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Propensities[1]

Lars-Göran Johansson

1. Introduction

Philosophers have long argued about the interpretation of probability. What do we mean when we talk about probabilities? Is it a measure of our degree of belief in propositions, or is it an objective property of events, situations, objects or states of affairs, such as a long run frequency or a dispositional property?

I think the reasonable stance is to say that we mean different things in different situations. We may call many measure functions satisfying Kolmogorov's axioms 'probabilities', and there is no reason not to expect finding many quite different domains of inquiry in which it is possible to construe a mapping from a set of events, objects or propositions onto the compact interval [0,1] of real numbers fulfilling these axioms.

My interest in this paper is in propensities and objective chances. The relation between propensities and chances is intimate. Propensities are attributes of physical objects, whereas chances are ascribed to events. The connection is: if a physical object has a *propensity p* to undergo a change from state α to β, then and only then this change has a *chance p* to materialize. One could perhaps attribute propensities both to objects and events, but I prefer to keep ontology straight by using different attributes for different kinds of objects.

A propensity, in my use of this term, is a dispositional property of a physical object. Under proper circumstances the disposition will result in relative frequencies that can be observed; relative frequencies are manifestations of these dispositions. Observations of relative frequencies give us evidence about propensities; they are not the same as propensities.

My use of 'propensity' is akin to Popper's, (Popper 1990) although not the same. There are three differences. The first is that Popper attributes propensities to physical situations, not to physical objects in isolation. Secondly, Popper views propensities as causes, which I do not. Thirdly Popper holds

[1] I want to thank George Masterton, Tor Sandqvist, John Cantwell and Sven Ove Hansson for most valuable comments on earlier drafts of this paper.

that for example a fair die has a propensity of 1/6 to come up ‘4’, whereas I hold that, objectively, it is either zero or one, as the case may be. This last difference reflects a difference regarding the scope of genuine indeterminateness. I’ll return to that in the next section.

‘Chance’ in my use of the term is not the same as Lewis’ term ‘chance’. Lewis holds that chance is objective single-case probability (1999, 227). This he analyses in terms of objectified credence, i.e., credence conditionalized on actual history and our best theory. His *New Principal Principle*[2], connecting objectified credence, C(|), with chance, P(), runs

$$C(A|TH)= P(A|T),$$

where ‘A’ is any proposition, ‘T’ our best theory of the world and ‘H’ the actual history up till now. In Lewis’ view this principle is an analysis of chance; hence chance for Lewis is an epistemic concept (and thus it is presupposed that T is true) and would change with changes of what we think is our best theory. Since Lewis contemplates the problem that future events might change the frequencies of events and thus the chance for an earlier event, it is quite clear that in his view chance cannot be a physical property of events, because if so, future events could affect earlier events, which would amount to an instance of backwards causation and that is impossible.

My use of ‘chance’ is not like that; In my use of ‘chance’, it is a quantitative property[3] of events that is independent of what we consider our best theory to be. Hence an independent characterisation of propensity (and hence chance), the objective quantitative measure function on physical dispositions, is needed.

It might be argued that talk about propensities, chances and dispositions in general is superfluous metaphysics. What we observe are frequencies and why not simply say that probabilities are relative frequencies? Let’s be sound empiricists. But that is not easily done.

Suppose we identify probability with relative frequency. Relative frequencies can be attributed to finite or infinite sequences of trials. No one has ever performed an infinite series of trials, so, sticking to sound empiricism, we might think of relative frequencies in finite series. But that does not conform to our intuitions. Take for example dicing. Suppose we perform 600 tests of a die and get ‘4’ 114 times, i.e., we have a relative frequency of 0,19. I think most of us would nevertheless say that the probability for ‘4’ ‘really’ is 1/6≈0.167, provided we don’t have any reason to suspect the die is not fair. We believe that if we would continue testing, the relative frequency

[2] In (1999, 243) Lewis introduces this *New Principal Principle* as an improved version of the original principle C(A|HT)= P(A), because he had spotted problems with the old version.

