PROBABILITIES, CAUSES AND PROPENSITIES IN PHYSICS

EDITED BY MAURICIO SUÁREZ

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PREFACE AND ACKNOWLEDGEMENTS

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CHAPTER 1: INTRODUCTION

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The present volume collects ten essays by philosophers of science and physics on three inter-related themes: probability, causality and propensities. The discussion centres on modern physics and, in particular, on the pre-eminently probabilistic branches of physics in our time, quantum and statistical mechanics. In spite of the technical nature of most of the papers, this is a collective effort in the philosophical foundations of physics, and of science more generally. In other words, it is essentially a book on the foundations of science rather than its application, and its main aims are conceptual, philosophical and methodological. In this introduction I provide a summary and a philosophical defence of some of the claims made in the book. The introduction is not meant to back up all of the specific claims made by the different authors (nor can it be understood as endorsement, particularly since some of the authors disagree with, or at least qualify, some of the claims I have made in my own work). Instead it is meant to underscore the importance of the topics on which the authors focus their analytical gaze, and their detailed development of these ideas.

The book is divided into three sections each devoted to one of the main themes. Thus the first part contains three essays devoted to probability in science; the second part contains four on the nature of causality particularly in quantum mechanics; and the final part contains some essays on propensities again mainly in quantum mechanics. In spite of the diversity of aims and interests, there are some common themes running throughout the book. In particular there is agreement in general on the following four joint themes or theses (N.B. not all authors would agree with all four): i) An emphasis on taking probabilities in physics to be objective features of the world as opposed to degrees of belief; ii) A correlated emphasis on the importance of transition probabilities – i.e. probabilities for objective changes of physical state – over merely conditional probabilities; iii) An additional reluctance to interpret all objective probabilities in any one of the traditional ways (actual or virtual frequencies, single case or long-term propensities); and finally iv) A general tendency to identify various causal commitments and presuppositions in foundational physics – including in several cases the causal relation between underlying dispositional properties, or propensities, and their empirical manifestations in terms of probability distributions.

The first three sections of this introduction review the contents of each of the parts of the book, always with an eye on these four interrelated philosophical themes. Then in sections 4-6 I develop my own philosophical understanding of these four theses, relating them to previous discussions in the literature, particularly the literature on probabilistic causation, causal inference, and dispositional properties. Section 7 draws some conclusions and provides some pointers for future work.

1. PROBABILITIES

The first part of the book contains papers by Guido Bacciagaluppi on transition probabilities; Sorin Bangu on the principle of indifference; and Roman Frigg on the typicality approach to equilibrium. All these papers concern the nature of probability as it appears in science, mainly in physics. I next provide a brief summary of their main results, with an eye on the particular themes that run through the book.

Transition probabilities and time-symmetry

In Chapter 2: "Probability and Time Symmetry in Classical Markov Processes" Guido Bacciagaluppi argues that time-symmetric transition probabilities can also be employed to represent typical examples of time-directed phenomena. Therefore transition probabilities, even if representing the chances of possible changes of physical states, can neither entail nor ground an objective distinction between past and future. To a first approximation, this implies that defenders of tensed theories of time and other philosophers inclined to deny the reality of becoming need not fear the concept of transition probability: it is not an essentially time-directed concept although it may of course be used to represent processes that are fundamentally directed in time. (Later on in section 6 of this introductory essay it is argued that Bacciagaluppi's thesis may have interesting implications regarding the nature of the propensities that might underlie transition probabilities).

Bacciagaluppi follows the usual definition of transition probabilities in terms of Markov stochastic processes. Roughly a process is Markov if the probability of any state at any given time is dependent only on the immediately preceding state; all previous states are statistically irrelevant. For a stochastic process this entails roughly:

$$P\,(S(t_{j+1})/S(t_j)\ \&\ S(t_{j-1})\ \&...\&\ S(t_1)) = P\,(S(t_{j+1})/S(t_j)) \qquad \text{(MP)}$$

where $S(t_j)$ is the state of the system at time t_j, and so on.

This equation is a simplified version of Bacciagaluppi's equation (3), where I have made explicit the dynamical properties of states, identifying them by means of time index variables. I have then kept states in the variable range of the probability function – as opposed to placing them in the subscript.[1] And I have represented a static probability,

[1] Bacciagaluppi's terminology employs the technical notion of an n-fold joint distribution, which is standard in the literature on stochastic processes (see e.g. Doob, 1953). According to this terminology, states 1 to n appear in the subscript of the probability function, and time indexes in its variable range. We then consider the n-fold joint probability distributions that the n states define over the time indexes. This terminology is more convenient for the derivation of technical results but it strikes me as less intuitive, at least for the purposes of this introduction.

when in a stochastic process each probability more generally carries a time index too – which determines the values of the probability at that stage of the process. Equation (MP) hence expresses a kind of statistical independence: the state at any given time is statistically independent from any previous state, conditional on the state just prior to it. In the language of contemporary theories of causal inference, the state at time t_j, $S(t_j)$ screens off the later state $S(t_{j+1})$ from any previous states $S(t_{j-1})$, ..., $S(t_1)$. In this simplified terminology the concept of transition probability can be expressed concisely:

$$P_{j+1/j} (S (t_{j+1}) / S(t_j)) = P_{j\&(j+1)} (S (t_{j+1}) \& S(t_j)) / P_j (S (t_j)) \qquad \text{(FTP)}$$

The equation expresses the *transition probability* that a system will physically undergo a change *from* state $S (t_j)$ at time t_j *to* state $S (t_{j+1})$ at a later time t_{j+1}. We may refer to this as a *forwards transition probability* (FTP) since it expresses the transition probability $P_{j+1/j}$ *from an earlier to a later time* of a change of state $S (t_j)$ into a state $S (t_{j+1})$. [2] (FTP) may be contrasted with the expression for the backwards transition probability (BTP), i.e. the probability of the same change of state *but from the later to the earlier time*: [3]

$$P_{j/j+1} (S (t_{j+1}) / S(t_j)) = P_{(j+1)\&j} (S (t_{j+1}) \& S(t_j)) / P_{j+1} (S (t_j)) \qquad \text{(BTP)}$$

Forwards and backwards transition probabilities need not be equal, and typically they are not. A stochastic process that is fundamentally time-asymmetric would normally establish different forwards and backwards probabilities for the same change of state. For instance a process directed ‘forwards’ in time would set one or zero backwards transition probabilities, while setting forwards transition probabilities between zero and one *for the*

[2] These notions are again expressed in my own terminology. The notation of n-fold distributions has, undoubtedly, an advantage at this point since it allows us to distinguish the concept of *symmetry* of the transition probability from the concept of *detailed balance* (see Bacciagaluppi’s section 3, where it is also claimed that under standard conditions these concepts are equivalent as statements of time-symmetry). But the distinction plays no role in this introductory essay which focuses instead on conceptual issues regarding objective probability.

[3] So, importantly, a backwards transition probability is *not* the forwards transition probability of the time-inverse of the state change: $Prob_{j/j+1} (S (t_{j+1}) / S(t_j)) \neq Prob_{j+1/j} (S (t_j) / S(t_{j+1}))$, with $t_{j+1} > t_j$. The latter is rather a different transition probability altogether, belonging to an entirely different Markov process.

very same change of state. A process directed 'backwards' in time would do conversely. If the forwards and backwards probabilities for all changes of state are equal, then the process is time-symmetric in a robust sense. More specifically, if all processes are time-symmetric then a consideration of the probabilities defined for the world-dynamics (i.e. the probabilities for all the changes throughout history of all the states of all systems in the world) would leave the direction (the 'arrow') of time completely undetermined. There would be no way to pick out a particular direction of time from any transition probabilities. Although such ideal and abstract world dynamics is not helpful in modelling any particular stochastic process, it does show that there is nothing in the concept of transition probability *per se* that contradicts time-symmetric fundamental laws. In other words, we may also define genuine transition probabilities in worlds endowed with fundamentally time-symmetric laws.

In the main section of his paper (section 4), Bacciagaluppi considers and rejects three different arguments that may be raised against this conclusion. These arguments purport to show that transition probabilities do in fact conflict with time-symmetric laws and, therefore, require a direction of time. Roughly they go as follows. First, there is the argument that ergodicity on its own defines an arrow of time because it entails that most systems will tend towards equilibrium. In our case this should mean that the stochastic process will tend to equilibrate in time, i.e. that it will tend to define identical and hence symmetrical probabilities for all state transitions in the limit (or to put it another way its single time n-fold distribution $p_n(t)$ becomes time-invariant in the limit). This seems to require asymmetry at some point in the process before equilibrium is reached. Second, there is the idea that, at least for some common processes, backwards transition probabilities fail to be time translation invariant. Consider decay processes where the probability of decay from an excited to a ground state in unit time is finite. Finally, there is the thought that backwards transition probabilities are not invariant across experiments with varying initial distribution, i.e. experiments where the initial time series data differs.

In all these cases transition probabilities seem to conflict with time symmetric laws because a fundamental distinction seems to emerge between forwards and backwards transition probabilities. Yet since we have just argued that the concept of transition probability itself cannot be used to introduce any fundamental time-asymmetry, it follows that these arguments must employ additional assumptions. It is to be expected that these assumptions are responsible for the conflict with time-symmetry and Bacciagaluppi argues convincingly that they reduce to the same mistaken presupposition in all three cases, namely: that the calculation of transition probabilities is to be worked out on samples that are *not* in equilibrium. In such cases the inference from the frequencies in the sample to the transition probabilities will yield an apparent time-asymmetry. However, once the samples have been 'cleansed' in order to generate 'unbiased' ones, the apparent time-asymmetry disappears. There is an interesting philosophical insight buried in this argument, which I shall take up briefly later in section 4 of this essay.

The principle of indifference

In the second chapter, Sorin Bangu reconsiders the role of the *principle of indifference* in the ascription of probabilities with a particular emphasis on its use in physics. Keynes first stated it as follows: [4] "The *principle of indifference* asserts that if there is no known reason for predicating of our subject one rather than another of several alternatives, then relatively to such knowledge the assertions of each of these alternatives have an equal probability". There are a number of well known arguments against the principle, many of them taking the form of counterexamples, or paradoxes. Typically these counterexamples show that the application of the principle leads to several inconsistent probability ascriptions to the same event. The so-called Bertrand paradoxes stand out: on the basis of geometrical considerations, and under several assumptions of continuity and smoothness of the probability density, they show that the principle of indifference leads to inconsistent probability ascriptions. A very simple version due to

[4] In the *Treatise on Probability* (Keynes, 1921) which traces it back to Bernouille's application of the principle of sufficient reason. For discussion see also Gillies (2000 chapter 3).

Van Fraassen is often discussed. [5] Consider a factory that produces cubes of length *l* up to 2 centimeters. What is the probability that the next cube produced has an edge ≤ 1 cm? A straightforward application of the principle of indifference yields probability = ½. But, we could have formulated the question in several different ways. For instance, what is the probability that the next cube has sides with an area ≤ 1 cm^2? The principle now yields the answer ¼. And how about the probability that the next cube has volume ≤ 1 cm^3? The answer provided by the principle is now 1/8. These are all inconsistent with each other since they ascribe different probabilities to the occurrence of the very same event.

More generally the principle of indifference employs a problematic inference from our epistemic situation of relative ignorance regarding the outcome space of a stochastic process to a definite probability ascription over the various outcomes. The inference is problematic in just the way any inference from ignorance to truth is problematic. [6] But in addition there is a sense, which I discuss in the second part of this introduction, in which the principle may invite an illegitimate inference from a merely epistemic fact about our knowledge (or lack thereof) to an objective fact about the physical world – more in particular about its dispositional properties.

