanics assume a deterministic micro-dynamics. These probabilities therefore lack another characteristic feature of chance: they are not incompatible with determinism. Third, the quantum mechanical probability that my cup is now in a given micro-state may well be either one or zero, although statistical mechanics assigns non-trivial probability to this event. It is hard to see how a single basic physical probability could take several values for the same token event. Fourth, the suggestion that the probabilities of statistical mechanics are basic physical quantities appears to render them epiphenomenal: since the evolution of a physical system is completely determined by its present micro-state and the fundamental dynamical laws, it isn't sensitive to any further basic physical quantity.

I would add a fifth objection. The reason why physicists identify the probability of S given M with $\mu(S)/\mu(M)$ is patently not that this matches the values of some basic physical quantity. To be sure, the Lebesgue measure of M within S is a well-defined quantity, but (again) it is only one among an infinity of similar quantities, none of which is metaphysically more basic than the others. The choice of $\mu(S)/\mu(M)$ is justified by its formal simplicity together with the fact that it fits the empirical phenomena. Unfortunately, all this applies just as well to (say) the dynamical probabilities in GRW quantum physics. So this fifth objection is actually an objection to fundamentalism about objective chance in general, not just to the fundamentalist interpretation of statistical mechanics.

In any case, many believers in objective physical chance wisely restrict their interpretation to dynamical probabilities in fundamental physics. So they still owe us an account of what all the other theories are saying.

Skeptical interpretations such as the best-systems analysis promise a unified interpretation of probabilistic theories in all parts of science. The only problem is that the interpretation is not at all plausible. The problem I have in mind is especially striking for fundamental theories. Without going into details, let's assume that a fundamental physical theory, which we'll call T, entails that a given carbon-14 atom has a 50 percent probability of decaying within the next 1000 years. What is the content of this statement? What does it say about the world? On the best-systems account, 'chance' is analyzed as 'the probability function that figures in whichever physical theory best combines such-and-such theoretical virtues'. A statement about chance is therefore a statement about the content of whatever theory best combines those virtues. So if T says that the carbon atom has a 50 percent chance of decaying, what it says is that *the probability function that figures in whichever physical theory best combines such-and-such theoretical virtues assigns value 0.5 to the decay.* This is absurd. The basic laws of physics do not quantify over physical theories, nor do they talk about methodological standards like simplicity and strength.

Why is this absurd? One reason is that it delivers implausible truth-conditions. There is a general and important difference between an ordinary 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 best-systems proposal, however, the 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 entail that this statement is part of the best physical theory and thus a law (according to the best-systems account of laws).

That there is something wrong with the proposed truth-conditions is forcefully brought out if we consider theories that don't just make outright probability statements. Suppose the basic laws of T 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 F at t' has probability x. On the best-systems account, what this means is the following: if H is the history of the world up to t, then the best theory assigns probability x to F at t'. That's clearly wrong. The best theory might well be T; and by assumption, T does not assign an outright probability to F at t', even if H happens to be the history of the world up to t.

You might object that no serious scientific theory contains history-to-chance conditionals. It's true that stochastic dynamical theories generally assign probabilities to future states only relative to past (or present) states, but arguably these "conditionals" are better understood as conditional probabilities. I agree. But versions of the present problem occur 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.

Anyway, what's absurd about the best-systems analysis of probabilistic theories isn't just a matter of truth-conditions. We expect the basic laws of physics to specify quantitative relations between fundamental physical properties. The laws should therefore be expressed in a highly restricted language in which all non-logical and non-mathematical terms stand for fundamental quantities. This is precisely 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 chance 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 an event E, this is patently 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.