[3] I’m using the words ‘property’ and ‘attribute’ interchangeably and I do not intend any reification of either.

approach the ‘true’ value. So our intuitive idea is that probabilities are the relative frequencies we would get in the long run, i.e. in infinite series of trials. The basic reason is that we conceive of probabilities as *stable* objective properties of objects or situations; if we would identify probability with actual frequency in a finite series, we would often be forced to say that that the probability for e.g. ‘4’ when dicing would change from one series of tosses to the next. But we are strongly convinced that in the long run the frequency will stabilize at 1/6.

However, the formulation ‘the relative frequency we would get in the long run’, means ‘the relative frequency we would get, if we were to perform an infinite series of trials’ which is a subjunctive conditional, the truth value of which cannot easily be settled. The sceptic might ask: How could you know in advance what we would get? Perhaps the long run frequency for ‘4’ is 0.18, while still the die is fair. The natural reply would be something like this. If the long run frequency really is 0.18, the die cannot be fair. All sides of a fair die have equal chances of coming up, so the probability distribution would be uniform in an infinite series of trials.

But then, how do we know that all sides have equal chance, i.e., that the die is fair?

Usually, lacking evidence to the contrary, we start by assuming a uniform distribution. Then, if we encounter evidence against a uniform distribution, we reject the uniformity and the fairness assumption. But if observed relative frequencies are the only means for determining fairness, then the attribution of propensities and objective chances, as something distinct from relative frequency in the long run, is idle.

However, evidence concerning fairness can be obtained independently of frequencies of outcomes. A fair dice is mirror symmetric in three dimensions. This is a categorical property, more or less directly observable. Now, let’s suppose that by observing its symmetries we can convince ourselves that a die is fair, while the long term frequency for e.g. ‘4’ still deviates from 1/6. Could that happen? Yes, of course it could; the initial conditions in the series of trials might not be uniformly distributed. I refer to such things as the orientation and angular momentum of the die at start of each toss, etc. All these conditions are clearly observable.

So suppose further that we have made observations and found that the distributions of these parameters in an actual finite series of trials of a fair die are all uniform. Could it still be the case that the relative frequency of e.g. ‘4’ deviates from 1/6? I would say no, the argument being that the outcome of dicing is determined by initial conditions and deterministic laws, viz., the laws of classical mechanics. If the initial conditions in a sequence of trials are uniformly distributed, the outcomes will also be thus uniformly distributed. I’m here assuming that dicing is a realisation of classical mechanics, a deterministic theory, and that quantum fluctuations have no influence on the dynamics.

Since the initial conditions are observable, we can use classical mechanics to calculate the outcome. Since the outcome is determined by laws and initial conditions, the probability for, say 4, in a single case is either zero or one, as the case may be.

We no longer have a chance event. Thus it is clear that when we call dicing a chancy event, we do so because we have no detailed information about all those factors that contribute to the outcome. By saying that dicing is chancy, we don't imply that the outcomes are *genuinely indeterministic*, only that we have no complete control of the process. That goes also for the use of 'chance' and 'random' in many other contexts, for example in the discussion of causes of diseases. If we compare two persons, similar in a number of relevant respects, but differing in that one got a particular disease and the other not, we are prone to say that it was a mere chance that the one fell ill and not the other. However, few would conclude that there is nothing more to do or say about this disease; most takes for granted that further research is meaningful and it is rational to hope for finding relevant differences between those who fell ill and those who didn't. We see that in many cases 'randomness' means 'lack of information' or 'lack of experimental control'. But not in all cases; there are genuinely random events in nature, viz., indeterministic ones. Or at least, so I believe.