Bangu agrees that there is at least a priori no reason to support the principle, and he does not attempt to provide new arguments to support it. His aim is rather to contest two other arguments against the principle, a classic argument by Hans Reichenbach (1971 /1949), and a more recent one by Donald Gillies (2000a). These arguments attempt to show that the principle is not an a priori truth, and is moreover redundant even as a contingent truth about the correct adscription of probability values in specific situations. In other words the principle is not even a necessary tool or condition for the practice of probabilistic inference. Or, to invoke Reichenbach's own terminology, [7] the principle of indifference can neither be *validated* a priori nor *vindicated* a posteriori.

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[5] Van Fraassen (1989, pp. 303-4).
[6] See Strevens (1998, p. 231) for further discussion.
[7] As applied to the rather different problem of induction – see Reichenbach (1951, chapter 14) and Salmon (1991) for a critical discussion.

Reichenbach's argument appears to aim for a stronger conclusion than Gillies'. Reichenbach proposes a proof that the principle of indifference grounds no sound inferences at all to the probabilities of physical events *that can not be established by other empirical means*. In other words, the principle does no outstanding work at all in practical inference. By contrast, Gillies accepts that the principle does some heuristic work – in suggesting new hypotheses or physical theories entailing probability values for various outcomes. However, although it may be heuristically useful in generating new physical theories or hypotheses, it has no standing as a logical principle. Employing Reichenbachian terminology once again, we may say that, according to Gillies, the principle has an inferential function in the context of discovery, while lacking it in the context of justification. By contrast, Reichenbach appears to claim that the principle has no inferential function in any context whatever.

Nevertheless both arguments share the aim to show that the principle is redundant in the ascription and justification of probabilities: any work the principle could appear to do in providing probability values for outcomes, in any context, is work that can be done by other methods. More generally both Reichenbach and Gillies aim to provide alternative means for the justification of probabilistic hypotheses and stochastic laws, which would eliminate any need for the principle, or render it otiose for this purpose. We may thus refer to their arguments as 'eliminativist'.

Bangu finds both 'eliminativist' arguments defective. He first shows that Reichenbach's argument is either circular or unsound: either the principle of indifference is itself assumed in the proof or it remains thoroughly unjustified. Reichenbach's argument is a development of yet another argument found in Poincare, and goes roughly as follows. [8] Consider a roulette wheel, evenly divided into red and black intervals, corresponding to red and black numbers. In the absence of any further information, an application of the principle of indifference entails that the probability of obtaining a red or a black outcome should be the same and equal to ½. The question is whether there is a distinct procedure that would enable us to derive the same result but without invoking the

[8] Reichenbach (1949); Poincare (1912). For a summary and review see Strevens (1998, pp. 236-8).

principle at all. Poincare and Reichenbach reason as follows. Consider that the outcome of the game is determined by where the wheel stops, and may be represented by a variable θ ranging between 0 and 2π. Let then d (θ) be the probability distribution over θ. The probability of obtaining a red number is given by the sum over the probabilities that θ falls in a particular red square. Now assuming that the intervals alternate rapidly in θ, *and* that the function d (θ) is smooth over the intervals (even though not necessarily constant), then the probability of red and black is equal. This reasoning appears to provide us with a procedure that enables us to derive the correct ½ probability values for red and black from the physical symmetry of the roulette wheel without apparently invoking the principle of indifference. However as Bangu points out, the argument depends upon the function d (θ) is smooth. And the only real reason to suppose this is that the symmetry of the wheel requires that d (θ) is uniform, i.e. that it is the same for every discrete value of θ. To say this is just to state the principle of indifference over again: we ascribe equal probability to all possible outcomes because there is no reason to anticipate one rather than another result. Unfortunately what this means is that the smoothness of d (θ) depends upon the principle of indifference itself, so the procedure described by Reichenbach and Poincare does not actually do away with the principle in practice. Hence a vindication remains a possibility.

Bangu then discusses Gillies' argument and he claims that it does not hold water either. He points out that the kinds of methods that Gillies invokes as replacement for the principle of indifference for the justification of probabilistic hypotheses are subject to precisely the same kind of objections that show the principle itself to be untenable. Gillies claims, following Jaynes, that the principle of indifference provides us with a heuristics for seeking new statistical theories and hypotheses. [9] But he also claims that the principle is dispensible as a method for justifying statistical hypotheses, which may always be justified by means of a more appropriate methodology. In particular Gillies defends a 'methodological falsificationist' approach to the testing of statistical hypotheses, partly inspired by Popper and partly by the classical statisticians Fisher,

[9] Gillies (2000, p. 47-49), where several examples from physics are provided, such as the viscosity of gases and Bose Einstein statistics.

Neymann and Pearson. In this account, a *falsifying rule for probability statements* (FRPS) is formulated, which enables us to construe probabilistic statements as falsifiable ‘in practice’, even though from a strictly deductive point of view, such statements are in principle unfalsifiable. [10] A statistical hypothesis H is then methodologically falsified by a sample of data points {e_1, e_2, e_3, ... ,e_n} if there is a test statistic X whose value lies below the statistical significance level, which is typically fixed at 5%. [11]

Howson and Urbach have argued that the falsifying rule requires a decision regarding the outcome space of the test statistic X. And whether or not the data points may be said to falsify the hypothesis H may well depend on this decision. In particular they claim that a decision is required to determine the “stopping rule” describing the conditions under which the experiment is terminated or finalised. For instance in assessing of the hypothesis that a particular coin is fair, we must repeat the experiment a number of times and different rules may be applied to the termination point. As a result the outcome space (the space of all possible sequences of outcomes) is affected. [12] Bangu goes further in claiming that the decision regarding the outcome space is akin to the decision that the principle of indifference promotes in order to ascribe equal probability to outcomes evidentially on a par. In both cases the decision involves fixing the outcome space. According to Bangu this compromises Gillies’ argument for the dispensability of the principle of indifference. The type of methodology that we would be attempting to replace the principle with is thoroughly infused with just the sort of difficulty that led us to abandon the principle in the first place. [13] Thus, Bangu concludes that there is not yet a good argument against the vindication of the principle of indifference in practice.

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[10] See Gillies 2000, p. 147.

[11] A test statistic for an experiment is a random variable X, whose value can be calculated as a function of the data sampled, X (e_1, e_2, e_3, ... , e_n), and that can be taken to represent the outcome of the experiment. Note that the same experiment may yield different values for the test statistic, depending on the data sampled.

[12] Howson and Urbach (1993, pp. 210-212). In their example we may choose either to terminate the experiment as soon as 6 heads occur, or rather after 20 trials regardless of the outcome. The size of the outcome space is then predetermined in the latter case (= 2^{20}) but not so in the former. Even if the outcome spaces happened to have the same size in both cases (because say the 6^{th} head happens to occur on the 20^{th} trial), it would still be the case that the stopping rule could affect the result of the application of the falsifying rule, falsifying it in the former but not the latter case.

[13] Note that Gillies disagrees that a falsificationist methodology is in any way threatened by Howson and Urbach’s argument. See particularly the discussion in his interesting review of their book (Gillies 1990, pp.

Typicality in Statistical Mechanics

In the third and last chapter in the probability section of the book, "Why Typicality does not Explain the Approach to Equilibrium?", Roman Frigg critically evaluates attempts in the philosophy of statistical mechanics to provide typicality-based explanations of thermodynamic irreversibility. Consider a classical system consisting of n particles, each endowed with three degrees of freedom, and governed by Hamiltonian dynamics. Its state may be represented in a constrained 6n-1 dimensional energy hypersurface Γ_E of the corresponding 6n-dimensional phase space Γ. Each macroscopic state (defined by sets of macroscopic properties) M_i will define disjoint and exhaustive subregions Γ_{Mi} of Γ_E. The second law of thermodynamics is then supposed to entail that the evolution of the entropy of the macrostate of any (freely evolving) system mirrors the increase of thermodynamic entropy over time, reaching a maximum value at equilibrium. Suppose the initial state of the system is x (t_0), and the final state is x (t). Then let Γ_{Past}, Γ_{Equi} be the past and the equilibrium macrostates of the system, so $x(t_0) \in \Gamma_{Past}$, and $x(t) \in \Gamma_{Equi}$. It seems to follow from the second law that any system whose initial macrostate is Γ_{Past} will eventually wind up in Γ_{Equi}.

Why is this so? And more particularly: is there an explanation for this fact in statistical mechanics? [14] We may refer to any approach that aims to provide an explanation by invoking the notion of 'typical state', as a 'typicality explanation' (of the approach to equilibrium). This type of approach relies on the thought that the equilibrium macrostate Γ_{Equi} is the largest among all the regions Γ_{Mi} under some standard natural

90-97). Howson and Urbach respond in the 2nd edition of their book (p.p. 214-215). This debate turns on whether or not the stopping rule is relevant to the performance of the experiment, and therefore relevant to the evaluation of the application of the falsifying rule. It is surprising that this debate does not yet appear to have been linked to the question of the nature of the probabilities involved, and in particular whether they are subjective or objective probabilities.

[14] Should there be one? The presumption that there should is of course tantamount to the view that thermodynamics should be reduced to statistical mechanics. It is controversial whether such attempts have been successful. Moreover it is unclear that they should be in order to ground thermodynamic irreversibility. See for instance Sklar (1993, chapter 9). Such interesting questions are beyond the purview of this essay or this book.

measure, such as the Lebesgue measure μ.[15] Frigg discusses three different typicality approaches and his sober conclusion is that none are actually viable. As is often the case in a philosophical dispute much hinges on the initial formulation of the problem. Frigg first outlines a standard formulation which he helpfully refers to as ‘gloss’, and which he goes on to dispute (in section 4 of his paper). This formulation is however sometimes adopted by other authors as a fact, namely ‘the fact that equilibrium microstates are typical with respect to Γ_E and the Lebesgue measure μ’ (p. 5). Indeed the three approaches discussed by Frigg in some way link this ‘gloss’ to the dominance of the equilibrium macrostate.

The first approach appeals to the brute fact of typicality itself. In other words it aims to explain the approach to equilibrium as a result of the typicality of equilibrium states. Frigg rightly points out that there is no reason to suppose that atypical states need evolve into typical states just because the former are atypical and the latter are not. And this is true even if the atypical states made up a measure zero set. The evolution of the states depends rather on the specific dynamical laws that operate, and cannot be settled just by looking at the measures (relative sizes in the case of the Lebesgue measure) of different regions of phase space.

