21 Arguments Against Propensity Analyses of Probability[*]

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1 Analysing Probability

By 1660, Arnauld and Nicole could deploy a recognisable concept of probability:[1]

> ...in order to decide what we ought to do to obtain some good or avoid some harm, it is necessary to consider not only the good or harm in itself, but also the probability that it will or will not occur, and to view geometrically the proportion all these things have when taken together. Arnauld and Nicole (1996, 273–4)

Already the concept possessed some central defining features: (i) probability is a mathematical measure of the possibility of the occurrence of events (ii) it is intimately connected with rational decision making (iii) its assignment to an event is not dependent on the actual occurrence of that event. Call this the pre-theoretical conception of probability: it makes no claim about what instantiates or embodies the probability of some event, it simply states that there is a useful notion that has these features. With the rise of classical statistical mechanics, this pre-theoretical conception of probability was given a home in science. Some minor alterations were made, but it was recognisably a precisification of that same pre-theoretical concept that was being used.

It was only at this point that empiricist philosophical scruples against modality came to bear on the problem of the empirical content of probability,[2] but it would be wrong to think that the only desiderata was to adequately explicate the scientific role of the concept. For the pre-theoretical concept placed a great many restrictions on the intuitions that governed the acceptability of the scientific use of the concept. The platitudes that connect probability with other pre-theoretical concepts restrict how

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[1]Hacking (1975).

[2]Indeed, it was only at this point that a problem with the metaphysics of probability was even recognised: all of a sudden in the mid to late nineteenth century, various kinds of empiricist frequentist or subjectivist/epistemicist accounts of probability were on offer whereas the notion of unanalysed chance was relatively unproblematic (and underspecified) before this time.

much we can treat 'probability' in science as a technical term, to be defined as one might wish.

Rather, as Hájek (1997) has emphasised, we should treat the concept of probability as a target for conceptual analysis, responsive to both scientific and commonsense conceptual economies. We wish to find a concept of probability that makes the scientific use an explication of the pre-scientific use; but this project should not be mistaken for the project of discovering a scientific concept of probability. The second task already has been performed, exactly when we identified scientific probabilities with a normed additive measure over the event space of the theory. But to make this formal structure conceptually adequate we need to give an analysis of both the explicandum and the explicatum.

An analogy might help here: as standardly interpreted by the Kripke semantics, the $\Box$ of **S5** is a precisification of the pre-theoretical concept of necessity. Simply giving us various conditions on the way the box behaves that makes it roughly similar in behaviour to our use of necessity doesn't make it an analysis however. The role of the possible worlds itself cries out for philosophical attention, in just the same way as the original pre-theoretical concept of necessity did. So for example modal realism provides a putative answer to both pre-theoretical worries about necessity and about the precisified notion of necessity in **S5**. I take it that the relationship of (i) an analysis of probability to (ii) pre-theoretical probability and (iii) the Kolmogorovian measure theoretical formalisation of probability, has much the same structure.[3]

Carnap (1962, §3) has a long discussion of what he calls 'explication' of a pre-theoretical concept in terms of a scientifically precise concept. He gives a number of criteria: that the proposed explicatum be (i) sufficiently similar to the original concept to be recognisably an explication of it; (ii) more exact or precise, and have clear criteria for application; (iii) play a unified and useful role in the scientific economy (so that it is not just gerrymandered and accidental); and (iv) be enmeshed in conceptual schemes simpler than any other putative explication that also meets criteria (i)–(iii). These are good constraints to keep in mind. This model is altogether too compressed however: for it presumes that we have an independently good analysis of the scientifically precise concept (in effect, it suggests that scientific theories are not in need of conceptual clarification—that the 'clear conditions of application' are sufficient for conceptual understanding). It also suggests that the explicatum replace or eliminate the explicandum; and that satisfying these constraints is enough to show that the initial concept has no further importance. But clearly the relation between the scientific and pre-scientific

[3]Let us note in passing that as standardly used, 'interpretation' is a misnomer for the activity of understanding the concept of probability. For Kolmogorov gave us an interpretation in the logical sense: a sentence $\Pr(A) = p$ containing the one uninterpreted function symbol $\Pr(\cdot)$ is true just when in the model, Pr is assigned to some additive function P whose domain is a Boolean σ-algebra and whose range is the $[0, 1]$ interval, A is assigned to some member a of the σ-algebra, and 'p' denotes the value $P(a)$ in the model. We already have an interpretation in this sense. When I use 'interpretation' below, it will be in the sense of 'analysis', not in this logico-semantic sense.

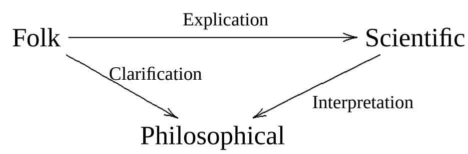

Figure 1: Constraints on conceptual analysis.

concepts is not so one-sided; after all, the folk are the ones who accept the scientific theories, and if the theory disagrees too much, it simply won't get accepted.

The picture as I see it is that these four constraints operate to connect both the scientific and pre-theoretical concepts with their putative analysis, and it is through similarity of the analyses that we can identify the scientific concept as a precisification of the pre-theoretical concept. Both stand in need of clarification or analysis however. I propose that the relation is something like that depicted in Figure 1. (Note that a single folk concept can have multiple analyses, due to the unavoidable vagueness of the folk concept.) I take this kind of approach to conceptual analysis to be *pragmatist* in some broad sense: it emphasises the conceptual needs of the users of scientific theories in understanding the aims and content of those theories.

With a given analysis in hand that satisfies Carnap's four constraints, we can then turn to external constraints on the analysis, not provided by the process of analysis or the concept under analysis. These will be constraints provided by our general philosophical outlook: whether the analysis is purely in terms of empirically acceptable concepts, whether it gives reasonable criteria of application for the concept, whether a concept like that deserves a place in our metaphysics. This is an important part of determining the adequacy of an analysis for a concept that plays a role in some larger conceptual economy. (To give an example: a non-Humean analysis of causation in terms of necessary connections between events might well satisfy all of Carnap's criteria, and yet be rejected for the "occultness" of its posited forces.)

One caveat: it might be better to abandon discussion of the concept of probability and replace it with talk of the relational property of probability. Concepts bear the unfortunate taint of being mental correlates of meanings: and this is far from what I intend to discuss. When I worry about conceptual analysis, I am concerned with giving an metaphysical basis to certain claims of folk and scientific models of the world; not with some mind-dependent presentation of the meaning of the word 'probability'. Traditional conceptual analysis, the replacement of one mental correlate with other 'clearer' mental entities, I confess to finding quite mysterious. The kind of conceptual analysis I favour is closer to uncovering real metaphysical definitions of the concepts in question. In this case, we have some folk concept, which has some explication as a scientific concept, both of which serve as constraints on what real metaphysical property can define and underly facts about the unarticulated concepts.[4]

The overall picture will be to propose an analysis which meets the internal and external criteria as best one can. With these demands on interpretation in mind, let us turn to a family of putative analyses of probability ("propensity interpretations"). This paper will be an extended case study of one putative conceptual analysis in the above sense. We shall see that, slippery creatures though they are, these analyses fail both internal and external kinds of tests of adequacy of conceptual analysis, and must therefore be rejected. Perhaps one can only come up with an imperfect or partial filler of the conceptual role of probability. I will suggest that propensity analyses provide such a poor filler of the conceptual role that we could not accept any of them as an analysis/interpretation.

2 *Propensity Interpretations*

Amongst philosophers of probability (though unfortunately not more widely), the problems with frequency interpretations of probability are well known and generally taken to be decisive.[5] To address many of these problems, Popper (1959b) introduced what he called the "propensity interpretation of probability". This was to avoid some of the main problems of the frequency interpretation; in particular, it was to emphasise (i) how probability depended on the physical generating conditions of a sequence of outcomes; (ii) how probability could apply to the single case; and (iii) how probability is counterfactually robust. But satisfying these three desiderata is not enough to uniquely fix the content of propensity approaches. As with any substantive philosophical project, it soon splintered into many different subprojects. Popper's original paper actually contained hints of all the different forms that the interpretation would take, not clearly distinguished by him, so it still makes sense to talk of propensity interpretations as a family characterised by the motivating remarks he makes.

I shall begin with a general survey, paying particular attention to the three desiderata above; then, following Kyburg (1974), distinguish two primary variants of the propensity approach to probability: the long-run propensity view and the single-case propensity view (which itself divides into two sub-variants).[6]

2.1 *From frequentism to propensity*

The shift from frequentism is initially small but significant. Popper introduces the view as the inevitable consequence of a natural thing that the frequentist (at least, the

[4] Jeff Speaks pressed this point on me.

[5] See Hájek (1997, unpublished), Jeffrey (1977) for many arguments against frequentism.

[6] See also Fetzer (1971, §I).
There are other, more idiosyncratic variants, such as the 'dispositional modal finite frequency' view of Jackson and Pargetter (1982). Some of the arguments below will apply to such views, insofar as they are propensity views at all; almost all of these idiosyncratic variants have other *sui generis* problems.

frequentist who believes in single case probabilities) should like to say : that the admissible sequences of events (for the purposes of calculating relative frequencies) should be sequences of outcomes of repeated experiments, rather than arbitrary ordered collections of events.

Popper thought that propensities were necessary in quantum mechanics, contending that single case probabilities must be physically real relational properties of physical systems, that could interact with each other to alter the probability distributions over events directly, in order to explain interference effects and superpositions.