Don't these problems for the best-systems analysis also arise for other theoretical terms?[1] Consider inertial mass. One might argue that our concept of inertial mass can be defined in terms that we don't want to find in the basic laws of physics (perhaps including terms for conscious experience, as suggested in [Chalmers 2012]). However, on this view it is plausible that the analysis only "fixes the reference". The analysis tells us which actual physical quantity the concept picks out, without specifying the essence of that quantity. Newton's second law then is a statement directly about the relevant physical quantity – as it turns out, the very same quantity that is also picked out by

1 Thanks to Al Hájek for asking me this question.

the distinct concept of gravitational mass.[2] This route is not available to skeptics about chance. On the skeptical account, there is no fundamental physical quantity (or power) to be picked out by the probability terms in physical theories. The only candidates in the vicinity are quantities like $\mu(S)/\mu(M)$. But as we saw, probabilistic physical theories are not charitably interpreted as talking about these quantities; it would rob them of any empirical content.

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. I mentioned another skeptical proposal according to which saying that the chance of an event is x means that a rational agent should assign degree of belief x to the event, conditional on the available information. Clearly this fares even worse 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

Recall our imaginary theory T which assigned a 50 percent probability to a carbon-14 atom decaying within 1000 years. We would like to know what this tells us about the world. What would the world have to be like in order for the statement to be true? I want to suggest that these questions rest on a misunderstanding. They assume that probability statements in scientific theories are meant to represent a special kind of fact. But this is not their point. When T assigns 50 percent probability to the atom decaying, it doesn't make an outright claim about the world. In particular, it doesn't make a claim about the value of some physical or non-physical quantity denoted by 'the probability of the atom decaying'.

I suggest that we should widen our conception of scientific theories. On the traditional realist conception, a good scientific theory simply registers important truths about the world – interesting and robust patterns in the history of occurrent events. But suppose 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. In this case, a theory could simply refrain from saying anything about the correlation 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 an interesting, robust, but *noisy* relationship between two quantities? How can she express this relationship in a scientific model or theory?

2 This may be denied. On some accounts, inertial mass is a fundamental disposition or *power*; roughly, its very nature is to measure an object's disposition to resist acceleration. Gravitational mass might then be a different power, even though the two always coincide.

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 the equation) with the understanding that a probabilistic model of this kind mirrors a noisy pattern of dependence in the world. This will enable her to develop a simple and informative model of the relevant facts. The two virtues, simplicity and informativeness, generally pull in different directions. A good model should then strike a sensible compromise. When putting forward a probabilistic model, the scientist suggests 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 analysis. The analysis starts with a competition between possible scientific theories. At this stage, the probability statements in these theories remain uninterpreted. Thus only the non-probabilistic parts of a theory can 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 to the analysis: without it, we don't have an interpretation of chance. But my topic isn't the ordinary or philosophical notion of chance. Interesting scientific theories don't contain the word 'chance'. They typically contain formulas specifying probabilities for various event types relative to suitable boundary conditions, but we don't have to understand these as statements about chance, or about anything else.

As an analogy, it may help to imagine scientific theories as agents making various claims about the world. We used to think these claims are always apodictic: all Fs are Gs; whenever $F = x$, then $G = y$, and so on. But now we also allow the agents to hedge their claims: if something's an F, it's probably G; if F has value x, then G is more likely to have value y than z. The point of these probabilistic locutions is not to introduce a third physical magnitude besides F and G. The agent doesn't make an outright claim about a three-way relationship between F, G and another physical quantity. Nor does the agent suddenly talk about her own degrees of belief. When she says 'probably p', she doesn't make an assertion with a special probabilistic content. She simply *expresses* her high degree of belief towards p.

I am not, of course, defending an expressivist interpretation of probability statements in ordinary English. Probabilistic theories are not really agents, they don't have degrees of belief, they don't produce speech acts. The analogy is only an analogy. In any case, my proposal has little bearing on the semantics or pragmatics of natural language. Arguably, scientific theories are best understood not as linguistic constructions, but as more abstract "models". In the case of probabilistic theories, this model will include a probability function. My proposal is that the point of this function is not to represent a certain quantity in the world. Even if theories are written down in language, the relevant term to form probability claims is usually something like 'P' or 'p' or 'π' – something that clearly doesn't have a determinate meaning in English.