The second approach consequently focuses on dynamics. Boltzmann’s original ergodic theorem is an attempt at a dynamic explanation (roughly the ergodic theorem states that the dynamics of the state is such that any trajectory sooner or later visits every point in Γ_E. In other words regardless of the initial microstate a system will eventually take every other microstate compatible with the macroscopic constraints.) [16] There are however well known problems with Boltzmann’s original ergodic theorem, and improved ergodic explanations of the approach to equilibrium have also been criticised. [17] This solution seems to be rejected by those who advocate the typicality explanation in any case. Another reading of the second (dynamical) approach regards chaotic dynamics as the key to the explanation of the approach to equilibrium. Frigg in turn distinguishes two

[15] A measure usually defined over the semi-closed intervals of the real line (see Halmos, 1974, pp. 65ff.)
[16] Sklar (1993, pp. 159-160).
[17] For a thorough critique see Earman and Rédei (1996).

versions of a chaotic explanation. The first is based upon the sensitive dependence on initial conditions characteristic of chaotic behaviour, and only requires chaos locally in a particular subset of the phase space. Sensitivity to initial conditions has been argued to ground a typicality explanation of equilibrium, in the sense that the trajectories that will exhibit random walk behaviour are ‘typical’. More specifically, the region of the phase space that contains the initial states of trajectories that exhibit this type of random walk behaviour has a Lebesgue measure arbitrarily close to 1. Frigg refers to this condition as the Typicality Past Hypothesis (TPH) but rejects the idea that all those trajectories that satisfy this condition actually carry typical initial conditions into the equilibrium region. He claims that there is an important set of such trajectories belonging to KAM systems that do not do so. So this typicality explanation also seems to fail for reasons not dissimilar to the ergodic explanation. The second version of the dynamical explanation is more promising according to Frigg. This focuses on the notion of global chaos, where the entire phase space exhibits chaotic features and not just isolated subsets of the phase space. Frigg discusses several ways of trying to make the notion of global chaos more precise and ground the explanation of the approach to equilibrium. The most promising are still prey to some of the objections that were raised against ergodic approach.

Frigg discusses yet a third approach, due to Lebowitz and Goldstein, which focuses on the internal structure of the micro regions Γ_{Mi} rather than the entire phase space. The important feature, according to Frigg, is the property of each state in Γ_{Mi} of being “entropy-increasing”. This is a relational property of states and dynamical trajectories: a state is entropy increasing if it lies on a trajectory that takes lower entropy states into higher entropy states. A system is then defined as “globally entropy increasing” roughly if every subset of its phase space is densely populated by such entropy increasing states. One would then hope that global entropy increasing systems are all necessarily equilibrium approaching. However this is unfortunately not the case, and any attempt to work out a fit between these two notions still requires us to make assumptions regarding the typicality of entropy increasing states within the phase space regions in accordance to the standard Lebesgue measure.

Frigg's conclusion is that any proper explanation of the approach to equilibrium will require a dynamical explanation; merely grounding it upon the typicality of the corresponding states within the phase space won't ever be sufficient. It does not matter whether entropy increasing states are typical in this sense – what matters is rather the details of the dynamical laws that evolve low entropy into higher entropy states. Without a reference to the dynamical transformation of the states, such explanations appear empty or vacuous. (See section 6 for a discussion of the dynamics of propensity states).

2. **CAUSES**

The second part contains essays by Federico Laudisa on the nature of causation in modern physics, Joseph Berkovitz on the more specific issue of backwards in time causality in quantum mechanics, Miklós Rédei and Balasz Gyenis on the causal completeness of probabilistic models, and a joint paper of mine with Iñaki San Pedro on causal inference in the context of EPR experiments.

From Metaphysics to Physics

In chapter 5, Federico Laudisa takes up the issue of causation in quantum mechanics, particularly in connection with the EPR correlations. Laudisa first rejects the idea that causality is anathema to quantum mechanics in general. He then endorses a form of causal pluralism that leads him to the view that many questions regarding causality in quantum mechanics may receive different answers in different frameworks, or depending on interpretation. (In fact he later makes it known that he subscribes to a stronger claim which I have defended *vis a vis* the EPR experiment, namely: that such issues have no determinate answers independently of the details of the models of the correlations provided within each interpretation). The rest of the paper is a review of the main difficulties that emerge in the attempt to provide causal accounts, mainly with reference to the EPR correlations within some of the different models and interpretations of quantum mechanics. In particular Laudisa focuses on the GRW and Bohm's theories.

One feature of Laudisa's analysis is his assumption that performing a measurement and obtaining an outcome is essentially the same event. The causal connections that he has in mind are between measurement-and-outcome events. (It is arguable that this rules out a propensity interpretation of the quantum state, something that I shall discuss in due course). Laudisa thinks that the superluminal nature of any putative connection in this case yields a 'weak' form of causality, which seems to violate intuitions regarding the necessary temporal priority of causes. Hence after reviewing some of the literature that disputes that there is necessarily a conflict between a causal reading of the EPR correlations and special relativity, Laudisa raises the question: is it possible to provide a causal understanding of the connection that does not require backwards in time causation? The key to a proper analysis, according to Laudisa, lies in a better ontological account of the theory in the first place.

This leads Laudisa to address two different interpretations, the GRW theory (section 4) and Bohmian mechanics (section 5). The GRW interpretation is well known for its postulate of spontaneous collapses of the wavefunction. These spontaneous localisation events occur sufficiently often for the detection of macroscopic superpositions not to be possible in practice. One outstanding problem with the account is related to its relativistic extension since the localisation events seem to privilege a particular hypersurface and might select a frame. Laudisa distinguishes two different proposals for its ontology, the 'matter density' and the 'flash' ontology. [18] The former assumes that a continuous field on 3-dimensional space represents the matter density in each point of space at each instant. The latter by contrast assumes a discrete ontology, in which matter is made up of discrete points ('flashes') in spacetime such that to each of these flashes there correspond one of the spontaneous collapses of the wavefunction. One advantage of the flash ontology is that it has been shown to be Lorentz-invariant, while

[18] See Tumulka (2007) for the distinction and a development of the 'flash' ontology.

prescribing the relevant probability distributions for all observables. This avoids any conflict between GRW and the temporal priority of causes over effects thesis.

Laudisa then considers the non-relativistic alternative to select a preferred foliation of spacetime. He finds that while this assumption is unjustified for orthodox quantum mechanics, it is unavoidable in the case of Bohmian mechanics. In this context, as is well known, whatever mutual causal influence there is between the quantum potential or wavefunction in configuration space and the particles inhabiting 3-dimensional space, is both simultaneous and epistemically inaccessible in the sense that only the consequences of the causal interaction (the positions of the particles) are detectable by measurement apparati, but not the causal interaction itself. (Again, it is worth noting that a propensity interpretation of the state in orthodox quantum mechanics would share this feature).

Causal Loops in Retro-Causal Models

In chapter 6, Joseph Berkovitz carefully considers a number of retro-causal models of the Einstein-Podolsky-Rosen correlations. These are models that postulate the existence of causes acting backwards in time. A traditional objection against such causes in general states that they may generate loops in time which give rise to inconsistent effects. In the simplest case, suppose *e* causes *c*, but that *c* precedes *e* and is moreover an inhibitor of *e*, i.e. *c* is a cause of $\neg e$. Now suppose the causing is deterministic in both instances: it then follows that *e* if and only if $\neg e$. The most straightforward way to avoid such inconsistency would be a total ban on retrocausality. But there might be other less sanguine ways to keep such inconsistencies at bay, similar to those often used to keep at bay the inconsistencies generated by 'bilking'. [19] Berkovitz focuses on the particular

[19] In the case of the famous 'bilking' argument (Black, 1956), the assumption is simply that an event *c* is the positive cause of an event *e* that lies in its past. The issue is then how to prevent the bilking of *c* after *e* has occurred. For if we prevent *c* from happening after e has already occurred, then this would generate the inconsistency that both 'c is the cause of e' and 'c is not the cause of e' are simultaneously true. Much will depend on whether 'bilking' is actually physically possible in the particular circumstances that give rise to

conditions that obtain in an EPR experiment, with an eye to investigating ways in which causal loops maybe evaded even if the postulated causal structure contains causes that act back in time in at least some frames of reference. In the end Berkovitz's assessment is sober: even where such models may be postulated and do not entail inconsistency, there are problems regarding their predictive or explanatory power; and the problems are sufficiently severe to make the models dubious or at least unnecessary.

Berkovitz applies retrocausality to a specific experimental setting that he calls experiment X. This is an EPR experiment where the right hand side measurement takes place before the left hand side setting in the laboratory rest frame. Let us denote by *l*, *r* the settings on the left and right hand sides; and by *L* and *R* the measurement outcome events on the left and right hand sides respectively. Suppose further that the right hand side outcome, *R*, is a deterministic cause of the left hand side setting *l*. Since we have assumed that *R* occurs before *l* in the rest frame of the laboratory, the causal connection between *R* and *l* is hence forwards in time in that frame. However, in a retrocausal model we additionally require either that i) *l* retro-causes the complete state at the source, or ii) both *R* and *L* jointly cause the complete state at the source.

We may then go on to appropriately distinguish two different kinds of retrocausal models: deterministic and indeterministic. In agreement with the standard understanding of these terms, a deterministic cause invariably brings about its effects in the appropriate circumstances. An indeterministic cause by contrast, determines the probabilities of its effects between zero and one – so it brings about its effects but only with certain probabilities. For instance in a typical retrocausal model of experiment X, the measurement setting on the left, l, may be a partial but deterministic cause of the complete state at the source, which in turn is a partial but indeterministic cause of the outcome events. (This seems to be what Berkovitz has in mind with his 'DS model'). By contrast, if the setting *l* only prescribes the probabilities for the complete state at the source, the model is indeterministic. In either case, there is a causal influence from

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c and *e*. Similarly for the type of inconsistency that causal loops may generate: much will hinge on the particular circumstances that bring about the EPR correlations.

settings or outcomes back towards the complete state at the source at the time of emission.

More specifically retrocausal models are typically assumed to violate the condition known as λ-independence, or 'hidden autonomy': [20]

$$\rho\ (\lambda\ /\ \psi\ \&\ l\ \&\ r) = \rho\ (\lambda\ /\ \psi)$$

where λ is the complete (hidden variable) state of the pair at the source, ψ is the quantum mechanical state, and l and r are the settings of the measurement apparatuses on the left and right side of the experiment respectively. In other words, in these models the hidden state at the source is statistically dependent upon the quantum state and the left and right settings. However, recall that in a typical EPR experiment the setting events take place in the rest frame of the laboratory *after* the emission event at the source and thus after the hidden state is determined. If the statistical dependence expressed by λ-independence reflected direct causal influence it would follow that posterior events causally influence antecedent ones. [21]

Berkovitz carefully analyses different kinds of retrocausal models of experiment X and concludes that these models entail the existence of causal loops. The issue is then how to interpret such loops and their consequences, and in particular whether they imply inconsistent predictions. Berkovitz concludes that the causal loops within some deterministic models entail inconsistent predictions, while those entailed by indeterministic models are unable to determine the distributions over complete states or measurement outcomes (unless supplemented with the appropriate statistical rules). [22] So

[20] 'Hidden autonomy' is Van Fraassen's (1982) terminology.
[21] But does statistical dependence reflect causal dependencies? Arguably the relationship is more complex and subtle. First, it is well known that statistical dependencies may mask hidden factors or hidden common causes. And second, the relation of conditional probability P (x / y) need not indicate that the conditioned upon event *y* is a direct cause of the event *x*. This requires a further assumption (see section 6 in this essay). I will follow Berkovitz here and assume for the sake of argument that causal dependencies can be read off statistical relations. In the second part of the introduction, I argue that conditional probabilities are not generally a reasonable way to read propensities.
[22] Throughout his paper Berkovitz assumes a single-case propensity interpretation of probabilities. But he shows that analogous results stand if the probabilities are understood as frequencies.

in the deterministic case, retrocausality possesses the potential to generate contradictions, while in the indeterministic case it is unable to generate any meaningful predictions at all. Either way these are important arguments against retrocausal models of the EPR correlations in general.