But, Popper claims, we do not have to delve into the foundations of quantum mechanics to see that the propensity interpretation is needed. Take two dice, one biased $\frac{1}{4}$ towards sixes, the other fair. Consider a long sequence consisting almost entirely of throws of the biased die, interspersed with one or two throws of the fair die. What is the probability of a six on one of the throws of the fair die? According to pure frequentism, since the die tossing event is a member of a collective whose frequency of sixes is very close to $\frac{1}{4}$, the probability is $\frac{1}{4}$. But it is a fair die: so intuitively, the probability is $\frac{1}{6}$. This leads one to try and ensure that this mixed die tossing collective is not a collective: that some kind of objective homogeneity of the experimental apparatus is required also. This was already implicitly present in frequentist accounts of probability, but only as a pragmatic feature about the kinds of sequences that we would wish to accept for scientific purposes. Popper elevates this pragmatic methodological constraint into a metaphysical constituent of the probability concept. Note also that the frequentist cannot explain the compellingness of his methodological precept, whereas the propensity theorist can.

This shifts probability from being primarily a property of sequences, to being primarily a property of the 'generating conditions' underlying such sequences. These conditions are supposed to include the experimental apparatus, (perhaps some subset of) the ambient circumstances, perhaps the outcomes of previous trials; in any case, it is a property of some actual physical entities which is supposed to manifest in each single trial. It differs then from frequentism both in not relying on hypothetical entities like infinite sequences and being well-defined in the single case.

It is natural to take this property to be a dispositional property of the generating conditions.In what follows, I presuppose no particular account of dispositions.[7] The categorical properties of the conditions may well underly and ground the dispositional properties, but it is the disposition of the generating conditions to display a certain outcome just when the relevant kind of test is performed using the experimental apparatus that is supposed to ground probability assignments.[8] This is what Peirce (1910, 169)

[7]Some accounts of dispositions will obviously be more amenable to some views of propensities. For instance, the 'dispositions as powers' view of Martin (1997), Mumford (1998), Shoemaker (1980) might be more amenable to single case tendency analyses. Views like Lewis (1997), Prior *et al.* (1982) where dispositions supervene on categorical properties might be more compatible with frequency views.

[8]Of course, it cannot be the die alone that has the property, but rather the entire die/thrower/surface

calls a 'would-be' of the dice, which he claims is exactly akin to a habit in an agent, and can provide the same explanatory resources.[9] What the different propensity interpretations disagree on is what the firing of this disposition is, and how the disposition relates to the probability.

Note quickly how taking probability to be (or be intimately related to) a disposition yields the three desiderata. To begin, it is a presupposition of the view that probability depends on the generating conditions of the event in question: such views trivially satisfy the first desiderata.

Secondly, if the probability derives from a disposition that is a continuing and stable property of the experimental apparatus, then it will be a property of the experimental apparatus even in the single case: even if the experiment is performed once and the apparatus is destroyed, the disposition will have still been activated and made its display. More carefully: the categorical properties that ground the disposition will have made exactly the same contribution as they would have made in the long run. Perhaps the value of the probability cannot be ascertained by taking the single case as evidence; nevertheless the property of the experimental setup that the probability value describes would still have been wholly present.

Thirdly, since dispositions are supposed to be present even when they are not active (that's what makes them dispositions), they have a certain modal robustness that actual sequences do not have. We can say that even if the dice is never thrown, were it to be thrown under standard conditions, then it would have a probability of $\frac{1}{6}$ to come up six, due to its propensity. Whereas there is no such modal claim to be made about actual sequences and what their frequencies would be, unless one has additional resources available to ground the modality. A dispositional propensity is one such resource.

How territorially ambitious are propensity theories? At least objective physical probabilities are supposed to be grounded in propensities. Whether all other uses are also explained by propensities depends on the individual propensity theory in question.

2.2 Long-run Propensity

The first variant is the *long-run propensity* view.[10] In slogan form:

(Long-run) Event A has probability p iff the experimental setup which generates A possesses a dispositional property to generate A with a characteristic relative frequency p in the long-run of trials of the setup.

In other words, were there to be a long run of trials of the experimental setup, it would be the case that the outcome sequence would have relative frequencies for each pos-

system.

[9]Of course, there are further explanations that can only be provided when we look to the categorical properties that ground the dispositions, in the case of agents they might be psychological states, and in the case of dice, physical symmetries perhaps.

[10]Defended by, for example, Popper (1959b), Hacking (1965), Gillies (2000, ch. 6–7).

sible outcome that define the value of the propensity. This counterfactual is true in virtue of the possession by the experimental setup of some property governing the outcome sequence; it is dispositional because it is correctly ascribed through counterfactual claims. The possession of the property conveys to the experimental setup some power or capacity to generate outcome sequences with certain features.

Just how long is long? It seems fair to attribute to Popper and others the view that since the propensity is operative in every trial, we have an actual basis for describing hypothetical outcome sequences, and that we need not therefore be actualist as far as the long-run goes as well. The propensity will be the non-modal ground of the assignment of a *virtual frequency* to the event-type in the long run sequences of trials of the apparatus. Therefore I take it that the long run is the infinite limit frequency:

$$\Pr(A) = \lim_{n\to\infty} \left(\frac{|A_s \cap S_n|}{n} \right),$$

where S_n denotes the first n members of the outcome sequence S, and A_s denotes the set of all A-events in the infinite sequence.

It then needs to be the case that the sequence of experiments forms a collective in the technical sense: an infinite sequence of outcomes with limit relative frequencies for each event (and often but not always the additional feature of randomness of the outcome sequence). Indeed, the view was largely present already in von Mises, who explicitly claimed that the empirical ground of the laws of collectives appeared in repeated experimental conditions. Nevertheless, the frequencies in this view are seen as evidence for the existence of the propensity which produces them, not by themselves the sole constituent of the probability, even if their values are exactly the same by virtue of the method of measurement adopted (infinite limits). The assignment of a propensity to a kind of trial guarantees that stable limit relative frequencies will exist, produced by the physical interaction of the propensity and other properties of the trial apparatus.

Propensities are, according to Popper, supposed to be thought of properties of repeatable types of generating conditions—this is to ensure that the same propensity is realised at each particular token of an experimental setup. This is what enables us to assign a probability in the single case, despite the fact that the value of that probability is defined by modal facts about infinite long runs of trials. What is supposed to happen is that finite trials and past experience give us a sense of what the propensities are, which then allows us to infer to the systems behaviour in the infinite sequence of trials, and hence to the probabilities of types of outcome. The virtual sequence measures the value of the probability, but its ground lies in the propensity. The propensity doesn't itself possess a value: it is a sure-fire disposition to produce frequencies over the course of the long run of trials. Its role is to ensure that the relative frequencies so discovered will be correctly applicable to the trial in question, so the trial isn't of an inhomogeneous kind that should not correctly have a probability associated with it.

Popper takes it that the introduction of propensities as unobserved entities to explain the magnitude of frequencies is akin to the introduction of forces to explain the magnitude of observable events, like accelerations. Furthermore, just as in the case of forces, the introduction of propensities is supposed to be a empirical physical hypothesis: a propensity doesn't merely tease out the conceptual commitments of probability, but rather posits a particular physical instantiation of probability around here. Nevertheless, in any world in which there are probabilities, there will be some disposition which realises them; in that sense, it is an analysis of probability.[11]

2.3 Single Case Propensity

The application of long run propensities to the single case remains problematic however. Even if we accept that the long run frequency has the value it has because of some propensity type which is token realised in each trial, nevertheless the value of that probability always refers to the infinite sequence, without it being immediately clear what sense it would make to assign a probability value to a single trial.

Popper speaks sometimes as if the dispositional properties which are token instantiated in each trial is in fact the real propensity, rather than a token representative of the long-run propensity. This second thread was taken up, and the propensity was identified with whatever in the actual individual trial was active in bringing about the outcome.

2.3.1 Tendency

The first suggestion is that the propensity is a kind of weakened or attenuated tendency for the generating conditions to cause or produce the outcome when subjected to the right kind of trial:[12]

[11] In what sense is the claim substantive? If the claim is empirically substantive, then there are possible worlds in which it is false, hence there are worlds which it is not true of that the bearers of probability are dispositional properties. Such worlds would be worlds where the bearer of probability is something else. But if this is true, the claim that the propensity account is an *analysis* seems highly problematic. The analysis should yield a relational property sufficient to pick out probabilities in every world in which they occur, and be compatible with every way in which such probabilities might be realised (just as an analysis of pain should be compatible with every possible physical basis for pain). Perhaps then the claim is supposed to be metaphysically substantive. I take this to mean that it provides an adequate analysis of probability in every world where some probability ascription is true of that world. But it must be noted that for this to be substantive, there must be some worlds where there are no probabilities. This commits one to somewhat controversial views about properties (that they do not exist in every possible world, or that relational properties can have contingent relations to monadic properties that they supervene on); it also commits one to the viability of fairly distant possible worlds (where partial belief isn't given a probabilistic analysis, there is perhaps only one event type, perhaps further odd claims). I rather think that the propensity theorist should give up on substantivity in this sense; analyses can still be controversial and epistemically substantive.

[12] Popper (1959b, 36) says:

> [W]e do interpret probability measures, or weights attached to the possibility,as measuring its disposition, or tendency, or propensity to realise itself. . .

See also Giere (1973, 1976), and the later Popper (1990); a close variant is Fetzer (1981).