An event is deterministic iff a description of it can be derived from initial conditions and deterministic laws. We are here considering dynamical laws, i.e., laws describing how states of physical systems evolve in time. Such a law is deterministic iff only one state at a particular time is compatible with the law and a complete specification of initial conditions; if more than one state at each point of time is thus compatible, the law is indeterministic. In other words, in the deterministic case the probability distribution over possible states is a singularity; only one state at each point of time is possible.

2. Objectivity and chanciness

Is lack of control or lack of information an objective feature of events? No. One might reasonably say that objectively the single case chance for, say '4', upon dicing is either zero or one, not 1/6, because conditional on the initial conditions and the force situation, the outcome is determined to be either '4' or 'not 4'. But of course, most people would, if queried, say that the probability for '4' when using a fair die is 1/6. Thus we have a reason to distinguish between objectivity and intersubjectivity; Objectively the chance for '4' is either zero or one, but intersubjectively the chance is 1/6.

In many contexts we equate objectivity with intersubjective agreement. This is at least in the present case wrong, if we take intersubjective agreement as meaning 'almost universal agreement'. For if a person has detailed information of the initial conditions of each trial of the die, he would be able to predict the outcome with complete certainty. That means that he would say that the probability for e.g. '4' is either zero or one in each single case, and he would be right and the majority wrong. The most reasonable stance would be to say that most people's *credence* in the proposition 'the outcome of the next toss is 4' is 1/6, whereas the objective probability is either zero or one, depending on the initial conditions.

There is no conflict in saying that objectively, the probability for '4' in a single toss is either zero or one as the case may be, whereas the subjective credence is 1/6.

Lewis' Principal Principle (both the old and new version) tells us that the *objectified credence*, i.e. the credence we ought to have in a proposition, conditional on our best theory and full information about the history up till now, should equal the objective chance, in this case zero or one. This seems perfectly reasonable; in the example just discussed, the objectified credence is either zero or one. The conclusions to be drawn are two: i) we should distinguish between events that are *genuinely* random and those that we usually call random, in spite of being deterministic, due to lack of complete information, and ii) we should restrict the concept of objective chance to events we believe are genuinely random. But are there any such genuinely indeterministic events?

3. Indeterminism, propensity and objective chance

I don't believe the world to be completely deterministic. We have very good reasons to believe that at least in the quantum world there are genuinely indeterministic events, for example decays of individual unstable nuclei, absorption of photons in atoms and state transitions during irreversible interactions.

Predictability entails determinism. (But the converse implication does not hold: there are physical situations that are completely deterministic but not predictable, as observed already by Laplace). So if we introduce the concepts propensity and objective chance we should construe them so as to cover only those genuinely indeterministic events, events such that even if we know everything there is to know about the conditions, the outcome is not predictable.

The distinction determinism/indeterminism is a piece of metaphysics, because whether a system is indeterministic or not cannot be finally decided. The reason is that in order to prove a system indeterministic, we need to know that our theory about that system is complete. For suppose we have a theory having indeterministic laws. In order to know that real systems obeying these laws really are indeterministic we need to exclude the possibility that our theory is incomplete and could be completed with so far hidden variables that determine the seemingly undetermined outcomes. But that cannot be done, for in order to do that we need to somehow directly compare theory and reality as it is in itself unmediated by our observations and concepts. So we are free always to entertain hope that an apparently indeterministic theory sooner or later will be replaced by a deterministic and complete one. So, for example, since Einstein couldn't believe that nature fundamentally was indeterministic, he thought that quantum theory is incomplete (He is reported to have said 'Der liebe Gott würfelt nicht.') and therefore possible to improve.

When it comes to a non-fundamental theory we are often in a position to say that it is not complete or that it is an approximation because we compare this non-fundamental theory with a more fundamental one. But with fundamental theories we have nothing better to compare with. This is the situation as regards quantum theory; it is a fundamental and indeterministic theory. (String theory is in a sense more fundamental, but it is a quantum theory and thus indeterministic.) The other fundamental theory, general theory of relativity, is deterministic, and since we have no other fundamental theories to consider, we can in what follows focus on quantum theory as the sole candidate for a theory in which we have any need for the concepts propensity and objective chance.

