Curious and sublime: the connection between uncertainty and probability in physics

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Let any one try to account for this [probabilistic] operation of the mind upon any of the received systems of philosophy, and he will be sensible of the difficulty. For my part, I shall think it sufficient, if the present hints excite the curiosity of philosophers, and make them sensible how defective all common theories are in treating of such curious and such sublime subjects.' David Hume [1].

When you come to a fork in the road, take it. Lawrence Peter ("Yogi") Berra

Probabilities make just as much sense if all possible results occur as if just one does. David Papineau [2].

Abstract

From its first significant appearance in physics, the notion of probability has been linked in the minds of physicists with the notion of uncertainty. But the link may prove to be tenuous, if quantum mechanics, construed in terms of the Everett interpretation, is anything to go by.

1 "Statistical" mechanics

The question as to when the notion of probability, in its modern guise, first made its appearance in physics is open to debate. But many would agree that something important happened in the 1870s when physicists working in the fledgling kinetic theory of gases were looking over their shoulders at what might be called the rival theory of thermodynamics. (A word by way of background. The kinetic theory sits within a fundamentally mechanical paradigm involving the Newtonian dynamics of colliding gas particles and the fundamental notion that heat is an aspect of motion. In contrast, the theory of thermodynamics, in dealing with gases, or more pertinently gases within heat engines, makes no claims about the microscopic make-up and behaviour of the gas and has a more non-committal notion of heat.) Here is a famous statement made by James Clerk Maxwell in an 1870 letter to Strutt:

> The second law of thermodynamics has the same degree of truth as the statement that if you throw a tumberful of water into the sea, you cannot get the same tumberful out again.[1]

We needn't dwell on the meaning of the second law of thermodynamics. Suffice it to say that thermodynamics deals (in part) with irreversible macroscopic processes, and what Maxwell is effectively saying is that within the mechanical paradigm such processes are not *strictly* irreversible after all, but rather appear to be so for all practical purposes. Henri Poincaré, the great *fin de siècle* French mathematician, was stongly interested in thermodynamics and found the kinetic theory of gases distasteful. However, he grudgingly admitted in 1893 that the "English" view of irreversibility has some merit, even if it is not the right model for the relevant thermodynamic processes:

> ... if one had a hectolitre of wheat and a grain of barley, it would be easy to hide this grain in the middle of the wheat; but it would be almost impossible to find it again, so that the phenomenon appears to be in a sense irreversible.[2]

The word 'probability' is not explicitly mentioned in these statements, but the notion is clearly lurking in the background. Maxwell, for his part, was more explicit in 1878:

> ... we have reason for believing the truth of the second law to be the nature of a strong probability, which though it falls short of certainty by less than any assignable quantity, is not an absolute certainty.
>
> Several attempts have been made to deduce the second law from purely dynamical principles ... and without the introduction of an element of probability. If we are right in what we have said above, no deduction of this kind, however apparently satisfactory, can be a sufficient explanation of the second law.[3]

Maxwell's warning was timely. Earlier, in 1871, the great Austrian physicist, Ludwig Boltzmann, had published a purely mechanical account of the tendency of dilute gases to spontaneously approach equilibrium, and in doing so to increase their entropy — a derivation which came to be known as Boltzmann's *H-theorem*. The second part of this result having to do with entropy is naturally associated with the second law of thermodynamics, but the first part having to do with spontaneous equilibration is not. Indeed, the principle that macroscopic systems which find themselves outside of equilibrium (such as a gas squeezed into the corner of a container, and then suddenly left to expand) will, if left to themselves, approach a unique equilibrium state and stay there forever unless disturbed, is fundamental to thermodynamics and underpins all of its laws.[4] It is also the true source of irreversibility in the theory. Boltzmann

[1] See [3], footnote 14.

[2] See [4]. For a discussion of Poincaré's initially sceptical attitude towards the kinetic theory of gases, see [5]; for his late conversion to the atomistic picture, see [6].

[3] See [7], p. 280. What exactly did Maxwell mean by probability in this context? There is no simple answer: see [8, 9].