(Tendency) Event A has probability p iff the experimental setup which generates A possesses a dispositional property (tendency) to produce or cause A to degree p.[13]

The dispositional property is unlike other dispositions, in that it does not always manifest when trialled. It is commonly thought that dispositional properties like fragility can be analysed into an ascription of a modal property of breaking whenever subjected to conditions C. But for this propensity, there will be no list of background conditions such that were the apparatus to be exposed to them, it would necessarily produce some outcome. No fixing of relevant causal factors will render the causal production sure-fire.

The analysis of this weakened causal production relation cannot itself be in terms of probability or frequency, lest the analysis be rendered circular. The frequency data has a role as evidence for the numerical magnitude of this tendency, with successive trials similar in the relevant respects (i.e. with all possible causally interfering factors held fixed) providing a firmer and firmer fix on the exact value of the propensity, as the evidence incrementally confirms some hypothesis about the strength of the causal relationship.

Though the causal claim entails some claims about frequencies, it is not entailed by them. Its analysis must therefore consist at least in the provision of some other truth conditions. One may initially think that the notion of partial causation might help (in the sense that my fatigue and the glare were both partial causes of my car crash), but in fact this is not so. A partial cause is often thought to be part of a complete explanation that would logically entail the event caused if given in its entirety. But even a complete probabilistic causal account will not entail that some particular outcome event occurred. And no account of partial causation has ever quantified the part-cause-of relation in the way that is required for probability.

There remain many possibilities: I will sketch a couple to give the general idea. (i) We could have a counterfactual analysis of the causal relation, and perhaps analyse the strength of the relation in terms of the proportion of nearby possible worlds with the features. A causes E to degree p then obtains when amongst all the nearest A-worlds, E occurs in p proportion of them.[14] (ii) Perhaps we have a law of nature that entails a relation of 'probabilification' between states of affairs, or event types, or events of instantiation of universals, such that it is a metaphysical primitive that it has a degree equal to the probability of the outcome conditional on the trial.[15] (Note both approaches normalise the degree of causation to the $[0, 1]$ interval.)

[13] Compare: "The strength of the propensity of CSU [the chance setup] to produce outcome E on trial L is r" (Giere, 1973, 471).

[14] A naive extension of Lewis (1973).

[15] Perhaps along the lines of Armstrong (1997); for criticism, see van Fraassen (1989). Tooley (1987) uses logical probability (degree of entailment) between propositions about instantiation of universals.

If we take propensities to be causes in any of these ways, we shall have to admit a far wider class of relevant bearers of the propensity than the mere experimental apparatus. We know that causation is a complicated business, and that various factors can be causally relevant, either helping or hindering the production of some event, even when the primary cause is evident. So too, we should like to think that the die is the primary bearer of the property that constitutes the propensity disposition; but that various other factors must collaborate in order for that cause to manifest its correct degree of causation with respect to the outcome in question: the dice must be thrown correctly, gravity must be as it should be, and so on. Popper seems to favour taking the entire state of the universe to be the bearer of the propensity. Giere too abandons a quest that the long-run propensity view perseveres with: to uncover exactly those features of a kind of trial which are statistically relevant for the outcome (i.e. attempting to partition the set of possible experimental apparatus into equivalence classes under homogeneity of frequency outcomes). Rather, the single case in all its particularity is the bearer of the propensity; this is putatively to avoid problems of subjectivity of classification of an event in a particular statistical class.

The view also yields a non-empiricist kind of causation; for we can consider two worlds identical in all particular events, and nevertheless attribute to them different propensities. But perhaps this is not quite right; as I read Giere, he really wants propensities to be a new class of dispositional property that is particularly tied to causal phenomena; so two worlds that disagree on the value of propensities would disagree over some physical fact. It just turns out to be the case that this kind of physical fact is underdetermined by its effects. This makes propensities quite different from Newtonian forces, whose values are constrained by and determinable from the measurable effects on acceleration of specified masses, or by measuring induced current, &*c*.

But the postulation of a physical property enables the neat integration of probability claims into broader scientific contexts. Giere can account for probabilistic independence in terms of causal isolation. He can account for methodological rules governing the choice of appropriate event space by pointing to the serious physical possibilities of the chance setup—in particular, highly unlikely possibilities can still be assigned probabilities despite the fact that in all likelihood they will not occur in the outcome sequence, hence wouldn't form part of the event space in a purely frequentist framework.

We should note too that this view emphasises the role of indeterminism in generating probabilities: for in a fully deterministic world, specifying the entire state at a time is enough to determine or fix completely the future evolution of the system; arguably, the only probabilities will be trivial 1 and 0. Giere uses this as a stick to beat the frequentist, claiming that frequentists assign non-trivial probabilities even in fully deterministic cases, and hence must be subjective and rely on ignorance interpretations of probability. He himself advocates a fictionalist account of macrophysical probability.

In any case, the account has to provide more argument to show how it actually functions as an interpretation of probability: for the physical magnitude of a propensity doesn't automatically satisfy a putative axiomatisation of probability unlike (some) relative frequency accounts.[16]

2.3.2 *Mellor's Distribution Display Account*

The other single case variant is a kind of hybrid view: it emphasises the role of the propensity in being completely fixed by the single case and the actual whole state of affairs, while taking over from the long-run view the idea that a probability is given by a distribution over a partition of the event space, not the individual production of one event from that space.[17]

(Distribution Display) Event A has probability p iff the experimental setup which generates A possesses a dispositional property to produce a probability distribution Pr over a partition including A, where A has the value p in the distribution ($p = \int_A \Pr(x)dx$).

Note that this disposition is sure-fire, producing some event with some value of the distribution in every trial—avoiding some of the difficulties with the truth conditions for chancy dispositions.[18] We should not think, just because a single event cannot reveal the full shape of the distribution, that it is not fully displayed in each trial. That would be to make the frequentist mistake and identify the probability with the evidence.

Note also that the disposition is a property of the individual trial apparatus, *not* a disposition to produce outcomes distributed according to the distribution Pr over the long run of trials of the same type. The actual distribution over outcomes is evidence for the stable continuing underlying propensities of the objects involved in the trials. These stable propensities convey on their bearers very particular capacities to manifest a probability distribution. Hence not just any physical arrangement which produces variable outcomes over time under repeated trials can have genuine propensities (unlike long run propensity views). In particular, Mellor's view is that genuine indeterminism is necessary for propensities.

Mellor begins somewhat differently from the other accounts we have discussed. He thinks that we begin by taking frequencies as evidence for simple theories about

[16]This is at odds with the contention of Kyburg (1974, §4) that the dispositions in question 'fail to add anything to' the hypothetical limit frequency view. Kyburg mistakes the explanatory significance of the postulation of the propensity has for understanding the concept of a reference sequence and understanding frequencies: I therefore disagree with the semantical analysis of propensity statements he gives. I don't think that for Giere or the later Popper the 'almost-certainty' of a frequency claim exhausts the statistical content of a propensity ascription, since frequency claims are almost certain of 'pseudo-statistical' systems as well.

[17]This variant is defended by Mellor (1971, 1995).

[18]Thus, according to Mellor, the tendency views confuse one aspect of the display of the disposition (i.e. the production of some particular event of a partition) with the disposition itself. Note that there may also be some non-sure-fire disposition to produce a particular event; but that will not feature in the analysis of the probability relation.

the events in question, such that those theories can guide our rational expectations (our credences) in the events. He thinks that the construction of such theories will depend heavily on an examination of physical properties of the system in question, especially symmetries.[19] Then through the Principal Principle (Lewis, 1980), we transfer this rational credence to the chance of the event in question, at which point we identify the propensity as just that physical property of the circumstances surrounding the event which makes our credence rational and undergirds the chance. One way to think about it might be to suppose that propensities are the physical hypotheses that best explain our rational credences in cases where there is some objective ground to those credences according to our best theories.

This kind of view can be contrasted with the single case tendency view by suggesting that the distribution displayed supervenes on the other physical properties possessed by the system, in particular, the categorical properties discussed in the theory of that system. The distribution is determined by some arrangement of occurrent categorical properties of the components of the system, and hence is manifested in each trial that subjects those properties to certain interactions. The tendency views postulate irreducible tendencies that do not supervene on any other occurrent properties of the system, but rather are dispositions inherent in the constituents.

Mellor (1995) develops this somewhat differently. He takes the propensity to be the disposition of the chance setup picked out by Ramsifying the description "the property of the chance setup such that it governs the frequency $f_{A,n}$ of outcome A in initial segment of the outcome sequence S_n so that $\lim_{n\to\infty} f_{A,n} = \text{chance}(A)$." This ensures that we identify that property which is probabilistically significant for the outcome sequence. This gives a constitutive connexion between the outcome sequences, the chance and the propensity. This view moves closer to a 'theoretical term' view of probability, especially in the emphasis on the Ramsified description, and perhaps avoids some of the problems raised with the earlier account.[20]

Again, some similarities with the preceding views should be noted. Mellor thinks that an adequate theory of a deterministic universe would have us set the credences to either 1 or 0 if we knew all the relevant evidence; the specification of the complete state of the system means that the propensities are only trivial in deterministic worlds. And again, the postulation of properties that satisfy certain desiderata means that the analysis has empirical content and could be refuted by the non-existence of a class of properties with the features he demands.

[19] See Strevens (1998) for a related view.
[20] See argument **17**.