The question for us now is if it is possible to define a measure function over genuinely indeterministic events in physical systems described by quantum theory, satisfying the following conditions:

i) it should satisfy Kolmogorov's axioms for a probability measure

ii) It should be possible to derive probability distributions for state changes directly from the axioms of the theory.

If there exists such probabilities I will call them *propensities* when attributed to physical objects and *chances* when attributed to state changes of these objects. Thus, an *object* has a propensity of x% to undergo a particular state change, iff the objective chance for this *state change* is x%. Physical objects have propensities and events have chances.

The point of condition ii) is that it guarantees that probability distributions can be calculated without use of frequencies or credence functions and this is necessary because otherwise are propensities idle; if the only way to acquire information about probability distributions is to observe relative frequencies there is no point in introducing propensities as something distinct from frequencies. Similarly, if we define propensity using credence functions, however objectified, they are objectified credence, which in essence is not a property of objects in the world but of our minds. (Our minds are in a sense objects in the world, of course, but the point is that propensities is thought of as attributable mainly to things that are not minds.) So the question is: how do we determine probabilities for what we believe to be genuinely random events in quantum physics? The answer is easy; the scalar product of two normalised state functions in the same Hilbert space fulfils Kolmogorov's axioms for a probability measure, and these scalar products, expressing transition probabilities, provide the required propensities.

Let's imagine that we have a physical system, isolated from the rest of the world. Its state at time t is Ψ_t and its evolution in time is determined by the time dependent Schrödinger equation. This time evolution is deterministic, no chanciness occurs.

However, sometimes there occurs a collapse of the state, either spontaneously, or induced by some external condition. A collapse is a non-linear, non-unitary, irreversible and indeterministic event. The key feature is indeterminism; the others follow, in the context of quantum mechanics, from indeterminism, as I have shown elsewhere (2007, ch. 6).

The common view is that the collapse occurs if and only if a measurement is performed upon the system. This view, however, faces severe difficulties. From a strictly physical point of view, measurements are but examples of ordinary interactions, i.e., interactions between the measurement device and the measured system. As is well-known, the measurement system could, theoretically, be included in the description of the measured system and this combined system obeys the deterministic Schrödinger equation. It means that it will not collapse. But, observations show that it has collapsed. The way out in the Copenhagen interpretation has been to say that the ultimate cause of the collapse is the observer's change of mind state. But no one could imagine how a change in the observer's mind could cause a physical state change in a physical system outside the observer; hence the state referred to by 'Ψ' is actually not the state of the physical system but the *information state about* the system. Thus the collapse is no real physical change but a change of information on part of us humans.

There are deep problems with this view and few nowadays endorse it. Many physicists and philosophers of science believe that the collapse is a real change of the state of the system, and this collapse has nothing to do with human observations. Others think that there is no real collapse but we can, for all practical purposes, describe the state change as such.

My own view is that we have good reason to accept that collapses occur. Moreover, I have proposed a kind of explanation of collapses; a collapse is a necessary consequence of discreteness of interaction, i.e., of the quantisation postulate (see my 2007, ch. 6). It occurs not only in measurement situations but regularly when two systems exchange conserved quantities, irrespective of one system being a measurement device or not.

However, for the present discussion it doesn't matter which view one takes, so long as one accepts that the collapse is a real state change, i.e., so long as we accept that the collapse of the state function is a genuinely random physical event and is not dependent on human observers.