Causal Completeness of Probability Theories

In chapter 7 Balasz Gyenis and Miklós Rédei provide a review and reassessment of recent work regarding the notion of causal completeness for probability spaces. They provide very precise formal definitions of some of the most important terms in this literature. For instance, they define the concept of generalised Reichenbachian common cause (in section 3) and the notion of causal completeness that follows from it (section 4). They then review some of the main results on causal completeness derived within the so-called 'Budapest school'. [23]

The basic formal notion is that of a general probability measure space (£, Φ), where £ is an orthocomplemented lattice and Φ is a generalized probability measure or *state*, a σ-additive map Φ: £ → [0, 1] where Φ (0) = 0 and Φ (1) = 1. (Roughly: the elements of the lattice {A, B}, or variables, correspond to one-dimensional observables while the measure Φ defines the probabilities over the values of these variables ascribed by a quantum mechanical state). We may then define a correlation as follows: $Corr_{\Phi}$ (A, B) is the measure of correlation between compatible variables A and B in the state Φ.

A generalised version of Reichenbach's *criterion* of the common cause [24] may then be formally characterised as follows: [25]

C_k is a Reichenbachian common cause of the correlation $Corr_{\Phi}$ (A_I, B_J) > 0 between A_I and B_J if Φ (C_k) ≠ 0 for all k ε K and the following conditions hold:

[23] The name 'Budapest school' was introduced by Jeremy Butterfield (2007, p. 807).
[24] For the distinction between the 'criterion' and the 'postulate' of common cause see Suárez (2007b).
[25] See Gyenis and Rédei's Definition 3.1.

1. Corr_Φ (A_I, C_k) > 0.
2. Corr_Φ (B_j, C_k) > 0.
3. Corr_Φ (A_I, B_J / C_k) = 0 for all k ε K.

Gyenis and Rédei then show that these conditions reduce to the usual Reichenbach characterisation of common causes in the limiting case of two-valued variables. The intuitive idea is indeed the same, namely screening off: conditionalising upon the common cause renders its effects statistically independent. (The first two conditions assert that the common cause is statistically relevant to each effect taken separately).

The question of causal completeness of probability spaces is then in a nutshell the following: given any correlated variables A_I, B_J ε £, can we expand the probability space (£,Φ) so as to find a common cause variable C_K, satisfying the relations above, which is included in the space? Gyenis and Rédei formalise the notion of *causal completeness* as follows: A probability space (£,Φ) is causally complete with respect to a causal independence relation R and correlation function Corr_Φ if for any two compatible variables A_I, B_J in £ there exists a generalized Reichenbachian common cause C_K of size K ≥ 2 in £ of the correlation. [26] The causal independence relation R minimally requires logical independence – but it must impose additional conditions. [27]

Under these conditions Gyenis and Rédei review a number of important results on causal completeness; the most important seems to be "proposition 8", which states that 'every atomless general probability space is causally event-complete'. This means that there are statistical theories that are causally complete: i.e. they contain the Reichenbachian common causes of their correlations. Gyenis and Rédei point out that it

[26] See Gyenis and Rédei's definition 4.1. A common cause variable C_K has size 2 if it has two values. For instance an indicator function (on-off) can be represented as a size two variable (C, ¬ C).
[27] Gyenis and Rédei leave open what this further conditions may be, which seems wise since their aim is to describe formal models applicable to any physical set ups. In causal modelling one would of course like to know more about this relation, and in particular the physical conditions that must obtain for A, B to be causally independent in the prescribed sense.

follows from this result that one may not refute Reichenbach's common cause principle by appealing to the thought that statistical theories are *generally* causally incomplete. [28]

Robustness and the Markov Condition

Chapter 8 is my own discussion (jointly with Iñaki San Pedro) of the relationship between the robustness condition once defended by Michael Redhead for the quantum correlations and the Causal Markov condition (CMC) that has been much discussed recently in the causal inference literature. We argue for a tight connection between these two conditions, namely: robustness follows from the CMC together with a number of additional assumptions. First we take Richard Healey's (1992) distinction between two forms of robustness, each appropriate for the assumption of total or partial causes. (Healey reserves the term "robustness" for the first condition only, while using "internal robustness" for the second condition.) We then show that each notion of robustness follows from CMC and the assumption of either total or partial causes under the only further assumption that there exists one independent disturbing cause acting on the putative cause of the cause-effect link (in other words, that a form of intervention is possible). This entails that from the standpoint of an interventionist account of causality there is no real difference between applying robustness or the CMC. And the latter condition is more general since it does not require interventions (or disturbing causes). So it may be safely assumed in all future discussions regarding the status of causality in quantum mechanics. The robustness literature is thus shown to be superseded, and we recommend philosophers of science and causal methodologists alike to focus on the status of the CMC in quantum mechanics instead.

This argument so far supports the programme of the causal Markov condition theorists, such as Jim Woodward and Dan Hausman. However, in the second half of the chapter we go on to disagree with Hausman (1999) and Hausman and Woodward (1999)

[28] The reasoning is convincing but one wonders to what extent the arguments against Reichenbach's Principle depend on the claim of (formal) incompleteness. For discussion see San Pedro (2007, chapter 3).

over the status of causation in quantum mechanics. It has traditionally been supposed that quantum mechanics provides a striking refutation of the principle of common cause and other standard methods of causal inference. This would arguably compromise the validity of CMC – at least in indeterministic contexts. Hausman and Woodward have claimed that the CMC is not false in quantum mechanics, but rather *inapplicable*. That is, they maintain that the conditions that would allow us to apply CMC are not met in this setting, and it is impossible to tell whether CMC obtains or is violated. We argue that on the contrary there is in principle no reason why the CMC cannot be applied. What's more the application of CMC does not support the traditional judgement regarding causation in quantum mechanics. On the contrary our assessment is that whether or not CMC is violated depends very sensitively upon both the detailed statistics modelled, and the interpretation of quantum mechanics applied. As an example we discuss the status of causality in EPR in the context of the model of Bohmian mechanics. Steel (2005) has argued that in this context the CMC fails; we argue that to the contrary it arguably obtains, provided enough attention is paid to the details of the model itself. More generally, our paper is a call to apply the CMC to quantum mechanics in order to figure out causal structures, but to do so judiciously – and this, we claim, requires a healthy dose of methodological pragmatism. Philosophers ought to start by looking at the diverse range of models available first within a number of different interpretations and then draw their judgements on the basis of a consideration of their details.

3. **PROPENSITIES**

The third and final part of the book contains three essays on propensities, mainly in the quantum domain. Mauro Dorato reassesses the role of dispositions in quantum mechanics, Nicholas Maxwell reviews the latest stage of his 'propensiton' theory, and Ian Thompson provides a philosophical analysis of nested dispositions in physics.

Dispositions in the Ontology of Quantum Mechanics

In chapter 9 Mauro Dorato considers the role of dispositions in quantum mechanics. In particular the most substantial part of the paper defends a role for dispositions within the so called Ghirardi-Rimini-Weber (GRW) interpretation. Dorato defends the view that the probabilities for collapse ascribed by these theories can be given an objective reading – in particular, they are interpretable as propensities. He suggests two different ways for doing this. First, he aims to show that dispositional readings of the spontaneous collapses postulated by these theories are not only possible but natural. Second, he argues against alternative non-dispositional interpretations of collapse probabilities, particularly the Lewis-style best system analysis account.

On the first issue, Dorato argues that dispositions are natural on both the original mass density localisation proposals of Ghirardi-Rimini-Weber (1986) and the most recent proposal attributed to Tumulka (2006), the so-called 'flash ontology' proposal. (The supposed advantage of the latter is the existence of a relativistic extension). Secondly, Dorato argues against Frigg and Hoefer's (2007) attempt to read quantum probabilities in the GRW interpretation in a Humean way, in accordance with the best system analysis. Dorato's main claim seems to be that the quantum probabilities are conditional probabilities and therefore relations between sets of events or properties at the quantum level. A Humean reading of such probabilities would then incur a fallacy of omission – since it fails to explain what such conditional probabilities are *conditional upon*. (Dorato's claim is controversial and heavily dependent upon the interpretation of conditional probability; the claim however has a more solid basis if grounded on *transition* as opposed to merely conditional probabilities – and I argue in this introduction that quantum probabilities should be understood as transition probabilities).

In the final section of the paper Dorato argues against my own selective propensity interpretation (Suárez 2004; 2007a), which he appropriately links to some aspects of Bohr's response to the measurement problem. As I understand it Dorato is charging the selective propensity interpretation with a possible fallacy in its description of the actualisation of dispositional properties. Such actualisations may or not be physical processes. If they are physical processes, then the selective propensity account is incomplete since it does not describe them. (Note that it follows from this that all collapse interpretations, including GRW on Dorato's own dispositional reading, are also incomplete). If on the other hand such actualisations are not physical processes then the application of propensities remains mysterious (and its explanatory power is compromised): we are back to the old 'dormitive virtue' objection to dispositions in general.

Dorato's objections are intricate and interesting but in my view they ultimately fail to hit their target. The selective propensity account indeed remains silent on the physical processes that underlie the actualisation of propensities. It takes the standard propensity view that dispositions are displayed in probability distributions, each in its proper context of application. [29] But it does not aim to explain the mechanisms – if any – that connect dispositions and probabilities. Such mechanisms would appeal either to categorical properties in which case dispositions are ultimately reduced, or to further dispositional properties. Either option seems viable from a dispositionalist point of view, but neither seems called for since the very existence of such a mechanism seems a remnant from categorical property-speech. Consequently I also disagree with the need to provide a categorical basis for the dispositions which Dorato and I do agree are applicable to Bohmian mechanics (We agree on the applicability of dispositions, but the agreement seems to end there – I take such dispositions may well be ultimately irreducible while Dorato thinks they must be reducible to the only categorical property available in Bohmian mechanics, i.e. position).[30] The general explanatory question that

[29] Mellor (1971).
[30] I introduce irreducible dispositions into Bohmian mechanics in Suárez (2007, section 7.2). However, I was not the first person to suggest such a reading. Pagonis and Clifton (1995) are an antecedent (although to my mind they mistakenly understand dispositions relationally, and identify them with aspects of Bohmian contextuality). An attempt closer to my own ideas is due to Martin Thomson-Jones (Thomson-Jones, unpublished). We both defend irreducible dispositions with probabilistic manifestations for Bohmian mechanics but unlike Thomson-Jones I restrict the applicability claim to the causal or maximal interpretation. Thomson-Jones' unpublished manuscript is dated after the submission date of the final version of my paper. However, I was in the audience both in Bristol (2000) and Barcelona (2003) where preliminary versions of Thomson-Jones' paper were presented. Although I don't recall the details of these

Dorato wants to ask: “by virtue of what mechanisms does a propensity generate a distribution?” has in my view no genuinely dispositionalist answer.

The Propensiton Theory Revisited

Chapter 10 contains Nicholas Maxwell’s latest defence of his ‘propensiton’ version of quantum theory, which he has been developing for more than three decades now (see Maxwell, 1972 for the earliest defence). Maxwell argues that the propensition quantum theory (PQT) has testable consequences that could in principle distinguish it empirically from the orthodox quantum theory (OQT). So the PQT is not merely an interpretation of quantum theory: it is an alternative theory in its own right. Its main merit, according to Maxwell, is to combine indeterminism – understood as the idea that there are essentially stochastic or probabilistic processes out there in the world which generate certain outcomes with certain probabilities – and realism – the view that at the quantum level nature too is determinate: properties have values all the time independent of whether or not subjected to measurement.