[4] Recognition of this point has recently led to some commentators to refer to the principle as the "minus first law of thermodynamics"; after all, there was already a "zeroth law"! See [10].

would seem then to have done what Maxwell claimed to be impossible. But Boltzmann quickly realized, after he received criticisms from various quarters, that his result was indeed too good to be true. He admitted, in 1877, that his result was only valid with high probability. Better put, he admitted that the H-theorem could not hold in all conceivable circumstances for a gas originally outside of equilibrium, but that it would hold for all practical purposes over any reasonable length of time.

A number of points are worth making about this episode.

(i) The first point concerns the evidence for Maxwell's and Boltzmann's probabilistic reading of the second law of thermodynamics. There was no direct empirical evidence; it was all theoretical. If you judge the probability of something to be low enough ("less than any assignable quantity"), in practice you don't expect to see it. And that is no different from what you expect when the probability is zero. There were no established cases of a violation of the second law of thermodynamics known in the 1870s. The reason, or at least the original reason, that violation of the second law of thermodynamic processes was even contemplated by practitioners of the kinetic theory of gases was theoretical; it was that the fundamental Newtonian laws of collisions between gas particles look the same whether you are facing the future or facing the past — they do not define an arrow of time. (Another way of putting this is that a slow-motion film of colliding particles played backwards would, according to the theory, show particles obeying the same laws of collision as in the film played forwards.)[5]

(ii) The second point needs a little more background. Boltzmann would go on to argue that his 1871 mechanical argument actually had the seeds of probability built into it, if only he had had the wit to see them, and even today some commentators claim (not always for the same reasons, confusingly) that the assumptions in the original H-theorem, correctly construed, are essentially probabilistic. This is all rather contentious[6], but what concerns us is Boltzmann's own thinking. In his proof of the H-theorem, Boltzmann was concerned with how the distribution function f associated with a gas within a finite container evolves over time, and used what has come to be known and celebrated as the Boltzmann transport equation to describe this evolution. The distribution function itself is defined as the number of gas particles that at any given time have simultaneous positions and velocities within any given small (infinitesimal) ranges. Essentially this amounts to a (theoretical) counting exercise, though Boltzmann explicitly referred to the function as the "probability" for any particle in the gas to have a velocity and position in the relevant ranges.[7] Despite this nomenclature, which Boltzmann would later cite as evidence that there was a probabilistic underpinning of his H-theorem all along, there was no

[5]Two decades after Maxwell and Boltzmann had given a probabilistic account of the second law, it was shown by Poincaré that an isolated finite mechanical system like a gas would, in all probability, and if one waited long enough, keep coming back to its initial macroscopic state, no matter how far from equilibrium that state was. Though the waiting times involved are typically stupendous (much larger than what we now take to be the age of the universe) this "recurrence theorem" – which has an even stronger analogue in quantum mechancis – was another reason to think that the second law could not be absolute; for further details see [5].

[6]For a sceptical analysis, see [5].

[7]In fact, the meaning that Boltzmann assigned, explicitly and implicitly, to the notion of probability throughout his work on statistical mechanics was even more varied and confusing than in the case of Maxwell; see [8, 9].

hint in the 1871 paper that equilibration of the gas was anything but absolutely irreversible. And the reason is simple. Counting is not the same as expecting, and the notion of expectation that is caught up with the real notion of probability (to be further explored in the next section) is arguably not found in the original theorem. This leads to the following side remark.

In an 1955 review paper on statistical mechanics, Dirk ter Haar wrote:

> The first man to use a truly statistical approach was Boltzmann [in 1877, not 1871] and at that point kinetic theory changed into statistical mechanics even though it was another twenty years before Gibbs coined the expression.[8]

The point is well-taken, but in a sense Josiah Willard Gibbs missed a trick. The modern field of statistics is intrinsically bound up with the notion of probability. But if, in relation to common usage, a distinction can be drawn between statistics and probability, it arguably would emphasise the role counting plays in statistics. What Boltzmann did in 1871 had a statistical element in this sense. What he did in 1877 was to apply truly probabilistic considerations to the kinetic theory, and it might have been more appropriate if Gibbs had introduced the expression 'probabilistic mechanics' for what was common in the work he, Maxwell and Boltzmann did.[9]