How, then, do we calculate the probability distribution over the outcome space? The recipe is as follows. The collapse is a state change whose result is one out of several possible states. Each such possible state can be characterised by a state function ϕ_i, i=1,…, n, assuming there are *n* distinct possible states. (It could even be an infinite number of possible states.) These functions could always be chosen so as to constitute a complete orthonormal set of functions, spanning the same Hilbert space in which the original state function Ψ is defined. If this is done, the scalar products $\langle \Psi, \phi_i \rangle$, usually labelled transition probabilities, each give a number between zero and one, provided Ψ is normalised. (This expression could be visualized as the overlap between the two functions and the bigger the overlap, the higher probability, which means that the likelier that the system change its state from Ψ to ϕ_i.

The conditions of completeness and orthonormality together guarantee that the scalar product is a function fulfilling the axioms for a probability measure. So here we have a way of determining propensities without relying on frequencies or credence. Hence, the two conditions on propensities are fulfilled; the axioms for a probability measure are fulfilled and they are defined without using frequencies or credence functions, which means that in any model of the axioms of quantum mechanics we can calculate transition probabilities, i.e., propensities.

Some might think that quantum probabilities are so peculiar so as to cause more interpretative problems than we solve. This is in general true, but transition probabilities are completely classical.

4. Conditional propensities

Paul Humphreys has claimed (1985) that propensities do not obey Kolmogorov's axioms for a probability measure. His argument is that the axioms entail Bayes' theorem, i.e, a formula for inverting conditional probabilities, but propensities are non-invertible. In the case of only two alternative events, B and not-B, Bayes' theorem is

$$P(B \mid A) = \frac{P(A \mid B) \cdot P(B)}{P(A \mid B) \cdot P(B) + P(A \mid \neg B) \cdot P(\neg B)}$$

Humpreys gave some examples, such as this one: when light above a frequency threshold hits a metal surface, electrons will be emitted from the surface. Electron emission is an indeterministic event, so one can say that an electron has a certain propensity *p* to be emitted, conditional on the metal being exposed to light above the threshold frequency. Using Bayes' formula for calculating propensities, one can calculate the inverse, viz. the propensity for the metal being exposed to such light, conditional on an electron being emitted. Since exposure of light above the threshold is a necessary condition for emission, this latter probability equals one, and so the propensity must be the same. But this is not what we get when using Bayes' formula. Humpreys concluded that no formal analysis of propensities that have the following statement as a theorem is adequate:

> *Cond*. If the probability of A given B exists, so does the probability of B given A.

I disagree with Humphreys on this point. I will first discuss his examples and then proceed to a general discussion about asymmetries of conditional propensities.

Humphreys writes (op. cit., p. 558):

> Whether or not a particular electron is emitted is an indeterministic matter, and hence we can claim that there is a propensity p for an electron in the metal to be emitted, conditional upon the metal being exposed to light above the threshold frequency. Is there a corresponding propensity for the metal to be exposed to such light, conditional on an electron being emitted, and if so, what is its value?

Humpreys naturally concludes, since propensity is a disposition, that the latter propensity is unaffected by a later emission of electron from the metal. But this disagrees with the probability value we can calculate using Bayes' formula.

If we try to formalize Humpreys example, we must have him assuming a two-dimensional outcome space consisting of the four outcomes (L=metal exposed to light above threshold, E=emission of an electron): {(L, E), (¬L, E), (L, ¬E), (¬L, ¬E)}. We can now assign a probability distribution over this outcome space and then calculate the conditional probabilities definable in it.

Since exposure to light is controlled by the experimenter, the probability p(L) simply reflects a decision made by the experimenter, viz. which runs to count in the experiment; Thus 'L' is no stochastic variable. The probability axioms do not require that outcomes to be stochastic variables, so there is nothing formally wrong with using this outcome space when calculating probabilities. But of course, it would be ridiculous to identify a frequency determined at will by an experimenter with a propensity of the object. But so would calling this frequency a probability. The very point of calling events probable and attributing them probabilities is that they are not under voluntary control. So the reason why the conditional probability p(L|E) cannot be given a meaningful interpretation as a propensity is not that propensities cannot be inverted, but that L-events are no stochastic variables.