Maxwell is right that indeterminism and realism are not necessarily in contradiction. Some of the extant alternative interpretations of quantum mechanics – such as the Ghirardi-Rimini-Weber (GRW) collapse interpretation, and the Quantum State Diffusion (QSD) theory – are already living proof. [31] And Maxwell is right to claim that his propensiton theory (PQT) was formulated before these theories came onto the market. The PQT is distinct from either of these more established alternatives on several counts. The most important difference is that Maxwell postulates the existence of distinct entities - propensitons - which live in physical 3-d space and whose states are described by the quantum wavefunction. It is the physical interaction between such entities that ‘fires’ the spontaneous collapse of the wavefunction.

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talks I am sure I was influenced by them, as well as many friendly chats with Martin over the years – for which I am very grateful.

[31] It is not surprising that such theories have already received interpretations in terms of dispositions – see Frigg and Hoefer (2007) and Suárez (2007, section 7.1).

The theory has several virtues, not the least of which is to have anticipated collapse interpretations, and Maxwell canvasses and studies them well. Like any other version or interpretation of quantum theory the propensiton theory also has its own difficulties. They are related to Maxwell's essentialism about laws combined with the claim that the nature of the entities fundamentally depends upon the laws that govern their behaviour. In tandem these two assumptions entail that the shape of the propensitons is given by their geometrical counterpart in the dynamical evolution of the wavefunction.[32] Indeed Maxwell's physical picture takes it that a couple of propensitons ('expanding spheres') at some point clash, and immediately contract at that point. But this view faces a plethora of problems and difficulties, all connected with the literal geometric interpretation. First, there is the problem of how to interpret the contraction of the spheres; and in particular whether this process obeys energy momentum conservation; second there is the problem of how to interpret Maxwell's claims that the contraction processes result from inelastic scattering that creates new particles – particularly in light of the fact that some measurements on the face of it create no new particles – such as destructive measurements.

Derivative Dispositions

In the last chapter of the book Ian Thompson faces up to a fundamental question for dispositionalism, namely the nested exercise of dispositions in physics. The manifestation properties for dispositions need not be categorical. Rather dispositions will often be manifested in further dispositional properties. Thompson cites potential energy force and force as characteristically nested dispositions. (Potential energy force is the disposition to generate a force, while force is the disposition to accelerate a mass). These are, in his terminology, *derivative* dispositions. It is interesting to apply the idea to the dynamical evolution of quantum systems (section 4.3). Suppose a system in an initial state Ψ (t_0) is evolved by a Hamiltonian H to a new state Ψ (t_1). Thompson suggests that

[32] See Thompson (1988) for a similar assessment.

the Hamiltonian be a disposition to evolve the state, while the states be themselves dispositional properties, namely propensities to produce measurement outcomes with the various probabilities $p_\lambda = | < \mu_\lambda | \psi (t) > |^2$. The Hamiltonian represents a 'dynamical' or diachronic disposition that generates further 'static' or synchronic dispositional properties, or propensities, on measurement. [33] We may then refer to the latter as *derivative* dispositions.

The full range of derivative dispositions generates a 'grid' of dispositions that we may refer to as a *multiple generative level*. Thompson introduces a number of additional distinctions and terminology to supplement this idea. The terminology is essentially causal because Thompson assumes that the action of primary dispositions over the inferior levels down the grid is causal in nature. (Thus he would say the Hamiltonian disposition *causes* the successive sets of static propensities). The thesis that dispositions and their manifestations are causally related is not new. [34] It suggests that there is a particular time or instant at which the disposition fires to generate its manifestation. And this introduces questions regarding the nature of the 'firing' event, and whether it is grounded upon further dispositional properties. We do not enter these difficulties here. The point Thompson's essay makes admirably is the more basic one that the manifestation properties of dispositions may be dispositional too.

4. **TRANSITION VERSUS CONDITIONAL PROBABILITIES**

[33] The idea strongly recalls the distinction between dynamical and value states within the modal interpretation of quantum mechanics. See Van Fraassen (1991, chapter 9).

[34] Nor is it uncontroversial. Lewis (1997, pp. 149ff.) introduced the idea of causal bases for dispositions. Bird (forthcoming) discusses objections to the idea that stimulus conditions cause dispositions to manifest themselves. For the purposes of this introduction I have ignored stimuli and concentrated on the disposition – manifestation relation itself (e.g. in the discussion in sections 5-6).

Most of the authors in this volume discuss, often approvingly, the idea that the properties dealt with in fundamental physics and, particularly in quantum mechanics, may be essentially dispositional, or propensities. Objective physical propensities or chances are sometimes represented as forwards in time conditional probabilities. In this section, I provide a brief argument that the best representation is instead by means of transition probabilities, and that both representations are distinct.

Transition probability: Take One

Consider the equation for a forwards transition probability discussed in section one:

$$P_{j+1/j} (S (t_{j+1}) / S(t_j)) = P_{j\&(j+1)} (S (t_{j+1}) \& S(t_j)) / P_j (S (t_j)) \qquad \text{(FTP)}$$

This equation does not express a well-defined conditional probability. The probability functions are different in each side of the equality since the time sub-indexes are different. Rather the formula enables us to calculate the probability for a physical transition from the state $S(t_j)$ to the state $S (t_{j+1})$ by working out the probability of the earlier state at the time of its occurrence and then the joint probability of both states at the conjunction of *both* distinct times. Let me discuss more precisely the meaning of this expression shortly. For now let us just note that the expression of a transition probability crucially differs from the similar expression for the conditional probability of successive states at time t_j:

$$P_j (S (t_{j+1}) / S(t_j)) = P_j (S (t_{j+1}) \& S(t_j)) / P_j (S (t_j)) \qquad (CP_j)$$

It also differs from the conditional probability of such states but calculated at the later time t_{j+1}: [35]

[35] On the assumption of a fixed past and an open future (CP_{j+1}) does not express anything informative since $P_{j+1} (S (t_j)) = 1$ and $P_{j+1} (S (t_{j+1}) / S(t_j)) = P_{j+1} (S (t_{j+1}))$ for any states $S (t_j)$, $S (t_{j+1})$. But Bacciagaluppi is

$$P_{j+1} (S (t_{j+1}) / S(t_j)) = P_{j+1} (S (t_{j+1}) \& S(t_j)) / P_{j+1} (S (t_j)) \qquad (CP_{j+1})$$

Thus, a transition probability is at least *prima facie* distinct from the corresponding conditional probability regardless of the time that it is calculated at. The formal difference between the expressions reflects a deep physical distinction.

Transition Probability: Take Two

As a matter of fact (FTP) does not express a conditional probability at all since a transition probability is neither conceptually identical nor reducible to a conditional probability. We would be better advised to write transition probabilities down as follows:

$$P_{j \text{ » } j+1} (S(t_j) \text{ » } S (t_{j+1})) = P_{j\&(j+1)} (S (t_{j+1}) \& S(t_j)) / P_j (S (t_j)) \qquad (TP)$$

A new symbol '»' has been introduced to represent the actual physical transition from state $S(t_j)$ at t_j to state $S(t_{j+1})$ at t_{j+1}. The symbol characterises what is distinct about a *transition*, namely the actual dynamical *change* or transformation, of the state. Consequently one must distinguish carefully the probability of a state to state transition from the conditional probability of one of the states conditional on the other. $P (S(t_j)$ » $S(t_{j+1}))$ expresses the probability of a transition, while $P (S(t_{j+1}) / S(t_j))$ expresses the probability of the later state conditional on the earlier one. Conditional probability does not require nor entail a dynamical process that physically transforms the prior into the present state; it simply expresses statistical dependencies between different states regardless of what goes on 'in between'. (Conditional probability is compatible with such a process – the point is that it neither requires it nor does it ascribe it a probability). In other words (TP) and (CP_j) are not equivalent in the fundamental sense that they do not express the probability of the *same event*. (TP) expresses the probability of a dynamical

interested in the meaning that these expressions, and the corresponding concepts, may have in the absence of any assumptions regarding becoming or any other asymmetry in time. So he is right in considering them as distinct possibilities. The only reason I ignore (CP_{j+1}) in what follows is that all the considerations in the text above against reading (CP_j) as a transition probability apply just as well to it.

change of state and it presupposes that such events exist and moreover that they may be meaningfully represented in the sigma field that constitutes the domain of the probability function. (CP_j) by contrast expresses a conditional probability of the state at a certain time given the state at another time, and it is perfectly legitimately well defined on a sigma field where only states are represented. It does not require *changes* or physical transitions from one state to another to be represented in the domain of the probability function; in fact it does not require such changes or transitions to be events at all.

The advantage of starting out with (TP) as a definition of transition probability is that it becomes immediately clear that a good amount of substantial argument would be needed to show that transition probabilities conceptually reduce to conditional probabilities of *either* the (CP_j) or (CP_{j+1}) types. [36] In particular, the argument required is not simply formal, but would imply a difficult to justify restriction of the sigma fields over which these functions are defined.

Transitions are not conditionalisation processes

Transition probabilities (TP) are also distinct from Bayesian conditionalisation events, which are often taken to express the rule for rational change of subjective degree of beliefs:

$$P_{j+1} (S (t_{j+1})) = P_j (S (t_{j+1}) / S(t_j)) = P_j (S (t_{j+1}) \& S(t_j)) / P_j (S (t_j)) \qquad \text{(Cond)}$$

Conditionalisation is often invoked by Bayesians as a mechanism for the updating of rational degrees of belief in theories, laws, or other general hypotheses. It is rather unclear what it could possibly mean in the context of state-transitions. It could start to make sense if we could speak of a learning process whereby some agent first learns that

[36] A different further question is whether these probabilities (in particular (TP) and (CP_j), whenever they are both well defined) should coincide numerically for the initial and final states of any state transition. A study of the conditions under which they coincide is beyond the reach of this essay – but it seems to me to be an interesting and promising research project.

state $S(t_j)$ occurs, and then wants to update her estimate of the probability of $S(t_{j+1})$ in light of this new knowledge. However, the corresponding change in degrees of belief would take place at time t_{j+1}, the time at which the state changes to become the new state $S(t_{j+1})$. So by the time we are supposed to update, the new state already has objective probability one. Why would anyone want to use conditionalisation in order to update her degree of belief in a state that has already occurred? Why, more generally, conditionalise on the basis of information that is already old? Whatever (Cond) means it is certainly formally distinct from the expression of a transition probability (TP) – the latter neither requires nor entails any updating rule for the probabilities at any given time. [37]

Biased and unbiased samples

The key to transition probability is the expansion of the sigma field of a probability function in order to include a representation of physical transitions or state-changes. An interesting question is whether this compromises the notion that an unbiased data sample must be in equilibrium since we know that samples out of equilibrium may generate qualitative time asymmetries between forwards and backwards transition frequencies (c.f. the discussion of Bacciagaluppi's argument in section 1). There is reason to think that it does if there is reason to think that physical transitions or changes of state necessarily take place forwards in time. If so, the only events that are additionally represented in the sigma field of a transition probability are physical changes of state *forwards* ($S(t_j) \gg S(t_{j+1})$), but not physical changes of state *backwards* ($S(t_{j+1}) \gg S(t_j)$). As a result the data samples can at best contain the former type of events but not the latter. Not surprisingly, forwards transition frequencies over these data samples will appear to be time invariant but not so backwards transition frequencies. [38] In the view defended in this essay propensities are represented by forward looking transition probabilities. So in this view it is automatic that forwards transition frequencies measure the relative outcomes of genuine dynamical changes, while backwards transition frequencies are

[37] For a different argument to a similar anti-Bayesian conclusion see Guerra (2009, chapter 8).
[38] See Arntzenius (1995, esp. section 2) for a detailed example and discussion.

merely relative ratios of states calculated by means of the forwards transition probabilities and initial conditions. [39]

I conclude that the ontological primacy of forwards over backwards transition probabilities can only be denied if either i) genuine physical changes of state occur backwards as well as forward in time, or ii) no genuine physical changes of states occur ever at all. The former option entails denying that propensities, or objective transition probabilities, are time oriented. The latter option entails denying that such things as propensities, or transition probabilities, exist at all – in either direction in time. Both entail a major shift in our ordinary ontology.