(iii) The third point concerns the connection between this probabilistic element and the *uncertainty* involved in specifying the exact mechanical state of the gas, whether at the first instant of its evolution or at any other instant for that matter. A cornerstone of the dynamical assumptions going into the original kinetic theory of gases is that the gas is ultimately a deterministic Newtonian system. But our inability to know at any given time precisely what the state of the gas is – i.e., what the instantaneous positions and velocities of all the (huge number of) gas particles are –, together with limitations in available computational power, prevents us in practice from predicting with certainty what the subsequent behaviour of the gas will be. *Such uncertainty was the underpinning of the introduction of probabilities into physics.* Care must be taken in stating what is meant here. Boltzmann was of course aware of such uncertainty in 1871, but it was not enough to make him introduce truly probabilistic notions in the original H-theorem. Nor was it the motivation for his probabilistic turn in 1877, as we have seen. But without such uncertainty, the introduction of probabilities would seem to make no sense. And the subsequent development of our understanding of chaotic (but still deterministic) dynamical systems, such as weather systems, only seemed to make this connection between probability and uncertainty more intimate.

2 Quantum theory

Now it is widely thought that the advent of quantum theory led to a new and more fundamental role for probabilities in physics. The probabilistic nature of quantum predictions — say concerning the decay of a radioactive nucleus, or the direction a micro-particle will travel when emerging from a beam splitter —

[8]See [11].

[9]Note that the meaning of the terms probabilistic and statistical in [6] is quite different from that being used here.

is widely taken to reflect the existence of irreducibly random or stochastic processes occurring in Nature, and not human ignorance of precise micro-conditions which may be evolving deterministically. (But being quantum theory, nothing is certain: as we shall see in section 3.1 below there are well-defined deterministic interpretations of the theory, one of which, the so-called "hidden variable" interpretation, makes the role of probabilities look very much like that in classical statistical mechanics.) Such indeterminism is, however, *prima facie* at odds with fact that the most fundamental dynamical equation in quantum mechanics, the Schrödinger equation, is deterministic. To this day, there is no consensus as to whether the clash is illusory, or whether the probabilities only arise because there are conditions (themselves still subject to debate!) in which the Schrödinger equation breaks down. Be that as it may, the majority view today is almost certainly what it was around the birth of quantum mechanics, namely that in the kinds of micro-processes mentioned above, probabilities reflect at least *uncertainty about the future*, even when knowledge of the present state of affairs is complete.

I hinted above that there is a deterministic interpretation of quantum mechanics in which the nature of the probabilities is not like that in classical statistical mechanics. This is the so-called Everett interpretation, which was proposed in 1957 but attracted relatively little interest until recent years. If correct, the approach does something remarkable: it reconciles intrinsic randomness with determinism in an unprecedented and surprising way, and in doing so it casts considerable doubt over the traditional link between probability and uncertainty. The rest of this paper will be concerned with these matters. My purpose here is not so much to defend the Everett interpretation (though I will say why I think it deserves consideration) as to highlight, in an introductory way, its special place in the logical space of physical probabilities. But first, we need to look first at what probability itself means in physics generally.

3 Perchance

One of the giants of theoretical physics of the 20th century, Richard Feynman, gave the following answer to the question: what is probability?[10].