3 The Arguments

Let us now turn to the arguments. Why so many? A conceptual analysis has to meet typically indistinct criteria for successful interpretation of the starting concepts, so no one argument against any particular (consistent) analysis can be more than plausible. At best, one can make features of a proposed account explicit that are difficult to reconcile either with the internal constraints on the analysis or with other uses of the concept elsewhere. In this case, the number of distinct arguments indicates that there are a number of features of propensity analyses that have one of these problems. I think this will make clear the potential cost of adopting a propensity analysis: that one has to make considerable adjustments to the concept one tries to explicate, and the resulting concept fits poorly into pre-existing roles for the concept of probability. I think that adopting a propensity analysis is thus a difficult and unappealing proposition, and no casual browser of potential interpretations of probability should take this one home. But at least I have left the propensity theorist with a considerable burden to establish a tenable propensity interpretation within this framework.

I begin with arguments that I take to raise problems for each variant propensity analysis, moving then to arguments against each variant in turn.

3.1 Against Propensity Analyses in General

This section contains arguments designed to show that the concept of a propensity is a poor candidate to be used in an adequate explication of the concept of probability. This is both because the work propensities are required to do makes them a particularly problematic kind of property, and because their introduction into the analysis is mysterious. Note that there are no arguments against dispositions in general here. If such arguments in favour of the existence of only categorical properties were valid, then propensity interpretations wouldn't need 21 arguments against them, since they would be non-starters. I take it that dispositions are perfectly legitimate in many cases; I just think that propensities are not.

1. "Establishing the axioms". There is some minimal requirement that any physical property that putatively provides a metaphysical correlate to probability assignments be interpretable as a probability. Typically, this means that the property should satisfy some standard axiomatisation of probability that supports the features of probability required by scientific practice.

All propensity theorists have been at pains to emphasise that what they are advocating is nothing less than a new category of physical property, introduced explicitly to play the correct metaphysical role in understanding science. Christopher Hitchcock has pointed out that there is a significant task for the propensity theorist to explain why some axiomatisation of probability holds of this new physical property.

Consider the pure subjectivist about probability. They have a certain claim, namely that degrees of belief must obey the probability calculus on pain of irrationality. They argue for this claim on the grounds that were one's degrees of belief not internally constrained by the probability calculus, then one would be vulnerable to a 'dutch book': a set of bets on hypotheses, each of which is individually fair by your lights, but that nevertheless leads one to a foreseeable sure loss.[21]

But it is completely unclear what could play the dutch book role for the propensity theorist in justifying the axioms. The propensities either need to primitively satisfy the axioms, or produce empirically accessible phenomena like frequencies or credences which do.

If the propensities are simply to satisfy some axiomatisation of probability in some brute way, that would require some serious empirical work to establish. What guarantee then that the reason propensities satisfy the axioms is a conceptual one—what makes propensities an analysis of probability, rather than simply the empirical bearer of the concept of probability in this world? If propensities are to satisfy the axioms in virtue of some other fact about them, then it needs to be shown that they possess this fact: that, for example, they can produce frequencies of the relevant sort. Again, this will involve investigation into the physical bearers of propensity.

In sum, by adverting to physical properties of system in order to explain their probabilistic behaviour, propensity theories satisfy the intuition that probability has to connect somewhere with the objective situation. But they also fall prey to an additional explanatory burden, namely giving a physical explanation for the obtaining of the mathematical facts about probability. It is difficult to see how they can satisfy this burden while remaining true to the idea that they are giving an analysis of probability, rather than giving a pseudo-scientific account of the particular occurrent facts that make probability ascriptions true of certain parts of this world.

2. "Disunity". Propensity theorists have emphasised that they take physical indeterminism as given by quantum mechanics far more seriously than frequentists did. If propensities are the kind of thing possessed paradigmatically by quantum mechanical systems, then we know they must be properties quite unlike those we are familiar with, e.g. angular momentum or velocity. But we also know that angular momentum and velocity are the properties that the chances in coin tossing cases supervene on. The question immediately arises: what kind of property is it in virtue of which we deploy the concept of probability in both these situations? For they are very different physical situations that both happen to have probabilities attached to them. So probability is a physically disunified concept, and the question should rightly be pressed as to whether propensity (as a putative physical realiser of probability) is disunified in the same way. If it is, it seems that the operation of the property cannot be as straightforward as any of

[21] de Finetti (1964), Jeffrey (forthcoming), Ramsey (1990).

the accounts we have considered describe: there are, at least, no straightforward arguments in the literature about how radically different physical properties are supposed to instantiate the axioms of probability, and surely this is a burden the propensity theorist must try to discharge.

Perhaps the concept of propensity can be 'metaphysically unified', while the physical realisers are disunified.[22] But the problem is not whether there can be multiple realisers of propensities, but whether all those realisers will have enough features in common to be bearers of probability. Given that the probabilistic *theories* give perfectly acceptable accounts of the phenomena in question, the question is whether giving a metaphysically loaded slant to the probability ascriptions of such theories actually aids in their interpretation.

3. "Determinism and Propensity". Of course, one way of resisting the demand for an argument that propensity is really one physically unified concept is to reject the claim that propensities exist in physical situations outside of indeterministic situations. Giere makes this move when he claims that a non-factive analysis of probability attribution in macroscopic situations is the only correct approach. Against this kind of suggestion, there are two points: firstly, that this contravenes the methodological precept we discussed at the beginning, that probability analyses need to be responsive to commonsense intuition. To reject probability for coins, dice, roulette is to clash drastically enough with the pre-theoretical concept that we might think some other concept is really being elucidated (analogy: imagine if we took the lesson of the analysis of temperature as mean kinetic energy to be that, outside of ideal gases, there is no correct assignment of temperature).

Secondly, it is not just a matter of commonsense intuition that is at risk from this move. Classical statistical mechanics proposes non-trivial probabilities, and yet is underlaid by a purely deterministic theory. To deny that these probabilities are 'real' is simply to come into conflict with one of the starting points of any genuine inquiry into the nature of probability: that it should explain the empirical success of probabilistic theories like statistical mechanics. It is a heavy burden on the propensity theorist to explain why these 'pseudo-probabilities', given that they are the best fillers of the role available (as far as explanation and prediction go), should be denied the umbrella of probability.

The later Popper wants to resist this kind of conclusion: he in fact wants to say that macroscopic events are indeterministic.[23] But this seems an ad hoc maneuver at best, one which is not borne out by any analysis of the dynamics of statistical mechanics.[24] I think that the correct response is to follow Clark (2001, 275): "It seems to me that

[22] Perhaps quantum propensities and coin propensities are determinates of the determinable 'Propensity', as has been suggested by Mark Johnston.

[23] See Miller (1996).

[24] See Earman (1986), Sklar (1993).

the issue of determinism versus indeterminism really ought to be (is) irrelevant to an interpretation of probability theory." But this is cold comfort to the propensity theorist who takes one of the prime motivations for their theory to be that it is so dependent on indeterminism. (It should be noted that no help is provided by moving to a quantum statistical mechanics, because of the very different ways that the two kinds of probability enter into the theory.)

4. "Generalised Probability Spaces". Quantum mechanics is a probabilistic theory; but its probabilities may not be those of the familiar classical world.[25] In particular, the additivity axiom does not in general hold: for quantum mechanical observables, $\Pr(A \cup B) \neq \Pr(A) + \Pr(B) - \Pr(A \cap B)$. This doesn't necessarily demand a revision in classical probability theory, but on the most straightforward understanding of how to generate probabilities for quantum observables (as opposed to classical measurement observables), it seems to.

If propensity interpretations are so dependent on indeterminism, and if (as we currently think) genuine indeterminism only arises in quantum mechanics, then propensity interpretations are committed to failing to give an interpretation of classical probability spaces. More cautiously, propensity interpretations have no way of giving an empirical interpretation to the additional structure that classical probability spaces have, because the only empirical resources they allow themselves are sufficient only to constrain probabilities to a general probability space. Furthermore, the aim of propensity interpretations that are so far extant has been to show that they validate the standard Kolmogorovian axiomatisation; but since this axiomatisation fails in their preferred cases, they need to show that propensities satisfy the more general constraints.

A propensity theorist might draw an analogy with non-Euclidean geometries, suggesting that quantum mechanics shows us that the real calculus of probabilities is non-additive, and that additive probability spaces are special cases that arise locally at macroscopic levels.[26] However the cases are dissimilar: a Euclidean space is no less genuine a kind of space, whereas if propensities are quantum dispositions, non-propensity-based additive probability spaces are defective kinds of probability space. The objection is that there seems no way for the propensity theorist to explain how probability could have been genuinely applied to classical probability spaces if propensities correctly interpret probabilities, except by mistake.

5. "Finkish Propensities". Dispositions are malleable things: they can be altered by altering physical aspects of the bearers of the disposition. What if the displays of the disposition are some of the things that can alter the categorical basis? Consider a glass

[25] This is for perhaps two reasons: either because the underlying algebra on the event space is an orthomodular lattice algebra, and not a Boolean algebra; or because of non-commuting operators, there are sets of operators, each of which have individually well defined probabilities, but fail to have joint probabilities. For details, see Dickson (1998); van Fraassen (1991, ch. 5).

[26] The analogy was suggested by Mark Johnston.

that if struck immediately hardens: until it is struck, it is fragile, but once it is struck it is not.[27]

Consider now a finkish propensity: surely a possibility, since we have little information about what propensities are, except that they are a certain subclass of dispositions (and there is nothing that leads us to suppose that this subclass happens to exclude the finks). Whenever a finkish propensity is trialled, the outcome fails to happen, although there is a positive propensity for the outcome event to occur. Or consider a finkish propensity to produce a set of events $A_1 \dots A_n$, one of which (A_i) retards its own occurrence.