The idea that probability statements in scientific theories don't have truth-conditional content may raise a familiar worry. How are we to interpret sentences like 'either $P(A) = x$ or $P(A) = y$', or (returning to an earlier example) 'if H, then $P(A) = x$'? As I mentioned above, it isn't obvious that statements like these ever occur in reasonable scientific theories. Anyway, all that matters is that such theories can be evaluated for simplicity, strength, fit and other theoretical virtues. That's the only "interpretation" applicable to probabilistic theories. 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 probabilities. I am not going to put forward precise rules for determining a theory's qualities here, but I don't see any insurmountable difficulties arising from logically complex statements like the above.

4 Confirmation and endorsement

If probabilistic theories don't make outright claims about the world, we need to reconsider our attitudes towards such theories. In particular, we need to adjust some traditional ideas about confirmation. Without truth-conditional content, probabilistic theories can't be confirmed or disconfirmed in the usual sense. They also can't be believed.

On the other hand, consider why it is useful to learn that a given probabilistic theory has high fit, especially if it is also simple and strong. This is useful because it reveals a lot about general patterns in the world. Suppose you know that a coin is about to be tossed a million times, and that the best systematization of the outcomes – in terms of simplicity, strength and fit – specifies an 80 percent probability of heads for each toss, independent of the others. It then follows that about 80 percent of the tosses will come up heads, as otherwise a different probability assignment would have fared better. It also follows that the sequence of outcomes won't have a very conspicuous pattern. For example, it won't be 200000 heads followed by 800000 tails, nor 200000 repetitions of HHHHT. In either case, the best systematization of the outcomes would not resort to probabilities but rather specify the exact sequence. Probabilities are only needed if the sequence is sufficiently irregular. Now consider an arbitrary member of the sequence, say toss 512. Unless you have special information about this particular toss, you should be about 80 percent confident that it results in heads. Moreover, since you know that the outcomes form an irregular pattern, your degree of confidence should be fairly insensitive to information about the outcome of other tosses: assuming that toss 511 landed tails, your degree of belief in heads on toss 512 should still be about 80 percent. These observations easily generalize (see [Schwarz forthcoming]): on the assumption that a given theory fares well in terms of simplicity, strength and fit, a rational agent without unusual further information should normally align her credence with the theory's probabilities.

None of this requires that the theory is true, or even evaluable for truth and falsity. Thus instead of talking about belief (or degrees of belief) in scientific theories, we can speak of two other things which conveniently go together: (i) believing that the relevant theory fares well in terms of simplicity, strength and fit, and (ii) endorsing the theory in the sense of making its probabilities one's own degrees of belief. Accordingly, when we test a probabilistic theory, we test (i) to what extent the theory fares well in terms of simplicity, strength and fit, and therefore (ii) to what extent it shall guide our credence.

Again it may help to think of theories as agents. If an agent has only apodictic beliefs, then trusting the agent means to believe that the world really is the way she represents it as being. But what if an agent gives 60 percent credence to a hypothesis, and you trust her judgment? Then you shouldn't be certain that the hypothesis is true, nor that it is false. Rather, you should also give it 60 percent credence. Accepting or endorsing a theory is like trusting an agent whose degrees of belief match the theory's probabilities.

The practice of scientific confirmation largely conforms to this picture already. The usual method of testing a probabilistic theory is to draw a large sample and compare the sample frequencies with the theory's probabilities. In Bayesian statistics, this is justified by a variant of Bayes' Theorem according to which the posterior (epistemic) probability $Pr(T|E)$ of a theory is proportional to its prior probability $Pr(T)$ times the probability $P_T(E)$ the theory assigns to the evidence:

(1) $$Pr(T|E) = \frac{P_T(E)Pr(T)}{Pr(E)},$$

This follows from Bayes' actual Theorem together with the assumption that the epistemic probability of the evidence given a probabilistic theory equals the probability the theory assigns to the evidence:

(2) $$Pr(E|T) = P_T(E).$$

The only point where all this requires adjusting on the present picture is that what is tested is strictly speaking not the proposition T itself (for there is no such proposition), but the hypothesis $\Box T$ that the T is the best systematization of the relevant phenomena.