This mistaken reasoning is connected to a conflation of two concepts, viz., conditionals and conditional probabilities, as will be discussed in the next section.

A second problem with Humpreys description of the example is his mixture of macroscopic and microscopic concepts, viz., 'exposure to light' and 'electron emission', and one might wonder if a consistent microscopic description would make his example convincing. So let us take 'L' to mean one single photon, not a huge number, arriving at the metal plate. But what does that mean? A photon has a well-defined position only in interactions, i.e., when it is produced or absorbed; during propagation it cannot be attributed a position like 'arriving at a metal plate', unless it interacts with the metal plate. A mere arrival of a photon is no event that can be identified by any physical criteria. The photon 'arrives' if and only if it gives of its energy to the metal and that is the same event as an electron being emitted. Thus, our two-dimensional outcome space would in this reading collapse to one dimension and no conditional propensities can be defined on it.

In the next section of his paper Humpreys has a more detailed and general argument to the effect that propensities conflict with axioms for probability. Using another physical event, the reflection and transmission of photons on a half-silvered mirror, he states the following regarding propensities: (T=transition, I=impinge, R=reflection of a photon, B=background conditions)

i) $Pr(T|IB)=p>0$
ii) $1>Pr(I|B)=q>0$
iii) $Pr(T|\neg IB)=0$

The last assumption says that the propensity for a photon being transmitted through the mirror equals zero if the photon does not impinge on the mirror. Furthermore he states an independence principle saying that propensity for impinge is independent of transmission and reflection:

CI: $Pr(I|TB)=Pr(I|\neg TB)=Pr(I|B)$.

The assumptions i), ii), iii) and CI together with Bayes' theorem yields a contradiction. So either propensities cannot be identified with probabilities or one of the four assumptions are not true of propensities. I take the latter stance.

In order to analyze the situation we need, as before, to be clear about how Humphreys conceives the outcome space. Since transmission and reflection of a photon exclude each other and also exhaust the possibilities, the outcome space must be

$\{(I, T), (I, R), (\neg I, T), (\neg I, R)\}$

The background conditions are assumed to be stable. They are thus idle in the description of the events in the outcome space and we can suppress 'B'.

As before the problem is with the macroscopic descriptions of what is said to be microscopic indeterminisitic events. The impinging of a photon on a mirror is no event that can be distinguished by any criteria, unless it interacts with the mirror. A photon cannot be said *be* somewhere when it does not interact with the surroundings, as already has been pointed out. So let's try and interpret 'Impinge' as that the photon really interacts with the mirror. If so, Impinge must be identical to the event of the photon either being reflected or transmitted through the half-silvered mirror; thus $I=T\vee R$. If the mirror is *exactly* half-silvered, a photon that interacts has equal chances of being reflected or transmitted. Since $\neg T=R$ and $Pr(I|\neg T) = Pr(T\vee R|\neg T) = Pr(T\vee R|R)$, we have

$$Pr(I|T)=1$$
$$Pr(I|\neg T)=1$$

So the first equality of CI is correct. (But I think Humpreys' motive for CI was that Impinge is *causally independent* of transmission and reflection; this thought surfaces later in the paper in which he discusses causation and probability.) What then about the second equality, i.e., what is Pr(I)?

In order to fulfil CI, we must now put $Pr(I)=1$. This can only be true if we conceive of an experiment where only photons observed to have been reflected or transmitted through the half silvered mirror are counted. Bur if so, assumption ii) is false.

If we on the other hand conceive of a situation in which assumption ii) is true, we must think of more photons than those transmitted or reflected being counted. But if so, the second equality of CI is false. Hence, Humphreys' premises cannot be fulfilled in any consistent description of the type of experiment considered.

Of course, we know very well that probabilities can only attributed to well defined experiments, and this is true no matter if we interpret probabilities as manifestations of propensities or not. If an object has a certain propensity, all by itself, to do something, then, of course, if we want to calculate it we must determine the outcome space clearly.