This is a special case of causal interference in the manifestation of a disposition, with the special feature that the interference happens to *exactly* cancel the display. We cannot in general assume that the lack of manifestation is a symptom of the absence of disposition to manifest.[28]

Are we then committed to saying that, in this case, the generating conditions are not disposed to produce the outcome event after all? It certainly would tell against a counterfactual account of propensities in terms of their potential displays. But arguably this counterfactual element, while perhaps dispensable in the case of ordinary dispositions, is essential when dealing with a disposition that is supposed to underlie probability. This is because probability is closely connected with possibility. A non-trivial probability for some event means that event is seriously possible; that there is some world where it occurs. But if finkish propensities are possible, then some non-trivial probabilities lose this connection with the modality of possibility.

One possible response is to consider whether the finkish preventer is lawlike associated with the disposition.[29] If it is lawlike associated, then perhaps we have a more complex disposition, not a finker. If it is not lawlike associated, then the connexion between the finker and the disposition is not as tight as needed for the problem: perhaps the finker could be controlled for while the disposition manifests. The problem with the first case is that it is empirically underdetermined. If a coin, biassed in favour of heads, also had a finkish propensity to prevent the landing of heads, such that the frequency evidence was even, we would not postulate any kind of complicated metaphysical setup to account for this. Rather, we would attribute a garden-variety propensity to land heads underlying a probability of $\frac{1}{2}$. The second case fails because it is supposed to be a feature of this disposition that it produces its own preventer: circumstances of controlling for the preventer while allowing the disposition to display seem not to be able to arise,

[27]This is perhaps a counterexample to a straightforward counterfactual account of dispositions. See Lewis (1997).

[28]Imagine a similar circumstance: we have a drug which causes side effect D, and we wish to prevent the side effect from occurring. We could eliminate the tendency for the drug to cause D; or we could add some D-preventer to the drug. These are different causal situations, and different dispositions are active. We should not collapse them into each other.

[29]This response was proposed by Mark Johnston.

hence we do not have the ability to collect statistical data that would support the second case.

It seems that the finkish propensity can have necessarily defective empirical consequences, since every attempt to manifest it yields the wrong evidence for its value. This is surely a problem for a putatively scientific theory: not only are we told that propensities are a new class of physical property, but we now conclude that sometimes there is no accurate evidence of the existence or operation of this property. This objection, that propensity is a superfluous metaphysical posit, will come up forcefully in the discussion of single case propensities below (§3.3); it is not often noted that it can arise more generally.

6. "Conditional Propensities". On a propensity account, conditional probabilities, $\Pr(A|B)$, must be construed as conditional propensities: when the system produces outcome B, then it has a propensity to produce A with a certain frequency, or with a certain degree of tendency, or in a suitable conditional distribution. On reflection this is quite strange. What we require is a conditional disposition: a dispositional disposition. It doesn't seem that a dispositional disposition is anything other than a disposition simpliciter however, with somewhat stronger conditions to elicit display. The disposition to produce outcome A when trialled in a trial that produces outcome B.

Let us introduce for this concept the notation: $\Pr_B(A)$, intended to mean the probability of A in the probability space generated by B (i.e. the events are a σ-algebra of subsets of B). This probability space, I suggest, gives the correct formalisation of the conditional disposition for A given B. For propensity theorists, specifying the physical situation gives the probability space; so there should really be no such thing as conditioning on an event in the event space, but rather the construction of a new probability space based around the newly uncovered physical situation.

If we then stick to our original analysis of conditional probabilities, we are forced to admit $\Pr(A|B) = \Pr_B(A)$. But these two quantities are not in general the same. For by the ratio rule,

(Ratio)
$$\Pr(A|B) = \frac{\Pr(A \cap B)}{\Pr(B)},$$

a well defined conditional probability is the ratio of well defined unconditional probabilities. But for the probability space generated by B, B will not have a non-trivial probability. In its own probability space, of course, it has probability 1: but that cannot be its value in an arbitrary probability space. There is in general no well defined probability for B. An example: there is a great difference between $\Pr(A|B)$ and $\Pr_B(A)$ when B is the event "the coin is tossed" and A is the event of "heads lands uppermost". Obviously $\Pr_B(A) = \frac{1}{2}$; but who knows what value the conditional probability has? This nonequivalence of concepts that the dispositions make equivalent is a problem for accounts that ground probability in dispositions. The propensity theorist only has the resources to account for one kind of conditional dependence of probability; but there are two.

The propensity theorist could argue that this simply means the ratio analysis of conditional probabilities is incorrect. There are independent grounds for thinking this.[30] But it must be noted that this is a significant disagreement with standard probability theory (Kolmogorov, 1956), which propensity theorists have not yet motivated. Furthermore, the kind of conditional dependence they have to favour will set them at odds with others who have rejected unconditional probabilities.

7. "Mathematical Propensities". The law of large numbers states that, with probability 1, the long-run frequency of attribute A_i will have a limit that equals the probability p_i of A_i. Then we have a probability of a probability (treating the first-order probabilities as random variables), which on the propensity analysis seems to commit us to a disposition of a disposition.

That is fine. But the law of large numbers is a mathematical fact. Hence any disposition involved in making it true is a mathematical disposition. Since mathematical facts are widely supposed to be necessary, a straightforward modal account of such dispositions cannot be correct, unless dispositions that are always firing can be countenanced. A causal account cannot be correct either since mathematical truths are the wrong kind of thing to be a relata of the causal relation. And the methodology of Mellor's approach seems not to get a handle on this situation. Propensity theorists are left with an open question as to how to understand probabilities of probabilities.

8. "Quantification". Some people think that non-trivial probabilities apply to quantified sentences: 'The probability is 0.9 that all ravens are black'; 'the chance of there being a white raven is 0.02'. But exactly what kind of propensity is there to make these sentences true? For the long run propensity, the problem is very difficult: as it stands, these sentences (e.g. 'all ravens are black') get made true all at once, eternally, so there is no sense to be made of repeated trials of them. So there cannot be a long run for these propensities to manifest.

So we must have a single case view. But they too face problems: what kind of thing are the 'generating condition' for this event? Perhaps it is the entire universe; or maybe just the initial conditions plus the laws of nature. Can such things be properly said to have a disposition to make it true that all ravens are black? Can there be a causal tendency analysis of the probabilistic truthmaking relation, as this seems to require? These questions remain wide open.

Of course, perhaps such 'events' as all ravens being black do not properly get to have probabilities, and the use of probability and chance in the sample sentences is purely epistemic. But consider if the relative frequency amongst all nomologically

[30] See Hájek (2003b).

possible worlds of worlds where all ravens are black was 0.9: that would seem to be evidence of some kind that the laws of nature, indeterministic though they might be, entailed the truth of this universally quantified statement 0.9 proportion of some measure over the initial conditions. This might well form the basis of an objective probability assignment; one that we would be correct to demand that propensity theories explain.

9. "The Reference Class Problem". Reichenbach (1949, 374) says:

> If we are asked to find the probability holding for an individual future event, we must first incorporate the case in a suitable reference class. An individual thing or event may be incorporated in many reference classes, from which different probabilities will result. This ambiguity has been called the problem of the reference class.

The problem is, given some particular event, which type of outcome should we classify it within to determine its probability? The reference class problem is typically taken to be a problem for frequentist analyses, where the 'type' of outcome determines the reference class and sequence to which the event belongs. Consider the event of a man's death in full particularity: his is presumably the only death to satisfy all and exactly these particulars. To get a probability, we need to generalise away from these particulars, to fix certain factors and vary others. For von Mises (1957) the single case chance of a man's death was 'meaningless'; for other frequentists, the single case chance was the chance of dying for a man qua smoker, the chance of dying for a man qua regular swimmer, &*c*. The obvious problem is that competing reference classes yield different probabilities, with no reference class standing out as the 'correct' one. Not only does the event seem to have no determinate unconditional probability, but there is no guide for the rational agent to assign one based on evidence, despite many attempts to provide one.

Propensity theorists had hoped to avoid this problem by arguing that a complete specification of the physical situation, including all the propensities in question, would contain all the statistically relevant features of the situation, and would thus uniquely classify each single event into a probability space specified by the overall statistical import of the set of propensities.

There are in fact two problems with this, corresponding to each of the main propensity variants. Since they both aim to show that the reference class problem is a problem for propensities too, I include them here rather than in the more specific arguments.[31]

Long run propensity theories immediately inherit the reference class problem from the frequency analyses. The frequency interpretations have consistently failed to supply principles that would allow a unique reference class to be determined. The conse-

[31] See Hájek (2003a) for arguments that *every* interpretation of probability faces a reference class problem.

quent relativisation of probabilities to different sets of trials or sequences will immediately carry over to propensities whose values are fixed by those trials or sequences.[32]

For single case propensity views, it is a little more tricky. There are two kinds, those which take the single case to have probabilities in virtue of non-propensity physical properties (like symmetries), and those which do not. For the first kind,[33] Hájek (2003a) argues that propensity will be inevitably relativised to a chance setup: the particular symmetries in question will end up determining the relevant probabilities. Consider symmetries as partitioning the outcome space; then different sets of symmetries provides different partitions, and combining probabilities from different partitions can lead in familiar ways to contradiction. For the chance set up of a repeated coin toss with outcome set $\{\mathsf{HH}, \mathsf{HT}, \mathsf{TH}, \mathsf{TT}\}$, one theory of the system will give propensity $\frac{1}{4}$ to each. But a different theory might take it that the relevant symmetries are permutation invariant outcomes: and the outcome set is then $\{\mathsf{HH}, \mathsf{HT}, \mathsf{TT}\}$, each with propensity $\frac{1}{3}$.[34] It will be then dependent on the theory what the propensity is; even if we take it that the first theory is correct, there are situations (in quantum mechanics) where theories of the second sort are acceptable. In any case, propensities are not brute, but rather are relative to the theory used to assign them to the experimental apparatus, and relative to the partition on possible outcomes that the theory introduces.[35]This is in addition to the fact that the event-types in question are relative to a set of generating conditions.