> We speak of probability only for observations that we contemplate being made in the future. *By the "probability" of a particular outcome of an observation we mean our estimate for the most likely fraction of a number of repeated observations that will yield particular outcome.* If we imagine repeating an observation – such as looking at a freshly tossed coin – N times, if we call N_A *our estimate* of the most likely number of our observations that will give some specified result A, say the result "heads", then by $P(A)$, the probability of observing A, we mean
>
> $$P(A) = \frac{N_A}{N}.$$

It is plain that Feynman avoids numerically conflating probabilities with frequencies of outcomes in trials involving repeated observations. Probabilities

[10]See [12], section 6-1.

just cannot be frequencies, because in the kinds of finite trials that occur in physics, frequencies fluctuate from one trial to another. (If you toss an unbiased coin 100 times, and repeat the process several times, do you expect to see exactly 50 heads come up every time?) Of course, as the number of observations (tosses) in each trial grows, you expect the fluctuations to die down. But the crucial word is "expect". Bernoulli's Law of Large Numbers shows only that when the number of repetitions increases without limit, one obtains a sequence of events in which the relative frequency of A lies as close as you like to the given value of $P(A)$, *with probability that is arbitrarily close to 1.*[11] In the light of this unavoidable impasse, we seem to be left with a rather weak notion of objective chances in physics. When the probability of any atomic nucleus of a given radioactive element to decay in half an hour is P, and we say by way of explanation that P is strictly in the nature of an "estimate of the most likely fraction *etc.*", are we not stepping out of the realm of the objective into that of the subjective? And how good a definition of probability is it that incorporates the word "likely"?

Probability is a curious thing. At a relatively informal level, we use the notion on an everyday basis, unreflectingly and fairly successfully, in making decisions about what to expect and how to organize our activities in the face of life's many uncertainties. The more formal, or systematic investigations into probability seem to have originated in the 1670s, in what appears to be a sort of miracle of suddenly converging ideas. But the resulting modern, relatively sophisticated notion of probability has proved hard to grasp. As the renowned Scottish philosopher David Hume wrote in 1748, "This [probabilistic] process of the thought or reasoning may seem trivial and obvious; but to those who consider it more narrowly, it may, perhaps, afford matter for curious speculation."[12]

Probability in its modern guise is Janus-faced. This apt expression is due to Ian Hacking, who in his influential 1975 study *The Emergence of Probability* provides a striking account of the seventeenth miracle just mentioned. Probability has what Hacking calls a statistical or aleatory side and an epistemological side. It has a connection with the tendency of certain processes to show stable long run frequencies on repeated trials, and it is also concerned with how the human agent forms degrees of belief or credibility on the basis of knowledge of such frequencies and other things, and hence how he or she decides to act. Perhaps "decision-theoretic" is more apt than "epistemological" for the second aspect; Hacking uses this term too.[13] At any rate, it is within this second aspect of probability, the feature of probability which makes it a guide to life, that such seemingly subjective notions as expectation and estimation finds their place and cannot be expunged.

The duality at the heart of probability has generated considerable discussion, not to say confusion, within philosophy. Philosophers can be very good at not understanding things; well, it is their job to express bewilderment when the subject deserves it, particularly when the subtleties are below the surface. And probability is certainly something worthy of philosophical analysis. One prominent contemporary philosopher, David Papineau, picks up the duality theme,

[11] For a detailed analysis of the possible pitfalls involved in interpreting the law of large numbers, see [9].

[12] See [1].

[13] See [13], Ch 2.

or something close to it, when he argues that in practice assigning numbers to probabilities involves two procedures:

> (1) *The Inferential Link.* We use frequencies to estimate probabilities. If we observe a frequency p for some type of result R in a finite sequence of trials of type T, then this is evidence that the probability of R in T is close to p. (2) *The Decision-Theoretical Link.* We base rational choices on our knowledge of objective probabilities. In any chancy situation, a rational agent will consider the difference that alternative actions would make to the objective probabilities of desired results, and then opt for that action which maximizes objective expected utility.[14]

Papineau speaks for many (but not all) philosophers when he claims that the mystery of probability resides in the fact that there are simply no good justifications that anyone has come up with for either of these links. Some readers may find this implausible, but Papineau has good arguments as to why the situation as he has defined it is, if not desperate, then unsettling.[15] The matter deserves much discussion, far more than can be given here. But it is worth raising a concern about the way Papineau and others have defined the dualistic problem, and to do this let's return to Feynman.