5. **PROPENSITY AS PROBABILITY**

Transition probabilities are thus probabilities of genuinely physical changes of state. They somehow reflect the tendencies or propensities that systems possess to exert such changes. How should we represent them? I will adopt the view that quantum propensities are displayed in probability distributions, namely the usual transition probabilities provided by Born's rule. In this section, I elaborate on the notion of propensity that underlies their discussion, in particular with reference to some of the key texts and positions in the more general literature. I first distinguish the notion of propensity discussed in the book from the more widely known *propensity interpretation of probability*. I then discuss some historical precedents for the sort of view that I discuss

[39] Penrose (1989, pp. 355-359) defends an apparently similar view regarding the quantum mechanical algorithm for computing transition probabilities (the Born rule) in general. He claims that the algorithm can err if applied to compute backwards state-transitions: "The rules […] cannot be used for such reversed-time questions" (ibid, p. 359). The representation of transition probabilities proposed here makes it clear why this should be the case.

here. Finally, I address the principal objection against the propensity interpretation in recent years, namely "Humphrey's paradox". [40]

Long-run versus Single Case Propensities

The philosophy of probability literature appropriately distinguishes two types of propensity interpretations: long run and single case. [41] The difference between these two types lies in the object that is identified as the propensity. Long run interpretations of propensity identify propensity with the dispositional property of a chance set up to generate frequencies in sequences of outcome trials, while single case interpretations identify it with the tendency to generate a particular outcome in a given trial. There are at least two long run interpretations: those which accept infinite virtual sequences and those which accept only long yet finite sequences. All long run interpretations have the following in common: a chance set up (an arrangement of distinct parts capable of generating a sequence of stochastic outcomes of some trial) may possess a propensity for some type of outcome if and only if the limiting frequency of such a trial outcome is well defined in each (long but finite, or virtual and infinite) sequence. Hence long run propensity interpretations agree with frequency interpretations in requiring sequences for the ascription of probabilities. The difference is that a long run propensity interpretation will not focus on the properties of the sequences (frequencies) but rather on the properties of chance set ups that generate those sequences. In other words, a long run propensity interpretation does not identify probability with frequency, but with the tendency to generate the frequency instead.

Similarly, a single case propensity interpretation will not identify probability with any trial outcome but with whatever dispositional property generates a particular trial

[40] The view of propensities that I shall be defending here is very much my own (see Suárez, 2004, 2007a), and none of the contributors in the book has explicitly committed to it. However I believe that this view, or a similar one, is required for the coherence of many pronouncements made in the book, particularly in the third part. If so, we may take this or a similar view to be implicit in the book, and its defence in this section to provide support for it.
[41] Gillies (2000a, pp. 124-126); Fetzer (1981, chapter 5).

outcome. So a probability in this case is a tendency that is exerted in every trial; no frequency in any finite – however long – sequence of such trials may fail to agree with the particular probability. The only frequencies that, on a single case propensity interpretation, need to agree with the probabilities are those pertaining to the virtual infinite sequences that would be generated if it were possible to repeat the same experiment an infinite number of times. Yet, unlike the long run propensity interpretation, the single case interpretation does not identify propensity with the tendency to generate any frequency, whether finite or infinite. Rather, it associates propensity with the tendency to generate *each* particular outcome in the sequence. [42]

When authors in the book discuss propensities they almost invariably have in mind a single-case interpretation. There are, however, a number of interesting differences among different single case interpretations and it is worth to review them quickly. [43] Gillies divides propensity interpretations into two types depending on what is regarded as an appropriate chance set up – i.e. the set of conditions that must obtain at a given time for the appropriate tendencies to be instantiated. Humphreys by contrast divides single case propensity interpretations into three additional types differing in their account of dynamics for propensities – i.e. their time evolution over a period and their effect on different events at successive stages.

Let me consider Gillies' taxonomy first, which divides all propensity interpretations into *repeated conditions* and *state of the universe* interpretations. The chance set up may be a simple enough arrangement that could be specified by means of just a few free variables or parameters. (The toss of a coin is an example). If so, a chance set up is defined by just a few conditions that are repeatable and hence allows for the same sort of trial to be repeatedly carried out. A single case interpretation of this sort implicitly requires all propensities to be conditional on such a set of repeatable conditions. Alternatively, a chance set up may include the complete hypersurface

[42] Long run propensities as tendencies to generate long but finite sequences are defended by Popper (1959), and as tendencies to generate long but finite sequences by Gillies (2000a, Ch. 7). Single case propensities are defended by Fetzer (1981, Ch. 5) and Miller (1994).

[43] I essentially follow the exposition in Gillies (2000a) and Humphreys (2004) and introduce further considerations along the way.

corresponding to a particular time *t*. If so, a chance set up is defined by the whole state of the universe at *t*. This type of single case interpretation too requires all propensities to be conditional – albeit conditional on a complete hypersurface. [44] In either view, there are no absolute propensities Pr (A_t) for any event or proposition A at any time *t*. Any seemingly absolute propensity is really a conditional propensity, Pr (A_t / $S_{t'}$) with t' < t, where S is either the full state of the universe at *t'*, or the particular set of conditions required by an appropriate chance set up at *t'*.

On the assumption that all propensities are conditional, Paul Humphreys provides a different taxonomy based on the dynamical evolution of conditional propensities. [45] A *coproduction interpretation* assumes that the conditional propensity is fixed once and for all at the initial time *t* whether by a particular set of relevant conditions at *t* or by the *t* hypersurface or time slice. Thus all propensities carry an implicit time index which need not coincide with the time index of either conditioned or conditioning event. For example Pr_t ($A_{t''}$ / $S_{t'}$) is the propensity at *t* for A at *t''* given S at *t'*. Under the natural assumption that t < t' < t'' a coproduction interpretation assumes that the conditional propensity of $A_{t''}$ given $S_{t'}$ is already fixed at the original time *t* given the background conditions at that time. A *temporal evolution interpretation* by contrast assumes that propensities evolve continuously in time so the propensity of $A_{t''}$ at *t* need not be identical to that at *t'*. The conditional propensity of $A_{t''}$ given $S_{t'}$ must then be evaluated at *t'*: $Pr_{t'}$ ($A_{t''}$ / $S_{t'}$) as the temporal update of the original propensity Pr_t ($A_{t''}$ / $S_{t'}$). Finally, a *renormalisation interpretation* assumes that updating is necessary even though there is no continuous temporal evolution of the propensity. (The difference between the renormalisation and the temporal evolution interpretations is that the former does not presuppose continuous evolution so updating in intermediate stages is not required. In the temporal evolution

[44] The different interpretations are then classified as follows: Fetzer (1981) defends a single case repeated conditions interpretation, while Miller (1984) defends a single case state of the universe interpretation. Gillies (2000a, pp. 130-36) argues that these interpretations succumb to Humphrey's paradox, and defends instead a long run repeated conditions interpretation.

[45] Humphreys (2004).

interpretation, by contrast, an updating at *t''* of a propensity first defined at *t* necessarily requires an intermediate updating at *t'*).[46]

The two taxonomies are orthogonal and, in principle, any of the 15 combinations is logically possible. Humphreys and Gillies in effect argue that as long as applied to single case propensities all fifteen of them are ruled out by Humphrey's paradox. In what follows I review the notorious paradox. For now I just note that all propensity interpretations so far analysed have one thing in common: they presuppose that there are no genuine absolute propensities and that all propensities are implicitly or explicitly conditional. Later on I shall argue that there is nothing in the dynamical interpretations *per se* that implies that this should be the case; and that there are alternative ways of understanding both *relevant conditions* and *state of the universe* interpretations.

Humphrey's Paradox

'Humphrey's Paradox' (HP) was first described by Wesley Salmon (1979, pp. 213-4) and James Fetzer (1981, p. 283) who was also responsible for naming it. Most commentators describe it not so much as a 'paradox' as a powerful argument against the propensity interpretation of probability.[47] The key idea underlying this argument is roughly that propensities partake of the asymmetry of causation in a way that probabilities do not. But if propensities are causally asymmetric and probabilities are not, then they can not be the same kind of thing. Hence a wholesale propensity *interpretation* of (the classical – Kolmogorov - calculus of) probability is out of the question.

Let me use a simple everyday example to try to make the rough idea a bit more precise. Some of my friends have remarked on my propensity to travel to North America in the spring. On the basis of the relative frequency in the last ten years, we may estimate

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[46] Humphreys actually lists a fourth case, the *causal interpretation* (Humphreys, 2004, p. 673). However, the causal interpretation is not really on a par with the other three since it is not *per se* a dynamical interpretation of the evolution of propensities. In fact it does not seem to exclude any of the other three dynamical interpretations, being rather compatible with any of them.

[47] Fetzer (1981, pp. 283-286); Gillies (2000a) and (2000b); McCurdy (1996); Miller (1994); Milne (1986).

the probability corresponding to this propensity roughly at P (NA / S) = 0.9 (where NA is my travelling to North America, and S stands for the – northern hemisphere – spring). We can then apply Bayes theorem in order to find out the value of the inverse probability of spring conditional on my travelling to North America: P (S / NA) = P (NA / S) x P (S) / P (NA). Dividing the year in four seasons and applying some estimates for the priors, we obtain P (S / NA) = 0.56. Let us suppose that there is a set of causal facts {F} underlying my friends' propensity adscription along the lines of the intended implication, namely that {F} are features unique to the spring season that attract me to North America, and cause me to travel there. We can suppose that {F} includes (in addition to facts regarding the seasonal weather in spring in both continents) some facts about my psychology, habits and values, my work schedule, my family and financial situation, etc. Whatever these causal facts {F} are, they fail to underpin similarly any propensity corresponding to the inverse probability. For whatever it is that causes me to travel does not also cause spring. In these terms, the inverse probability P (S / NA) does not seem to have any possible causal underpinning. The relevant causal facts relate to the conditioning event S, while the effects of interest relate to the conditioned event NA. But the inverse probability has inverted conditioned and conditioning events. And it is implausible to suppose that there are other facts {F'} about North America – or about my travelling there – that cause or bring about spring with a 0.56 chance. (Certainly those very causal facts which underlie my propensity to travel there in the spring do not probabilistically cause it to be spring when I travel; so {F'} ≠ {F}; and it is hard to see what other facts could be cited). [48]

On the basis of examples like this, many commentators have asserted that Humphreys' Paradox shows that very many well defined conditional probabilities are not propensities. This seems to rule out the propensity interpretation of probability in general since there is nothing about P (S / NA) that makes it in any way suspect as a well defined probability (certainly not as long as P (NA / S) is well defined too). Notice that there are two assumptions underlying this use of the example. The first (*Assumption* 1) is that the

[48] For the convenience of the story, I am assuming that the relata of causation are facts along the lines of Mellor (1995). But the argument does not hinge on this assumption.

propensity interpretation applies to conditional probabilities. [49] The second (*Assumption 2*) is that a propensity interpretation applies only when the conditioning event is a cause or partial cause of the conditioned event. This assumption trades on a supposedly intimate link between propensity and causation whereby the former inherits the asymmetry characteristic of the latter.