For the non-symmetry based, non-supervening theories, like Giere's, which are irremediably single case, there is another objection, due to Howson (1984).[36] This is what we might call the generalisation failure objection. The usual reference class problem is taken to be a problem about how to assign an individual event to a reference class. If however brute single case chances are taken as primitive, there is a converse problem: how to classify the statistically relevant properties and gain information about *other* trials from this one. Howson points out that to generalise the single case probability to a class of similar events, one needs to abstract away from some of the particular detail, while holding fixed the statistically relevant properties of the trial. But *the notion of holding fixed makes no sense in the single case*. Everything is (triv-

[32]One hope remains: that if propensities form a distinctive metaphysical kind, we could provide non-probabilistic criteria for deciding which propensities were present in a given physical situation. This seems to be part of the idea behind specifying the generating conditions: the hope is that the probabilistically relevant features will thereby be fixed. However, this will not help. Firstly, there doesn't seem to be any means of detecting the presence of propensities apart from the probabilistic phenomena that the system enters into. Secondly, the epistemic problem about how to decide which propensities are relevant for the determination of the appropriate reference sequence remains unanswered. Blithely asserting the existence of unique 'generating conditions' won't help, because for long run propensity interpretations, every possible set of generating conditions will define a long run of some sort, with some defined probabilities for the events that occur in it.

[33]Canonically, supervenience based theories like Mellor (1971).

[34]Note that the relative insensitivity of single case propensities to frequency evidence helps keep this second symmetry theory viable in the face of the data.

[35]Indeed, any counterexamples to the Principle of Indifference which can be resolved by appeal to theory can yield a counterexample to reference-class independence for symmetries. See van Fraassen (1989, 303).

[36]See also Howson and Urbach (1993, 346).

ially) held fixed; there is no generalisation. The single case view solves the reference class problem only by trivialising it: everything has its own unique reference class. No rule is even envisaged to tell me how to apply the inevitably partial knowledge I have about similar cases to this one.

Hájek (2003a) takes the lesson of these failures to be that conditional probability, conditional on a reference class or set of background conditions, is the fundamental notion, and we can only talk of unconditional probability when context fixes a conditioning event. I think this is roughly correct;[37] but nevertheless, it must be noted that very few propensity theories as they currently stand are formulated as based on axiomatisations of conditional probability.[38] Indeed, part of the very motivation for many propensity theories was the idea that they could give us the unconditional probabilities that frequencies could not provide (i.e. independent of reference sequence). If they cannot, then one advantage over frequentism is lost.

3.2 Against Long-run Interpretations

10. "Frequentism Revisited". The long run propensity view is closely tied to frequency interpretations. As we shall see, this will be its downfall. Part of the problem for frequencies is avoided because of the introduction of the generating conditions to undergird modal claims about probability. But frequencies fail for reasons other than their lack of counterfactual invariance, and these other reasons will carry straight over.[39]

We have interpreted the long run view as committed to infinite virtual sequences of outcomes within which to calculate the probability of an event. There are several problems with this view:[40] (i) Order matters for infinite sequences: the infinite sequence $0, 1, 0, 1, 0, 1 \ldots$ can be re-ordered $0, 0, 1, 0, 0, 1 \ldots$; the limit frequency of 1 changes from $\frac{1}{2}$ to $\frac{1}{3}$. It may be replied, that the temporal performance of the experiments gives a preferred order; regardless of whether this is so, it seems that something extrinsic to the generating conditions, namely, when they are activated, determines the frequency, while the probability is determined by no such thing. One might also wonder what feature of the generating conditions it is that constrains the temporal order of the non-actual trials! (ii) Given that these views rely on independent and identically distributed trials (but see argument **11** below), lots of sequences are possible. For a coin toss, the sequence $\mathsf{HHHHH} \ldots$ is possible; so is the sequence $\mathsf{THTHHTHHH} \ldots$; so is the sequence $\mathsf{HTHHTTHHHHTTTT} \ldots$ (i.e. 2^n heads followed by 2^n tails), etc. All of these sequences are possible, but all give the wrong answer. The first two give H a

[37] I have some reservations concerning whether we ever get to discharge the conditioning event in order to assign a direct unconditional probability, as it seems we must in some cases, for example when deliberating.

[38] Popper (1959b) is one exception; see also his axiomatisation of conditional probability (Popper, 1959a, Appendix *iv).

[39] There are lots of sub-arguments in this argument, some of which are independently devastating to this view.

[40] See Hájek (unpublished).

limit frequency of 1, even though T occurs infinitely many times in the second case. The third gives no limit frequency at all; the frequency oscillates between $\frac{3}{4}$ and $\frac{1}{2}$ for heads, but never settles down to a stable value. But now we have admitted that the long run frequency might be any value; the physical properties of the generating conditions which underlie the propensity don't fix the (hypothetical, counterfactual) value of the propensity. (iii) Perhaps at this point the long run propensity theorist will point to the abnormality of these sequences, and instead want to use the law of large numbers to show the typicality of the sequences with the right frequency. Firstly, this appeal is blatantly circular: for it presupposes that we have an independent grasp on the probability that appears in the LLN. Secondly, as Eells (1983, 419) points out, there doesn't seem to be any other non-circular constraint in the vicinity. Once we admit that the disposition is not to exceptionlessly produce sequences with the right limit frequency, we have left it open why there should be any statistical regularity at all in the sequences that are produced by trials of the apparatus. (iv) Finally, the limit frequency view is mathematically inadequate as an interpretation of probability: limit relative frequencies violate countable additivity, and they are not necessarily even defined over a field.[41]

So we might retreat to a more charitable position: that probability is the relative frequency amongst a finite virtual sequence; perhaps even the actual sequence of events. Appearances aside, this would not be more charitable. (i) How long a finite sequence? There seems no privileged place to stop in the absence of the constraint that the trials be actual. (ii) Certain probabilities of events are inaccessible: for instance, finite frequencies are restricted to rational probabilities. But quantum mechanical probabilities can be real valued. (iii) Rounding errors: let a fair coin be (virtually) tossed an odd number of times. Then the probability will not be half. We can also generate spurious dependencies the same way. Call A and B dependent iff $\Pr(A) \neq \Pr(A|B)$. Then we can get dependence without causation, so-called 'spurious dependence': simply consider a virtual sequence of 10 B's and 7 A's. Since these numbers are relatively prime, it is impossible for them to have non-trivial equal probabilities, hence they must be dependent; but we showed this without regard to the content of A and B.[42]

11. "The Jeffrey Problem(s)". With regard to hypothetical frequency interpretations of probability, Jeffrey (1977) makes the following point. Such interpretations are committed to the meaningfulness (indeed, the truth) of claims about what the coin would have landed were it to be tossed: namely, that it would have landed about $\frac{1}{2}$ heads were it tossed infinitely many times. Set aside worries about this particular counterfactual: focus on the idea of counterfactuals about chancy situations at all. For if we think the coin toss is chancy at all, then we should also think that there is no fact of the matter about what the coin would have come up.

[41] See van Fraassen (1980, 184).
[42] Further arguments are given against the finite frequentist position by Hájek (1997).

A very similar problem arises for long run propensity views. These views are committed to there being objective constraints on the space of possibilities: that there is some definite answer to what would have manifested if *this* coin were tossed an infinite number of times. But to give a definite answer as to how a chancy coin would behave is to misunderstand chance.

The long run propensity theorist could reply here that the standard Jeffrey problem doesn't quite apply. For it is perfectly compatible with there being no answer about how this coin would land on the next toss that there is an answer about how it will behave in the long run. Our intuitions about lots of counterfactuals allow for global constraints even when it is completely unclear what the local results will be (e.g. if a close election were to be held again, we may not know which candidate would win, but we know it would have been someone on the ballot).

Now, the previous argument (**10**) tried to show that in fact there will be no determinate global fact about the hypothetical sequence either. But grant that there is; there remains a further tension. The long run frequency constrains the overall frequency of the outcome. Chancy modality seems to be accommodated, because each particular outcome could have come about differently. But there is a global constraint which means that despite appearances, these outcomes couldn't vary too much: for they can at best permute the outcomes so as to leave the frequency unchanged. So if we consider some sequence S, and alter the first n members to all 1's, that places a constraint over the remaining members to 'make up the difference' in frequency that the alteration of the sequence made. In fact, then, the aberrant parts of the sequence impose a constraint over the other parts. If we consider that long run propensities are individuated by statistical properties of their displays, and if we consider them to be physical properties, then this very physical system could not have had a relative frequency other than the one it did have, so that the constraint is very tight indeed.

This seems to indicate that first, the trials are not *independent*, and second, the trials are not *chancy*.[43] But these are basic constraints that need to be satisfied if we are to have an interpretation of physical probability.