In the quotation at the start of this section, the fact that Feynman does not talk of probabilities in physics as being associated with objective chances or elements of reality may have been intentional. Granted, it is likely that he thought that all rational agents would come up with the same probabilities (estimates of likelihood) in the light of the evidence, in physics at least, but this notion of intersubjectivity is not what Papineau means when he talks about chance. Papineau has in mind a particular kind of fact of the matter out there in the world, something that would exist in the absence of all sentient beings capable of inductive reasoning. The existence of such a thing can, and has been, questioned. Yes, there are natural phenomena which can be described as frequencies of outcomes within certain kinds of repetitive trials, which may be stable enough to be interesting, but as we have seen these unfortunately are not probabilities. If there are indeed no objective chances in the sense of Papineau and like-minded philosophers such as David Lewis, even in quantum mechanics, then the Janus-faced nature of probability may seem less troubling.[16]

For in this case there is only one "link", not two, and that is from past or present evidence to estimations of likelihood, where the latter is directly understood in the decision-theoretic sense. Let me spell this out a little. First, the "evidence" need not be solely in the form of recorded frequencies. Given an untested coin, you would be justified in estimating the probability of heads on the basis of the degree of symmetry of the coin[17], on any knowledge of the coin's

[14]See [14].

[15]It is not just present-day philosophers who are baffled by probabilistic reasoning. Recall the quotation from Hume at the start of this paper.

[16]The subjective interpretation of probability has roots in the ideas of Jacob Bernoulli and Pierre-Simon Laplace, and was further developed in the twentieth century principally by F. P. Ramsey, B. de Finetti, L. J. Savage and R. Jeffrey. de Finetti's Exchangeability, or Representation Theorem is arguably the closest thing in this approach to an explanation as to why probabilities in physics *seem* objective. A careful critical analysis of how subjective probabilities are, or can be, used in statistical mechanics is found in [9].

[17]This procedure is closely linked to the use of the Principle of Indifference (or the Principle of Insufficient Reason) in the subjective interpretation of probability; see again [9].

construction, and so on. (Typically though the knowledge of frequencies over large enough trials will trump such considerations, when it becomes available.) Second, what a numerical probability means is just a (possibly intersubjective) degree of belief, a guide to action — it being taken for granted that ordinarily such action is designed to maximize expected utility. The source of Papineau's bewilderment as to why the existence of an objective chance in the world should have an influence on our future decisions, such as it is, fades away in this scenario. But we are still left with the link between evidence and estimations of likelihood; is there a lingering puzzle here?

Yes, in the sense that we face a variant of the age-old "problem of induction", posed with particular force by David Hume in his 1748 treatise *An Enquiry Concerning Human Understanding*, the reverberations of which are still clearly felt in epistemology. What justification is there for believing that natural regularities, strict or partial, observed in the past will persist into the future? What, in short, is the justification of inductive reasoning, without which life would be flatly impossible?

But one could say *No*, if one concludes, as I am inclined to, that the problem of induction is a pseudo-problem (even if not obviously so), that inductive reasoning is, rather like language, an instinct bestowed on us by evolution, in this case for reasons of computational economy, and that what would require justification is any deviation from it.[18]

Recall, finally, the question raised earlier about Feynman's definition of probability: should it contain a term such as "likely"? Is there not a whiff of circularity in the air? If there is no such thing as objective chance out there in the world, then probability is a number whose ultimate significance is of a decision-theoretic nature. The fact that it is normally constrained by objective frequencies does not undermine this point. The probability of a given event can be related to the degree to which an agent is prepared to bet on the prospect of that event occurring. If there is nothing circular about this somewhat crude characterization, then there is nothing circular about the statement that probability has to do with estimation of likelihood, because it means essentially the same thing.