5. Conditionals and conditional probabilities

However, the general question remains; is it meaningful to talk about conditional propensities? Even though Humphreys' counterargument failed, there might still be problems with a propensity interpretation of conditional probabilities. The first question we have to answer is 'what is the meaning of a conditional probability in the domain of genuinely chancy events, i.e., of quantum state transitions? So let's return to these and discuss in some more detail how to apply a probability measure to them.

A transition is a change from a state Ψ to a state ϕ_i, which is one out of a complete set of orthonormal states. But the same state Ψ could also evolve into a state ξ_j belonging to another orthonormal complete set. For example, a spin half particle may change spin state from spin up in x-direction either to spin up or spin down in z-direction or to spin up or spin down in the y-direction. However, the state Ψ cannot under identical external conditions 'chose' between evolving into a state ϕ_i, or into a state ξ_j, if these two states are eigenstates of non-commuting operators. The choice of the set of possible outcomes is made in the preparation step of the experiment by a suitable arrangement of external potentials. So one can in this case say: "The probability for a spin-half-particle, being in spin-up-state in the x-direction, to change into spin up in the z-direction, is 50%, if the external conditions are such that the particle with certainty will align its spin along the z-direction." So the probability distribution is conditional on preparation, viz., on the *type* of interaction to be performed.

This is, however, not a conditional probability: it has the form 'if B, then prob(A)=x', not 'prob(A|B)=x'. These two statement forms mean different things; it is known that if we equate these expressions we get the disastrous consequence that the probability for any event A is either zero or one (Edgington 1995).

Any quantum system S has a well-defined propensity to undergo a change form a state Ψ to a state ϕ. In order to check this propensity we must prepare a test situation such that S is forced into the state Ψ and then physically acted upon in a way represented by an eigenoperator to the state ϕ. Of course, a mathematical object does not act upon the real physical system; the operator operates on the function representing the real system and this mathematical operation represents a physical action on the system described by that function. This means that the outcome space is determined by the type of interaction to be performed, i.e., by the chosen operator. If we chose another sort of interaction, represented by an operator not commuting with the first, then the outcome spaces in these two interaction types necessarily differ. This means that the propensity for the state change $\Psi \rightarrow \phi$ can only be tested if the appropriate type of interaction takes place. So propensities only manifest themselves as frequencies in certain well defined experiments. But this is nothing peculiar for propensities, the same goes for dispositional properties in general. The solubility of sugar in water can only be manifested if sugar is put into water.

Nancy Cartwright and I both think that such quantum transitions can occur irrespective of the interaction is a measurement or not, while adherents to

Copenhagen interpretation and many others deny that. But this difference is immaterial for the present discussion.

Next, let's return to Humphreys argument that inverting conditional propensities result in absurd statements; so far I have only rejected his examples, not the general statement.

A transition from a state Ψ to a state ϕ_i is one out of a complete set of orthonormal states. The outcome space is the set of such transitions, i.e., $\{(\Psi \rightarrow \phi_1), (\Psi \rightarrow \phi_2), (\Psi \rightarrow \phi_3),\ldots\}$. An event is a subset of this set, for example $\{(\Psi \rightarrow \phi_1), (\Psi \rightarrow \phi_2)\}$. Let's call this event E_{12}, while E_1 is the label for $\{(\Psi \rightarrow \phi_1)\}$. Now we know what we mean by a conditional probability, such as $P(E_1|E_{12})$; it is the propensity for E_1, given that E_{12} occurs. If the distribution over the outcome set is uniform and the outcomes are mutually disjoint, this equals 50%.