12. "Long run dispositions". There is a worry about what exactly it means for a disposition to be active in the long run of trials, but not through being active at every instance of the trials (remember, this is not a single case view). This is a problem about ascribing physically potent dispositions to any abstract entities like kind of trials. Perhaps a kind of trial then cannot be an abstract object, but some temporally extended object, perhaps the fusion of all of the separate trials. But if the propensity is in fact not active in the single case, the propensity cannot be identified with any local disposition of each trial. Either the propensity is itself a fusion of the dispositions of each trial; or the kind

[43] Note that chancy here doesn't necessarily mean indeterministic. Even in a deterministic coin-tossing system, we should expect that counterfactuals as to what would happen if the coin were to be tossed are to be standardly evaluated in such a way that there is no determinate answer.

of trial must endure through time, with the propensity fully present at each moment. Both of these options seem controversial at best!

So maybe the disposition is like a law of nature, constraining the possible sequences but not by causing the outcomes, but rather by constraining the space of possibilities. But how a supposedly physical property could act so as to by itself constrain the space of possibilities is quite mysterious. And if it is a law of nature, then there are laws of nature for every kind of experimental setup which evidences probabilities. But we see no such laws in our best physical theory.

3.3 Against Single Case Interpretations

13. "Non-Humean". Grant that our world has single case propensities which give rise to some of the observed features. Since single case propensities are not identified with the frequency evidence, there is a certain amount of flexibility as to what outcome sequence the propensity will produce. Then the values of the propensities could have been different and the observed features could have been exactly the same. Therefore the observed features do not fix the propensities. Single case propensity is non-Humean—it does not supervene on the arrangement of local observable matters of fact.[44]

Reichenbach (1949, 371) argues that because of this flexibility, the occurrence of any single event has no power to verify the assignment of any given probability to that event. Quite rightly this narrow verificationism has been rejected: the occurrence of a single event can incrementally confirm or disconfirm some probabilistic hypothesis.[45]

But the empiricist spirit behind Reichenbach's argument remains appealing. Some contemporary metaphysical projects, in particular the project of Humean Supervenience of Lewis (1986), have retained this empiricism by requiring that local matters of fact about events at spacetime points ground every other fact. Frequencies do supervene in the right way; single case propensities do not. In virtue of this failing, they seem not to be able to connect in the right way with other roles for probabilities. In particular, the use of probabilities to ground rational credences seems shaky unless the single case probability can connect with the expected run of events over time (see argument **14**).

The single case propensity theorist might respond: since propensities are real properties, it is a matter of local occurrent fact whether one is present or not. This is not a problem for Humean Supervenience, since propensity forms part of the supervenience basis.

Now consider two distinct worlds that have exactly the same propensities for some

[44] Single case propensity also violates the constraint of Tooley (1987, ch. 4) that truthmakers for actual truths be themselves actual—the actual truth that this coin has a propensity relation of strength $\frac{1}{2}$ to produce heads will be true despite the failure of one of the relata to exist if the toss comes up tails.

[45] See Fetzer (1971, §II).

events, and yet differ in the outcome sequences that occur. It seems we need to specify both the propensities and the actual events to specify the supervenience base of that world. But if we need to specify all the local occurrent events to specify the world, then the further specification of propensities seems superfluous. It is difficult to see any further fact which doesn't supervene on the local occurrent matters of fact, except for the facts about propensities themselves. Propensities seem therefore to be introduced purely to supervene on themselves. Perhaps in the construction of a partial theory for the world the postulation of propensities gives an explanatory advantage. But it is difficult at best to see what genuine metaphysical significance this kind of propensity can have in this kind of broadly empiricist metaphysical framework.

Many single case propensity theorists aren't metaphysical empiricists, so this argument won't faze them. But they must make some concession to empirical constraints in the realm of statistical hypothesis testing, and here it should be noted that many of the problems with frequentist analyses of probability can recur for frequencies taken as evidence for probabilities. If frequencies are inadequate to be probabilities, then these inadequacies can appear when they are contrasted with the genuine probabilities of a statistical model. We can get spurious correlations and spurious agreements with the proposed propensity model of some situation, correlations and agreements which can lead to the propensity being accepted on poor evidence ('false positives'). It seems we are left without a reliable test methodology for propensity assignments.[46] Perhaps this epistemic risk is a necessary concomitant of all genuine scientific inference. Then the project of characterising reasonable statistical inference is one that must be attempted by all propensity theorists; to date, not all of them have discharged this responsibility.

14. "Horizontal/Vertical Problem". A severe problem for the single case chance theory is that it fails to meet the minimal criteria of providing a guide for rational expectation.[47] The semantics for single case propensities in Giere (1976) involves a uniform distribution over the set of all alternative possible worlds whose history up until now matches our world. The first problem is how to justify a uniform equipossibility assumption for all possible worlds (rather than one based, for example, on a Lewisian similarity metric which would make closer worlds more probable). Set that technical issue aside.

The more significant problem is how this probability across different worlds at the same time is to apply to *this* world at different times: for example, how does it apply to future frequencies? Severing the constitutive link between frequencies and chances means that we have no logical connection between the concepts of probability and

[46] See Eells (1983) for more on this theme.

[47] The canonical exposition is van Fraassen (1989, 84–5); see also Clark (2001), Eells (1983), Lewis (1994), Loewer (unpublished), Salmon (1979). It is raised by van Fraassen and Lewis as problems for the Armstrong/Tooley style account of nomic relations between universals, but applies to propensity interpretations more generally.

rational expectation. Since, as we have seen, the events that occur in a world and the chances of those events are not logically related, why should knowledge of the chances tell us anything about which events to expect to occur? There seems no way that these single case propensities can rationalise adherence to Lewis' Principal Principle or anything like it; but without the Principal Principle we have no link between the two major uses of probability. Lewis says: "Don't call any alleged feature of reality 'chance' unless you've already shown that you have something knowledge of which could constrain rational credence."[48]

This problem actually appeared earlier (argument **9**). Consider each event in its full specificity. We already decided that there was a difficulty in generalising from the single case to any particular outcome sequence. One might dissolve this worry by just accepting that every event is *sui generis*. Then each particular event provides no constraint on any of the other events, not even which events are members of the relevant comparison class that its propensity gives information concerning. There is no constraint on rational expectation provided by the propensities, because there is no information about which events other than itself it can be taken to apply to. (The failure of single case propensities to generalise correctly is due to the fact that single case propensities aren't closely enough connected to the larger pattern of outcomes.) Simply, this shows that rational expectation and propensities can come apart in a way that rational expectation cannot come apart from probability—so propensities are not probabilities.

3.3.1 Against Tendencies

15. "Humphreys' Paradox". This problem is devastating for views which take propensities to be weakened or intermittent causation. This is because causation fails simple inversion theorems of the probability calculus.[49]

Consider Bayes' Theorem; let $\mathfrak{B} = \{B_1, \ldots, B_n\}$ be a partition of the outcome space, and A some event. Then for each $1 \leqslant k \leqslant n$:

(Bayes) $$\Pr(B_k|A) = \frac{\Pr(A|B_k) \cdot \Pr(B_k)}{\sum_{i=1}^{n}(\Pr(A|B_i) \cdot \Pr(B_i))}.$$

The most natural interpretation of conditional probability as a propensity is to consider the conditioning event as a type of experiment, and to consider the propensity of the conditioned event in that experiment. Humphreys considers an experiment with an electron source, a half-silvered mirror, and a receiver. There is an overall probability of electrons passing through the entire apparatus; there is a further probability of the electron hitting the receiver given that it passed the mirror, and this is most naturally

[48]Lewis (1994)
[49]First pointed out in Humphreys (1985); see also Milne (1985).

construed as a conditional probability. But even if it made sense to consider the event of transmission through the mirror to have a propensity to bring about electron receiving events, the converse does not hold; the receiver does not have a propensity to bring it about that the electron was passed through the mirror. Or at least, the propensity should be 1 because an electron passing through the mirror must occur in order for the receiver to activate. But the inverse probability will not in general be 1. So there is an asymmetry in propensities as causes that is not present in probability; so probabilities cannot be propensities.

The point is simple: the interpretation of probability should not require actual backwards causation for every well defined inverse probability!

Some attempts have been made to rescue propensities from the paradox.[50] But these have mostly relied on weakening the causal conception of a tendency to a conception of propensities as tendencies for a system to produce outcomes. The argument goes, in Humphreys's argument the propensity for the electron hitting the receiver given it passed through the mirror is a propensity before it passes through the mirror. Thus the propensity is for a system prepared in that initial state to have the events S and D co-occur, both at some future date, and both causally relevant to the current state.

This approach however will not help the single case tendency propensity theorist: for they want to interpret propensities as directly efficacious between physical states and understand that causally. Either they fail to interpret the probability calculus, or they weaken their position to some kind of 'chances of co-production' interpretation of propensities. But this latter interpretation is subject to the problem of not providing much more than a redescription of the probability calculus, and the physical meaning of the interpretation is lacking.

Recall argument **6**, which claimed that the kind of conditional probability that propensity theorists are best able to capture is that of an event conditional on an event space. Humphrey's paradox seems to indicate that orthodox probability theory is committed to a kind of conditional probability which is not so closely connected to the physical realisation of the generating conditions. Christopher Hitchcock has pointed out that the propensity theorist really can't claim that their analysis of conditional probability is right, for their analysis doesn't seem to be able to explain how Bayes' theorem is true of probabilities—and Bayes' theorem is non-negotiable for the probability calculus.