3.1 Perchance to dream

You have been attending a lecture by the famous but obscure Professor X on probability theory, and finding it all rather confusing. Later that night, in a restless dream, you are watching Prof X give a tutorial to a group of students. He repeatedly tosses what appears to be a biased coin, and in this way is trying to explain the subtlety of the connection between probabilities and frequencies. Suddenly you are aware that every time Prof X tosses the coin, *both* outcomes, heads and tails, actually happen — but in a fashion hidden to him. At each toss, the world somehow divides, amoeba-like, into two causally disconnected copies, each sharing the same history up to the moment of the toss, but thereafter different. You can see in the dream in a God-like fashion that there are two

[18] de Finetti's representation theorem within the subjective interpretation of probability (see footnote 15) is sometimes said to provide a solution of the Humean problem of induction, but it is not, nor could it be, what philosophers who take the problem seriously are looking for. de Finetti himself recognized this, as well as the futility of the search for a "logical" solution; see in this connection [15].

versions of Prof X after each toss, sharing the same memories but one observing heads and the other tails. Each is unaware of the other's world ... and as the tosses continue the worlds go on dividing and dividing, in a process reminiscent of the branching structure of a tree.

The next day, relieved in the knowledge that there is at most one Prof X, you recall the moment in the dream when he claimed that the probability of heads for the biased coin was around 0.7; it was before you were aware of the bizarre consequences of tossing the coin. You now find yourself idly wondering *what Prof X could have meant.*

After all, doesn't the notion of probability only make sense when there is uncertainty about the future and then when only one of the various possibilities is realized? Yet surely there was something rational about Prof X assigning a probability to heads on the basis of the past frequencies (or the memories of those frequencies). Was Prof X coherent, in his own terms, only because he was unaware of the world-branching caused by tossing the coin? Odd, then, that what makes the action rational is confusion. (But it would also be odd, surely, if Prof X suddenly renounced all probabilistic inferences, and fell into predictive paralysis, on learning of the existence of invisible parallel worlds that can have no causal influence on him.) From the God's-eye perspective, everything that could happen was happening, and there was no uncertainty about the outcome of the tosses. Was Prof X not talking then about *genuine* probabilities at all? Or if he was, how could he have been objectively right to assign a probability of anything other than 0.5, given that both possible outcomes occurred?

These questions may appear whimsical, silly perhaps. But within certain parts of the foundations of physics community today, questions like these are being taken very seriously. The reason is of course that there is growing interest amongst physicists and philosophers in the Everett interpretation of quantum mechanics, sometimes referred to as the many-worlds interpretation. If this picture of quantum reality is even roughly correct, questions about the meaning of probability in physics become acute; indeed it is somewhat surprising that it has taken so long for them to be addressed with real care.

4 Everett quantum theory

To repeat, there is still no consensus as to how to understand quantum mechanics. Rival interpretations have been at war pretty much since the birth of the theory in the mid-1920s; one only has to remember the great debate between Niels Bohr and Albert Einstein that reached a peak in 1935. Everett's 1957 paper[19] appeared two years after Einstein's death. It is a great pity Einstein did not live to see Everett's work. Everett's proposal uniquely incorporated the three elements Einstein demanded of any reasonable physical theory, and in particular of an interpretation of quantum theory. These are that the theory be: *realist*, namely concerned at its most fundamental level with objects and processes that exist independently of the observer; *local*, incorporating no instantaneous action-at-a-distance, and finally, *deterministic*. The last desideratum may seem perverse in the case of quantum mechanics, which, again, is widely thought to undermine determinism, and it was perhaps the one Einstein was least sure about. But it should be borne in mind that the Everett picture is

[19] See [16].

not the only interpretation of quantum mechanics that is deterministic: members of an important class of "hidden-variable" theories are deterministic, the most famous case being the de Broglie-Bohm, or "pilot-wave" interpretation, but note that such theories are non-local.[20] I think it is fair to say that interest in the Everett interpretation has grown as the limitations of the rival views have become more apparent. In particular, it is, unlike the "orthodox" view (the co-called Copenhagen interpretation, a loose amalgam of ideas that are largely inspired by the thinking of Niels Bohr, and undergoing something of a renaissance today within the quantum information community) consistent with the program of applying quantum theory to the whole cosmos, though again it is not alone in this respect.