Paul Humphrey's own version of HP is not explicitly built on either of these two assumptions. But the assumptions are brought in implicitly. This is perhaps clearest in the discussion of the notorious example involving the transmission and reflection of a photon from a half-silvered mirror. [50] A source emits photons spontaneously; a few of these photons reach the mirror; among these a few are actually transmitted. Now let us consider the propensity for a single photon to be emitted at the source at time t_1; to hit the mirror at time t_2; and to be transmitted at time t_3. And let us consider the complete state of the source and mirror at time t_1; i.e. after the emission of the photon at the source. Humphreys invites us to consider the following assignment of propensities at time t_1:

i) $Pr_{t1}\ (T_{t3} / I_{t2}\ B_{t1}) = p > 0$

ii) $1 > Pr_{t1}\ (I_{t2} / B_{t1}) = q > 0$

iii) $Pr_{t1}\ (T_{t3} / \neg\ I_{t2}\ B_{t1}) = 0$

where B_{t1} represent the background conditions at t_1; I_{t2} the incidence of the photon upon the mirror at time t_2; and T_{t3} the transmission event of the photon. According to Humphreys these three propensity ascriptions are entailed by the physical and experimental circumstances. They do not follow from the formal features of the calculus of probability because the arguments in the propensity functions designate physical events and do not necessarily pick out subsets of a measure theoretic outcome space. [51] Indeed once the formal framework for the representation is chosen the content of

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[49] This need not rule out absolute propensities, although some commentators – notably Gillies (2000a, pp. 131-132) – go further to claim that all propensities are implicitly if not explicitly conditional. In this view a propensity interpretation of probability is always of (and only of) conditional probability.

[50] Humphreys (1985, p. 561).

[51] I have adopted Humphreys' suggested terminology and refer to propensities as Pr (-) and probability functions as either Prob (-) or simply P (-).

ascriptions i), ii) and iii) is not formal but empirical. However, it does not follow from the physical and experimental circumstances that the propensities involved are conditional nor does it follow that they must be formally represented in a way akin to conditional probabilities. This is a point that I shall take on later – and demonstrates that *Assumption 1* is built into the discussion of the example.

Humphreys invites us next to consider the following principle of conditional independence for propensities: [52]

Conditional Independence (CI): $Pr_{t1} (I_{t2} / T_{t3} B_{t1}) = Pr_{t1} (I_{t2} / \neg T_{t3} B_{t1}) = Pr_{t1} (I_{t2} / B_{t1})$.

Together with the ascription of propensities above, this principle contradicts the (Kolgomorov) axioms of classical probability. The contradiction with the fourth axiom, in the form of Bayes Theorem for conditional probability is particularly easy to show. [53] So, at least one among these assumptions must go. Some responses to HP have focused on trying to show that principle CI is false when applied *to this particular example*. [54] But in retort Humphreys produced yet another example that conclusively obeys CI. [55] Other authors endorsed the HP argument as a definitive reason to abandon the propensity interpretation altogether. [56] Humphreys himself concluded that the axioms of classical probability can not represent propensities accurately. But instead of abandoning propensities, he recommends abandoning the classical (Kolmogorov) calculus of probability as a representation of chance or objective probability.

Conditional Propensities

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[52] Humphreys (1985, p. 561; 2004, p. 669).
[53] Humphreys (1985, p. 562).
[54] McCurdy (1996).
[55] See Humphreys (2004). My objections below to CI are very different in nature and cannot be answered by means of new examples.
[56] Milne (1986).

The CI principle and its use in the derivation of Humphreys' Paradox require some careful analysis. Strictly speaking CI merely states that the propensity of the photon impinging on the mirror at t_2 is independent of the (later) event of transmission at t_3, and depends only on the background conditions at t_1. But Humphreys seems to think that the actual principle of conditional independence is more general, and CI as formally expressed above is merely a consequence of such a general principle. For he writes that the CI principle 'claims that any event that is in the future of I_{t2} leaves the propensity of I_{t2} unchanged. […] This principle reflects the idea that there exists a non-zero propensity at t_1 for I_{t2} to occur, and that this propensity value is unaffected by anything that occurs later than I_{t2}.' (Humphreys, 2004, p. 670).

Thus conditional independence in general, unlike CI in particular, applies to *any event* later than t_2, and not just to T_{t3} in particular. So the expression above is not a definition of conditional independence in general, but rather the application of conditional independence to the particular example. The main intuition is presumably that the propensities of the photon at t_i can be altered only by events at times $t < t_i$. But the only reason to suppose this is the temporally asymmetric nature of the "altering" relation – so *Assumption 2* is involved after all. More generally the intuition seems to be that a system's propensities at {x, y, z, t} can only be altered by events in {x, y, z, t}'s past light cone. If so, CI presupposes the view that propensities are time asymmetric in just the way causation is asymmetric in relativity theory under the 'causal' interpretation: no cause can lie outside the past light cone of its effects. So, a version of *Assumption 2* is again built into the application of a general principle of conditional independence to the photon example.

How plausible is this relativistic version of *Assumption 2*? There are many good arguments against the 'causal' interpretation of special relativity. [57] And even in a non-

[57] See Maudlin (1995), particularly chapter 5.

relativistic setting, *Assumption 2* is inconclusive since backwards in time causation in a fixed frame has not been decisively ruled out. [58]

Humphreys claims that CI holds in the *co-production interpretation* of propensities, [59] presumably because in this interpretation all propensities are fixed at the initial time t_1. But if this grounds independence at all, it is the very general claim that all propensities *at time later than t_1* (including therefore but not only the propensity for T_{t3}) are independent of the propensity for I_{t2}. This claim goes well beyond the general conditional independence that we have considered so far – which included only events in the future of t_2. The *co*-production interpretation on its own grounds CI but it also grounds other similar independence conditions that we would not want to have to assert in this case. The only apparent way to extract precisely CI out of the co-production interpretation is by adding *Assumption 2* or a similar causal principle. The co-production interpretation, in conjunction with *Assumption 2,* then entails that I_{t2} is conditionally independent with respect to those events outside of its proper past light cone. In particular it follows that I_{t2} is conditionally independent of T_{t3}, as stated in CI. So, CI requires *Assumption 2* after all, even in the co-production interpretation. [60]

Humphreys argues against the *co-production interpretation* anyway, on the basis that it is not a genuine single case propensity interpretation. He claims that it does not classify conditional propensities as real conditional chances in an ontological sense, but only in the measure theoretic sense. [61] I suppose that he must have in mind the view that at time t_1 all of the probabilities are fixed for all the propensities afterwards. So barring

[58] In fact many of the arguments against backwards in time causation turn out to depend on the fine grained space-time structure of the putatively refuting examples. Others, such as the bilking argument, attend to agency only, but seem inconclusive. See Black (1956) and Dummett (1964) for two classic sources and discussion.
[59] See for instance the table in Humphreys (2004, p. 677).
[60] We may wonder about the status of conditional independence in other interpretations of propensities. CI holds in the temporal evolution interpretation – since the propensity of I_{t2} is updated at time t_2. So Pr_{t2} (I_{t2} / T_{t3} B_{t1}) = Pr_{t2} (I_{t2} / ¬ T_{t3} B_{t1}) = Pr_{t2} (I_{t2} / B_{t1}) = 1 or 0. But it fails in the renormalisation interpretation since Pr_{t3} (I_{t2} / T_{t3} B_{t1}) ≠ Pr_{t3} (I_{t2} / ¬ T_{t3} B_{t1}) in general. However, Humphreys (2004, p. 673) finds that a similar principle holds in the renormalisation interpretation, namely the *fixity* principle. (The fixity principle states that: Pr_{t1} (I_{t2} / T_{t3}) = 0 or 1, which holds in the renormalisation interpretation since Pr_{t3} (I_{t2} / T_{t3}) = 0 or 1). In all cases, I contend, Assumption 2 is implicit in the derivation of CI.
[61] Humphreys (2004 p. 675)

the very ascription conditions at time t_1 and events prior to this, all other events are included in the outcome space and must be represented in the sigma field that defines the probability function.

But if this is a reason to reject the *co-production interpretation*, it is also a reason more generally to reject the representation of propensities as conditional probabilities. Let us accept like Humphreys that a ‘conditional propensity’ is a *sui generis* ontological relation between two events (or event types) *a* and *b*. This relation is entirely independent of any formal representation in measure theory (given the typical underdetermination of mathematics by physics it is in fact natural to suppose that the same propensities may be represented by means of very many different measure functions). Why are we then obliged to represent them by means of the standard representation for conditional probabilities? Why are we obliged to provide a measure theoretic representation at all?

6. **PROPENSITY AS DISPOSITIONAL PROPERTY**

As long as propensity is understood as an *interpretation of probability*, we have no choice. Probability is routinely represented in measure theoretic terms, and there are even some good representation theorems. [62] But why suppose that propensity *interprets* probability? [63] Once the idea has been given up that propensity is a particular kind of probability, or an interpretation of the term ‘probability’, it becomes possible to suppose that the relation between these two terms is something different; for example, something akin to theoretical explanation.

[62] As good as they come – typically not up to uniqueness. In particular, and rather to the point, the fourth Kolmogorov axiom is sometimes disputed – see, e.g. Hajek (2003).
[63] Why suppose that objective probability, or chance, requires any interpretation at all? After all many theoretical concepts bring their own interpretation and / or require no interpretation at all. Elliott Sober for one has recently argued for a no-theory theory of probability in Sober (2005).

Propensities Display Probabilities

Propensities and objective probabilities are distinct notions and it is the job of a propensity theory to establish how they are conceptually related. The two theories that have fundamentally taken this insight to heart are due to Hugh Mellor (1971) and James Fetzer (1988). Their view is that propensities are dispositional properties that are displayed in probability distributions but may not be identified with them. Instead of providing semantics for probabilities in the model-theoretic sense, propensities may be said to *explain* probabilities since they explain how a certain distribution rather than another one comes about in specific circumstances. But if we accept this understanding of propensities as dispositional properties, there seems to be no reason why the relations between such properties need be represented as conditional probabilities. Consider first the relation between the possession and manifestation conditions of a propensity, such as those involved in the fragility (F) of a glass and its breaking (B). Supposedly this is a deterministic disposition under certain conditions C; we may assume that it displays the conditional probability P (B / F & C) = 1. Every fragile glass that is hit under specific conditions (certain strength, etc) will break. But why represent this propensity as a conditional probability? Under different conditions D, the same propensity gets displayed in a probability of breakage less than one: P (B / F & D) = $x \leq 1$. So, in general, it makes sense to formally distinguish propensities from the probability distributions that display them.

There are at least three alternatives to the conditional probability representation. First, we may represent the displays of propensities always as absolute probabilities in the restricted probability outcome space. Thus, instead of writing P (B / F & C) and P (B / F & D) we may always write $P_{F\&C}$ (B) and $P_{F\&D}$ (B), defining these probability functions on the smaller space. Since the functions are different, their values may correspondingly differ too. The advantage of this representation is that every probabilistic display of a propensity ascription is then relative to a set of circumstances or manifestation conditions. The disadvantage is that it does not allow us to ascribe probabilities to the propensities themselves since {F, F' etc.} are not represented in the sigma field that defines the probability.