Could such a formula be given a meaning in terms of propensities? Does it make any sense to say "The propensity for E_1, given that E_{12} occurs, is 50%"? I think it does. First I see no reason not to say that a system in state Ψ has a certain propensity to change either into ϕ_1 or ϕ_2; it is the sum of the individual propensities because $E_1 \cap E_2 = \varnothing$. Secondly, I see no reason not to calculate the fraction of two propensities, and since $Pr(E_1|E_{12}) = Pr(E1 \cap E_{12})/Pr(E_{12})$, we have given the truth conditions for conditional propensities in terms of things that are meaningful.

What then happens if we invert, using Bayes formula? In this case we get $P(E_{12}|E_1)$, which is the propensity for a system in state Ψ change into either the state ϕ_1 or into ϕ_2, given that it changes into ϕ_1. I don't see any reason to say this is absurd or meaningless and this propensity is obviously equal to one.

So we have here at least one domain in which we believe there are genuinely indeterministic events that meaningfully can be attributed both unconditional and conditional propensities and, moreover, quantum theory provides us with measure functions for state transitions fulfilling the axioms for a probability measure. So I think Humphreys is wrong.

It should now be pretty clear that the reason why Humpreys found it impossible to interpret conditional probabilities as propensities has nothing to do with the nature of propensities, but with the implicit assumption that conditions prepared by the experimenter could be viewed as events in the outcome space and thus used in calculating conditional probabilities. Since those conditions are determined by the experimenter, he can *decide* the frequencies for different preparations. Hence they are not stochastic variables. Such frequencies cannot reasonably be called probabilities, precisely because the experimenter decides them. This is another reason why we need to clearly keep apart statements of the forms 'if A, then p(B)=x' and 'p(B|A)=x'; the conditional can be used to state probabilities under preparation conditions, whereas the conditional probability express fractions between probabilities. It should be pretty obvious that if we equate these things we make a mistake and this conclusion is confirmed by Edgington's proof.

6. The scope of genuine randomness

Are there *other* genuinely random events in nature? According to our present theories, the answer is no.

When judging the scope of the concept of genuine, irreducible indeterminism, our best option is to rely on our best fundamental theories about the world. They are relativity theory and quantum theory, more precisely, the standard model. Like most philosophers I believe that mental events supervene on physical events, i.e., I hold that if a mental change (within a person) has occurred, then there has also occurred a physical change. Supervenience can be stated as the slogan 'no mental difference without a physical difference.' The converse is however not true; many physical changes in our bodies are not accompanied by any mental change. It goes without saying that even biological and chemical changes supervene on physical ones, hence all events, physical, chemical, biological, social, psychological, historical etc, supervene on facts described in these two basic theories of physics. This is the minimal content of physicalism. Put differently, 'If the physicist suspected there was any event that did not consist in a redistribution of the elementary states allowed for by his physical theory, he would seek a way of supplementing his theory. Full coverage in this sense is the very business of physics, and only of physics' (Quine 1981, 98)

Of course, psychologists, historians and other researchers outside physics couldn't care less about physics and mostly they are perfectly right in so doing. But my point is ontological. I need not commit myself to any form of reductionism for the purpose of the present argument.

Now, indeterminism enters quantum theory during collapses and, as Bohr rightly observed, indeterminism is connected with the quantum of action. In situations where the quantum of action is too little to care about, there will be no indeterminism. So, for example, even though it is genuinely indeterminate whether a singular photon entering (or better 'being in the vicinity of') a material surface will pass through, be absorbed or reflected, when we aggregate and consider the proportions of these events when a flash of light hits a surface, there is no longer any indeterminism; Transmittance, reflectance and absorption coefficients, which are properties of huge aggregates of individual atoms, can be determined to any desired degree in advance. And since all chemical, biological and psychological phenomena are built up of basic physical events, there is no additional chanciness in nature. If there were genuine random events describable with for example biological predicates, we should have indeterministic laws expressed in non-reducible biological predicates. But we have not. I submit that all apparent randomness in statistical mechanics, biology, chemistry, psychology etc, is due to lack of complete information. So all genuine indeterminism is traceable to indeterminism in quantum interactions.

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