The Everett theory makes the dream, or possibly nightmare, outlined above a reality. For *every* quantum process involving a measurement the outcome of which appears to be probabilistic, all the possible outcomes are being realised in branching "worlds" which are invisible, one to another — the branching process itself taking place according to the deterministic time-dependent Schrödinger equation. The scare quotes in "worlds" are due to an important feature of the theory: the "worlds" are not *entirely* autonomous one from another (so they cannot really be counted, for instance), and the emergence of familiar macroscopic bodies within them is only of an approximate nature. Better put, "worlds" in the Everett picture have an irreducible element of vagueness associated with them, the kind of vagueness that is widespread in the physics of higher level systems and which couldn't be otherwise. "Worlds", and even observers like us within them, are not fundamental entities in the theory (for which vagueness is not permitted), but are what is sometimes called "emergent" or "effective" notions. Getting the the details right concerning what they are and how they emerge from the basic element of reality — the evolving wavefunction of the universe — has taken the work of many theoreticians, both physicists and philosophers, following Everett. (Much of this work centres on the process of "decoherence" in quantum mechanics, the details of which Everett only dimly anticipated in 1957.) It can be fairly said that the theory today saves the appearances.[21]

What for me is so striking about the theory, once the initial shock of contemplating a universe containing untold versions of you and me wears off (to the extent it does) is this. First, of all the main interpretations of quantum mechanics, the Everettian picture is the most literal, the most faithful to the standard formalism of the theory; it introduces no hidden variables, no modifications to the Schrödinger equation (so no non-local "collapse of the wavefunction"). Second, the theory provides a comprehensible mechanical account of how branching can occur which, counterintuitively, does not violate the conservation of energy, nor involve discontinuous changes over time. Third, the theory manages to square the circle, in the sense that it has a deterministic underpinning but is consistent with real stochasticity, or indeterminism, as observed over time along certain branches in the universe. But this second remark must be understood in the light of the point made in the previous paragraph, namely that "worlds", observers and hence probabilities, are only emergent entities in the theory, whereas

[20]For a classic review of the de Broglie-Bohm theory, see [17]. A more recent proposal for the basis of a putatively local, deterministic quantum theory is found in [18].

[21]A recent authoritative introduction to the Everett interpretation is found in [19]. The definitive collection of specialist essays, both favourable and critical, on the interpretation is found in [20].

determinism lies at the heart of the fundamental dynamics.

4.1 Probability in the Everett picture

In two influential papers in the 1990s, David Papineau[22] argued against a then widespread and still prominent objection to the Everettian picture of quantum reality, namely that it precludes any satisfactory notion of probability. Doubts had been circulating of the kind raised in the dream story at the start of section 3 above. One of the salient differences between that scenario and the Everettian multiverse is that observers in the latter are, unlike Prof X, aware, or they can be aware, that everything that can happen (within the bounds of quantum theory) does happen.

Are such agents precluded from using probabilistic reasoning of the normal kind? Papineau argued not, and used what might be called the pessimistic stratagem. An important part of his argument concerned the Inferential Link and the Decision-Theoretical Link; readers will recall his scepticism as regards any satisfactory justification of these links. He was to turn this difficulty on its head.

> As to a justification for these stipulations, the many minds theory can simply retort that it provides as good a justification as conventional thought does for treating its probabilities similarly — namely, no good justification at all .... It is true that the many minds view requires us to think about probabilities in a way we are quite unused to. Normally we think that just one of a set of chancy outcomes will occur, with the probabilities therefore indicating the outcomes' differing prospects of becoming actual. On the many minds view, by contrast, all the outcomes will definitely occur, on some branch of reality, and the probabilities therefore need to be read as attaching weights to these different branches. But it seems to me that this contrast is a 'dangler', which makes no difference to the rest of our thinking about probability. It does not disrupt either of the 'operational links' connecting probabilistic to non-probabilistic facts. And it does not contradict the theories which underlie these links, since we have no such theories.[23]

Indeed it is a common refrain today on the part of Everettians that at worst, the many-worlds theory is no more problematic in its treatment of probability than conventional theory, or other interpretations of quantum theory.