An alternative is to come up with a distinct representing symbol for what is, after all, a distinct relation. There are at least two different ways of doing this. We may first consider transitions of state, and put to use the notation that we devised to this effect in the previous section. Thus, $F »_c B$ denotes the transition from the dispositional 'state' F to the manifestation 'state' B under circumstances c. In the case of propensities the manifestation property is itself a probability distribution, and we may write $F »_c P(B_i) = p_i$ where B_i are the different possible values of the manifestation property B. In this representation the outcome space is includes *both* property possession and property manifestation events as part of the propensity and manifestation 'states'. So, we can define probability distributions over propensities, manifestation properties, and their transitions. I shall for the most part employ this notation in my discussion below.

Yet, note that another alternative would allow us to represent the relations among different propensities, whereby the possession of some propensity may causally affect another set of propensities. This is obvious in the case of logical entailment among properties, which may be modelled as deterministic causation. (A typical macroscopic case is colour under a dispositionalist reading; so for instance being white ipso facto entails being coloured, etc). But in addition there may be genuine 'productive' causation among dispositional properties. [64] Both may be understood under a very general causal relation and represented by some appropriate symbol such as "↪". [65] We may then write 'A causes B' as "$A \hookrightarrow B$". We saw in section 3 that there is a debate in the literature about whether propensities cause their manifestations. If it is the case that the manifestation relation is causal, then we can write $P_c(F \hookrightarrow B_i) = p_i$ instead of $F »_c P(B_i) = p_i$ without loss of generality. However, in line with my previous discussion I shall not assume that the manifestation relation is itself a causal relation, but shall instead employ

[64] For the distinction between 'productive' and 'dependence' or counterfactual causality, see Hall (2004).
[65] The symbol employed by Cartwright (1983, chapter 3) for this relation.

the ‘neutral’ notational system for transitions of state in general. From now propensities and their probabilistic distributions shall be denoted as F » P (B_i) = p_i where I shall drop the c subscript for convenience.

Absolute Propensities

We are now able to represent changes of propensity state as follows. Suppose that S_1 is the full state of the system expressing all its properties, whether dispositional or not, at time t_1 and S_2 is the full state at time t_2. Then S_1 » P (S_2) = p expresses the fact that the transition probability for a change of state S_1 into S_2 is p. This notation makes it unnecessary to represent a transition probability as a conditional probability P (S_2 / S_1) = p. As we saw in section 4 the conditional probability notation for transition probability is not only unnecessary but undesirable.

There are a number of advantages to this new notional system for propensities. Let me just comment on two of them since they relate to issues that were already mentioned in this essay. First, I address the distinction between different long run propensity theories that were reviewed in section 4. Second, I address some difficulties related to the principle of indifference that were briefly mentioned in the summary of Bangu’s paper in section 1.

Firstly, in section 5 Gillies distinction between *repeated conditions* and *state of the universe* interpretations was reviewed. Let us continue to refer to the propensity as F. In the standard propensity interpretation of probability this propensity is identified with the corresponding conditional probability: F = Pr_t (A_i / $S_{t'}$) = p_i where the {A_i} are the values of a given quantity to be measured at time t, the {p_i} represent their probabilities, and $S_{t'}$ is either the (hypersurface) state of the universe at t’ or the set of repeated conditions at t’ (with t’ < t). However, in the account defended here, these long run propensities must be reformulated as dispositional properties that display absolute probabilities. In accordance with our notation, we must write F $»_{St'}$ P_t (A_i) = p_i when under the circumstances S at t', the propensity A manifests itself as a probability distribution over the values of A at t. We leave open whether $S_{t'}$ represents the state of the universe at t' or the set of repeated conditions at t'. In either case propensities are dispositional properties that ensue – or evolve into – probability distributions. The conditional probability representation is altogether unnecessary.

Secondly, in the discussion of Bangu's chapter in the first section, a source of difficulties associated with the principle of indifference was mentioned. In particular, I voiced the concern that the principle may invite an illegitimate inference from a merely epistemic fact about our knowledge (or lack thereof) to an objective fact about the physical world – and in particular about its dispositional properties. I can make the claim more precise now. Under a conditional propensity account such as Humphreys, the principle of indifference leads from facts about our lack of knowledge regarding the outcome of a particular experiment to an incorrect ascription of objective properties in the world. For instance, under total lack of knowledge regarding the outcomes of an experiment A performed under repeated conditions S we would be advised by indifference to ascribe equal probability to all such outcomes and the corresponding propensity would be given by $Pr(A_i / S) = p_i$, with $\Sigma p_i = 1$ and $p_i = p_j$ for any i, j. It seems clear that no knowledge (or lack thereof) of any finite sequence can justify such an ascription of a propensity. So, under this construal of propensities, the principle of indifference leads to an incorrect ascription of objective facts about the physical world, namely its propensities. [66]

Now, interestingly, the problem disappears as soon as a dispositional account of propensities is embraced, with a concomitant representation in terms of the notation that we have developed. We must then write $F \gg_S P(A_i) = p_i$ for the manifestation of F as A under circumstances c. It is then perfectly possible to apply the principle of indifference in order to fix the values of p_i in the absence of any knowledge regarding the outcomes. We obtain that $\Sigma p_i = 1$ and $p_i = p_j$ for any i, j, as in the previous case. However, we now

[66] The problem is most acute for long run propensity theories. Gillies (2000a) attempts to solve the problem by appealing to the notion of a falsifying rule for statistical hypothesis. But this is a controversial solution as discussed in the summary and discussion of chapter 2.

make no statement whatever regarding the propensity F that underlies this distribution. The principle of indifference applies only to the probability distribution that displays F but not F itself. Thus, we no longer commit the fallacy of going from lack of knowledge to objective facts. [67]

Humphreys' Paradox Revisited

Let us now bring the discussion to bear on Humphreys' Paradox. We are discussing the view that propensities are dispositional properties that are manifested as probability distributions under the appropriate circumstances. Suppose that under circumstances *c* propensity *F* is displayed as the probability distribution *P* over the values of some manifestation property *B*. I have argued that this is appropriately expressed as $F \gg_c P (B_i)$. One of the relata of the manifestation relation is a probability distribution – in agreement with the thought that propensities manifest themselves in probability distributions.

It should be obvious that symmetry fails on this representation. It does not follow from $F \gg_c P (B_i)$ that $B \gg_c P (F_i)$; it does not even follow that B has any manifestation properties at all! [68] The 'inverse' manifestation relation is not generally well defined. Moreover, Bayes Theorem has no application in these cases since all the probabilities are absolute and not conditional. So even restricting ourselves to the probability distributions that display the propensities, the 'inverse' probabilities need not be well defined either.

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[67] There is an interesting question here for the 'causal' notation alternative mentioned earlier. In that case we would write $P (F \hookrightarrow A_i) = p_i$ with $\Sigma\, p_i = 1$, and $p_i = p_j$ for any i, j. Here the application of the principle of indifference would lead us to infer objective facts. However, these facts do not regard the distribution of propensities but refer exclusively to the causal efficacy of propensities in generating distributions. It is an open question to what extent such an inference is prohibited by the sort of arguments routinely employed against the principle of indifference. Bertrand style paradoxes, for instance, are prima facie inapplicable given the apparent absence of any causal relations in those geometrical examples. This is an interesting topic for further work.

[68] Note that failure of symmetry is the case in the 'causal' notation too. Thus it does not follow from $P_c (F \hookrightarrow B)$ that $P_c (B \hookrightarrow F)$. It does not, in fact, follow that F has any causes at all, never mind that B is one of them.

One possible objection is that there is always an equivalent representation in terms of conditional probabilities. However, I do not think that a conditional probability representation of the above manifestation and, more specifically, causal relations is possible without significant loss of meaning. As we already saw in section 4, transition probability, which is possibly the most favourable case for the equivalence claim, is best understood as a change of propensity state and not as the outcome of conditioning.

In this account, the reasoning underlying Humphreys Paradox goes wrong at the very start. The representation of photon state transitions as ‘conditional propensities’ (i.e. conditional probabilities) is incorrect. Instead, these processes should be represented properly as involving probabilities for manifestation or causal relations between propensities. It is the photon incidence upon the mirror that manifests itself in its transmission (or partially causes it together with the background conditions at t_1). The incidence of the photon is a manifestation of its ejection or at least partially caused by it. Etc. The first three conditions should then be re-expressed accordingly:

i) $I_{t2}\ B_{t1} \gg P_{t1}\ (T_{t3}) = p > 0.$

ii) $B_{t1} \gg P_{t1}\ (I_{t2}) = q$, where $1 > q > 0$.

iii) $\neg\ I_{t2}\ B_{t1} \gg P_{t1}\ (T_{t3}) = 0.$

These equations represent the probabilities displayed by propensities and their relations. Since these probabilities are absolute, Bayes Theorem has no significant application. It is impossible to derive from these conditions a violation of Bayes Theorem whether in conjunction with a conditional independence principle such as CI – or any other of the principles discussed such as the zero influence or the fixity principle. [69]

[69] The same conclusion follows in accordance to the ‘causal’ notation. Humphreys conditions would be formalised as follows: i) $P_{t1}\ (I_{t2}\ B_{t1} \hookrightarrow T_{t3}) = p$; ii) $1 > P_{t1}\ (B_{t1} \hookrightarrow I_{t2}) = q > 0$; iii) $P_{t1}\ (\neg\ I_{t2}\ B_{t1} \hookrightarrow T_{t3}) = 0$. Since Bayes Theorem has no application, no contradiction can ensue.

7. **CAUSAL AND DISPOSITIONAL PRESUPPOSITIONS IN PHYSICS**

The overall outlook of the book is decidedly in favour of dynamical, causal, or dispositional presuppositions underlying the practice of probabilistic modeling in science. The authors find that probabilistic modeling often carries an implicit or explicit commitment to such notions. When it does not implicitly or explicitly carry such a commitment, it often needs to be supplemented with some inferential rules that can be grounded only upon such notions. Thus, transition probabilities express dynamical processes; the selection of probabilistic hypotheses often requires information regarding the physical properties of the systems described; and the explanation of equilibrium in statistical mechanics requires essential reference to the dynamical character of statistical laws. Causal hypotheses and causal reasoning are required to understand statistical inference in quantum correlation phenomena; such causal hypotheses may imply some temporal orientation on pain of causal paradoxes or loops. On the other hand, a proper analysis of these questions requires philosophers to come to grips and apply the latest techniques in the field of causal inference, including the latest versions of the principle of common cause and the causal Markov condition. Finally, quantum systems are likely endowed with dispositional properties that get displayed under the appropriate circumstances as the characteristic probability distributions provided by Born's rule.

In this introduction I argued that these diverse presuppositions are interlinked in many different interesting ways. For instance, transition probabilities must be understood as the probabilities of dynamical changes of state, and often express a system's dispositional properties. The manifestation of propensities may be understood as a kind of causal relation between the possession conditions and the manifestation outcomes. Statistical inference from frequencies to probabilities in quantum mechanics often requires causal hypotheses which are extremely sensitive to the particular interpretation of quantum mechanics employed in deriving those models. Etc. Every single one of those connections opens up a host of interesting philosophical problems and issues. The book demonstrates that work in the foundations of physics calls for deep and sustained philosophical reflection on such issues.

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