Principle (2) thus turns into

(3) $Pr(E|\Box T) = P_T(E)$.

The present observations lend some support to the skeptical proposal I quickly dismissed in section 2, on which statements about objective probability are statements about rational belief. This idea is not unpopular among physicists as an interpretation of statistical mechanics. In section 2, I complained that physical laws shouldn't contain terms from normative psychology. Nevertheless, the account I proposed is in some ways quite similar. If a theory T says that one ought to assign credence x to an event, then obviously testing the theory means to test whether one ought to assign credence x to the event. I have argued that this is just what we do when we test a probabilistic theory. The reason is that what is tested, confirmed or disconfirmed is strictly speaking not T itself, but $\Box T$, the proposition that T is a good systematization of the relevant facts. And this proposition is closely related to the proposition that one's degrees of belief ought to match T's probabilities, as (3) reveals.[3]

It may be worth repeating that on the present account, (3) is derivable in ordinary scientific contexts from independently plausible assumptions about rationality. It is not a mysterious link between a basic physical quantity and rational credence. This brings me to a nice side-effect of the present account.

5 Probability and actuality

In metaphysics, *Humeanism* is the rejection of primitive modality. Humeans believe that truths concerning what *must* or *could* or *would probably* be the case are ultimately made true by truths concerning what actually *is*. The suggestion I have defended does not require a Humean metaphysics. It is compatible with the perhaps non-Humean thought that what is systematized by probabilistic theories is not just the actual history of occurrent events. It is also compatible with the existence of fundamental powers and other non-Humean whatnots. But it supports the Humean perspective insofar as it maintains that the probabilities in scientific theories 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 in general and Humean accounts of chance in particular. Imagine a world that consists of nothing but a million atomic "coin toss" events, 80 percent of which result in heads. On a Humean account of chance, the

3 The epistemic account also brings out an important fact about scientific theories that is somewhat hidden in a best-systems account: good scientific theories typically assign probabilities to "outcomes" A under "conditions" C, where it is much easier to find out whether A obtains than whether C obtains. This fits the idea that objective probability is the credence one ought to have given easily available information.

chance of heads on each toss is determined by the actual sequence of outcomes – in this case, it is presumably identical to the relative frequency. Hence the assumption that the chance of heads on each toss is 0.8 logically 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 half heads and half tails.

Personally, I don't think this problem is as devastating as it is often made out to be. The anti-Humean intuition here is plausibly 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 outcomes has non-zero chance; so every sequence *might come about*. To be sure, it is reasonable to expect that roughly 80 percent of the tosses are heads, but one can't be certain: all tails, for example, has a miniscule but positive chance of $0.2^{1000000}$."

A close connection to rational credence is central not only to the concept of chance, but also to an adequate understanding of probabilistic theories in science. A major advantage of Humean accounts is that they can explain this fact, as we saw in the previous section. Simple chance-credence principles like (2) or (3) are valid in normal scientific contexts (at least to a very close approximation). This is the natural environment in which we acquired our concept of chance. However, the principles fail for very large events or very small worlds, as in the example of the coin toss world. More sophisticated principles that may hold universally (such as the "New Principle" of [Lewis 1994] and [Hall 1994]) no longer licence the entailment from 'positive chance' to 'might happen'. The anti-Humean intuitions are therefore easily explained: they arise from an unreflected application of simple chance-credence principles to cases where these principles are no longer valid.

The present account of probabilistic theories offers another, more charitable take on the anti-Humean intuitions. Return to the coin toss scenario and consider a theory that assigns probability 0.8 to heads. On the account I presented, such a theory entails nothing about actual outcomes. It does not entail that around 80 percent of the tosses come up heads. Indeed, there is a perfectly good 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 probability statements in scientific theories. I have suggested that these statements should not be understood as claims 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, probabilistic 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 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.

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