16. "Causal irrelevance and non-locality". An extension of the last problem. Let A have a propensity to cause B, and let C also have a propensity to cause B, but be causally isolated from A—say, at spacelike separation, so that A and C are both in the past light cone of B, whereas A appears in neither the past nor future light cone of C, and vice versa.

[50] See McCurdy (1996).

So $\Pr(B|A)$ and $\Pr(B|C)$ are both well defined; let us assume that the unconditional propensities are well defined also. In general, the inverse propensity $\Pr(A|B \land C) \neq \Pr(A|B)$. So C has some propensity significance for A, despite the fact that C is causally isolated from A. So the tendency that propensities have to produce events cannot even be a causal tendency in this setting, unless the causation involves faster than light backwards causation; or causal influence from causally isolated events. Either way, this is difficult to accept.

17. "The method of pure postulation." One way that the defender of tendency propensities could avoid many of the problems we have discussed so far is simply to stubbornly assert the existence of irreducible *de re* probabilistic dispositional properties as part of the fundamental furniture of the world.[51] I must admit, I have no knockdown argument (but see the next argument) against these people's robust sense of ontological entitlement. But I think that the more methodologically modest of us would baulk at such a method.

Surely this method of stipulation has all the advantages of theft over honest toil. Unfortunately, as in all such cases, merely positing the existence of such a new category of physical property will fail to establish the existence or uniqueness of the properties in question. At the minimum any such claim should involve at least some empirical research. Admittedly, we have some evidence for the existence of such a category, in the usefulness and applicability of probability itself. But such evidence is inconclusive at best, as the existence of other interpretations of probability shows. It is certainly not enough to establish on conceptual grounds alone the truth of any contingent existence claim about the kinds of properties which feature in the physical world.

One way to escape this charge is to suggest that nothing substantive has really been said. Consider some probabilistic theory, characterised by a class of probabilistic models. Some of the properties of the objects in the model, it will be claimed, are propensities. There must be some property that underlies the objectivity of assignments of probability; let that property be henceforth dubbed a propensity. This kind of response looks promising.[52] Unfortunately, the propensity theorists we have looked at are not content with merely picking out some theoretical entity by a description, but have proceeded to give substantive analyses based on it. These analyses have presupposed that 'propensity' picks out a non-gerrymandered class of properties, unified by their kinds of causal powers and by their relations to certain kinds of categorical properties of display events.

This however cannot be right. At best this shows that propensities can be an explication (in Carnap's sense) of the pre-theoretical notion. The identification of these features of probabilistic models with the concept of propensity and hence the construc-

[51] David Chalmers pressed this option on me.

[52] Indeed, some of those influenced by Mellor's use of Ramsifying have taken chance to be defined as a theoretical term in just this way: see Levi (1980, 1990), Lewis (1980).

tion of a propensity *analysis* is a further task. That this is so can be seen by looking at frequentist analyses of the very same probabilistic models: they replace the probabilistic properties of objects with non-dispositional properties of a different kind of object altogether. So this strategy cannot ground a propensity analysis.

But just as dubbing whatever property that some drug has to put one to sleep, 'dormitive virtue', fails to tell us anything new about that property, so dubbing some physical property 'propensity' gives us no grounds for an substantive claim whatever about that property. (Given the existence of grue-like predicates, even the claim that 'is a propensity' picks out some unified class of properties is debatable.)

Suppose that frequentism is true. Then there is a property of a class of events that underlies ascriptions of objective chance: the property "forms a collective which is objectively random". Then frequentism is a kind of propensity analysis! Or rather, a 'propensity analysis' in this sense is just an objective analysis of probability, nothing more.

Because it bears this trivial relationship to probability, no proposed feature of propensity can *explain* any of the features of probability. The attribution of any particular features to a propensity is illegitimate if it is introduced in this purely nonconstructive way. The fact that probability has a pre-existing content doesn't help to pin down propensities either, since part of the task of analysis is deciding how much of the pre-existing concept can survive in a philosophically rigourous framework. The semantic content of the description to be Ramsified remains just as unclear as the pretheoretical concept, and is no advance over it.[53] This kind of 'propensity analysis' is itself at best a placeholder for a fully spelled out analysis that the propensity theorists have as yet failed to give us.

In sum, the method of pure postulation either ends up in the position where substantive facts about probability are simply claimed to hold without argument, or no analysis has been given. One is methodologically unsound, and the other doesn't begin to address the question of analysing probability with which we started.[54]

18. "*De Re*?" If propensities are *de re* probabilistic properties, then their bearers must be *res*: i.e.objects. But there are sometimes too few, and always too many, objects of a probability ascription. To begin, propensities are properties of generating experimental setup. A sensible thought about a complex experimental apparatus is that its properties should supervene on the properties of its parts. So the propensity of the whole apparatus must supervene on the properties of the parts, so the propensity won't be a simple irreducible property after all. If we deny the supervenience thesis, and argue that probability is an intrinsic emergent property, then the bearers of this primitive *de re* property aren't at all like the entities we usually ascribe such properties to. A property

[53]Recall the first section: conceptual analysis is not merely a matter of mechanically deconstructing the concept.

[54]Thanks to Daniel Nolan for clearing up my thinking on this point.

of a complex that nevertheless doesn't supervene is a property to be very suspicious of in any case.[55]

It is even more suspicious when we consider that sometimes we ascribe chances of coming into existence, say when we consider whether fluctuations in the ground state of quantum field theory will happen to coordinate in such a way as to produce a particle. This has a well-defined probability; but it has no bearer, since the natural entity that has a chance ascribed to it doesn't yet exist, and may never.[56]

3.3.2 Against Mellor's Distribution Display Account

19. "Distributions Displayed?" Mellor's account requires that dispositions manifest each time they are trialled, so he requires that the probability distribution appear on each run of the experiment. However, it is deeply unclear what it means for a probability distribution to be completely displayed in a single trial while not having its full shape revealed. Perhaps it is that the physical property which the distribution function supervenes upon is completely displayed. But this begs the question as to how this physical property uniquely underlies *this* distribution, if the single case propensity is to be counterfactually independent of other instances. And if the distribution is somehow metaphysically fixed in some other way then Mellor owes us an answer as to what feature determines the distribution. I understand what it means for a frequency distribution to be partially displayed in each trial: each trial completely manifests the underlying feature that supports the frequency assignment. I don't understand what else might be happening in Mellor's case than this, and I don't understand how his account can avoid the same problems.[57]

20. "Subjectivity". In his criticism of Mellor, Salmon (1979) makes the point that the detour through subjective probability to ground objective chance is problematic. For in the absence of an accepted probability distribution, the only constraint on rational subjective credence is coherence with the axioms of probability. Whether this is empirically reasonable can only be found out after some constraint is placed on the subjective credences; and the only resource we have while constructing the theory is the frequency evidence. But of course it is possible for the frequency to arbitrarily diverge from the probability introduced in the theory and still be evidence for it; there is no logical link between the evidence and the reasonable credence. Furthermore, in the absence of an analysis of probability, it is not even possible to quantify the possibility of the divergence of frequency from probability so as to reassure us that the problem is never very bad (e.g. by using the law of large numbers). For all we can know on the basis of coherent credence, the only evidence we have in probabilistic theory construction might

[55] Chris Hitchcock suggested this worry.
[56] Karen Bennett suggested this general worry.
[57] This argument is dual to argument 12. Thanks to Alan Hájek for discussion.

be arbitrarily far from the actual value of probability we try to ascertain.

21. "Subjectivity again". The detour through rational credences makes for another problem. Recently a number of authors have criticised the adequacy of the standard probability calculus for credences: some have wished to move from perfectly precise probabilities to interval-valued or 'vague' probabilities (van Fraassen, 1990, Walley, 1991); some have wanted to reject countable additivity for credences (de Finetti, 1974), and have thus rejected conglomerability (Schervish *et al.*, 1984). If the arguments of these authors are accepted, then credences obey a related but different probability calculus. If we need to go via credence to get chance, the chance will also inherit these features. But chance does not have these features, and it would not play the role that it actually does in science if it were to have these features.

4 Conclusion

What I hope to have shown is that despite their promise, propensity interpretations face a number of difficulties. These difficulties derive from details about each particular implementation, as well as from very general features that all the implementations share. The diversity of arguments provided against these analyses indicates that there are problems with propensity as an analysis of probability for a broad range of philosophical positions concerning empiricism, laws and chances: so broad a range that I doubt that all the premises of these arguments can be coherently maintained at one time. But my aim is not to defend the individual premises, but to maintain the conclusion that propensity interpretations are untenable.

Of course, some of the arguments are more compelling than others, which itself indicates a fall back position for the defender of propensities as an interpretation of probability. This position will be dictated by exactly which arguments one finds convincing, and hence the premises of which one will deny. On this front, it seems to me that the best chances for a viable propensity interpretation will involve repudiating empiricist demands for a straightforward non-metaphysical interpretation of the disposition display and of the truth-makers for probability propositions. This may result in a propensity interpretation that construes propensities as primitive *de re* probabilistic causal powers of relational arrays of individuals. I think that if this is the best hope for a propensity interpretation of probability, then that is enough for a *reductio*. But even if it is not a *reductio*, it does place quite strong constraints on what type of propensity interpretation can be maintained. It should at least be dismaying how much philosophical baggage one has to accept in order to analyse probability in terms of propensities.

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