I think the sentiment is right, but I doubt if the conventional take on probability need be as bleak as the pessimistic strategem makes out. As I argued in section 2, a one-link view of probabilistic operations removes much of the mystery, and seems to me to fit naturally into the Everettian scenario as well.[24] At any rate, Papineau was right to question orthodox thinking when he wrote:

[22] See [2], [14].

[23] See [14]. Note that in referring to the "many minds theory" rather than the "many worlds theory", Papineau was addressing a variant of the Everettian view due to Michael Lockwood in [21]. Papineau's remarks concerning probability are nonetheless pertinent to the standard Everettian picture; indeed Papineau (*ibid.*) himself criticised the idea that quantum multiplicity concerns only minds and not worlds.

[24] A careful analysis along these lines is found in [22].

> I know it flies in the face of common sense to hold that all chancy outcomes occur. Still, ... this supposedly obvious truth floats free of anything else we do or think about probability. So ... it seems that nothing else argues against the many minds view except unfamiliarity.[25]

4.2 Uncertainty?

Recall the case of the beam splitter briefly mentioned in section 2 above. If a collimated beam of neutrons is directed at a certain angle into a transparent crystal block made of pure silicon, the neutrons will emerge from the crystal in *two* beams coming out at different angles – call them beam A and beam B. If the incoming beam is attenuated to the extent that only one neutron goes through the crystal at a time, we find (as quantum theory predicts) that when a detector is put into each outgoing beam, each neutron will be found with equal probability in one or other of these beams.[26] But one of the key things that makes quantum mechanics different from its classical counterpart is that before it reaches the detector, the neutron is described in the formalism as being in *both* beams at once: it is in a "superposed" state. Indeed, if instead of detectors being put in the path of the two beams, a way is found of bending the beams back together, they will interfere just as if each neutron were a wave split into two parts which are then made to recombine, producing a characteristic wave-like interference pattern which would never appear in the classical description of the behaviour of a beam of particles. Such neutron interferometry experiments have been performed over several decades, in a variety of spectacular forms, principally in Austria and the United States.

In the case where the detectors are in place behind the beam splitter, then according to the Everett interpretation, the result will be a superposition of two "worlds", one in which the neutron is detected in beam A, the other in which it is detected in beam B. At the start of the experiment, if the observer not only knows about quantum mechanics but accepts the Everett picture, then she believes that both outcomes will be realized. But, the argument goes, she will still act so as to maximise the expected utility based on the quantum probabilities. She is effectively using probabilities while at the same time being convinced that all outcomes that can occur will occur. Furthermore, this will be so when the state of the neutron beam coming into the beam splitter, and the effect of the passage through the silicon crystal, is known *in its entirety*[27]. As was

[25]See [14]. Since Papineau's early work, much has been written by philosophers on the nature of probability in the Everett scenario. I have in mind principally the work of Simon Saunders, David Deutsch, David Wallace, Hilary Greaves, Wayne Myrvold and David Papineau himself; for a review of this work see [23, 24, 19] and the relevant essays and discussion transcript in sections 3 and 4 of [20].

Some philosophers have proposed an important new way of reinforcing Papineau's 1995 suggestion that the conceptual role of probability is if anything clearer in the Everett interpretation than in conventional scenarios! The argument is couched in terms of the two-link view of probability outlined above; indeed it could be seen as an attempt to justify this view. This new argument is based on the Deutsch-Wallace theorem, which cleverly uses a variant of the Principle of Indifference (alluded to above) within quantum mechanics in order to derive the standard Born Rule for probabilities. For details and further references, see [19].

[26]The probability for detection in each of the beams A and B will depend on the exact form of the wavefunction describing the incoming beam, in accordance with the Born Rule; but in typical cases it will be approximately 0.5.

[27]It turns out to be unnecessary to know the precise configuration of all the silicon atoms in

mentioned in section 2, there are situations in quantum theory when the probabilities need not reflect any uncertainty in the initial conditions on the part of the observer. The question now is whether any uncertainty *at all* is involved, within the Everettian picture.

If there is uncertainty about the future in the beam-splitting example, it is far from obvious what it is about. There is nothing in the early approach that Papineau took to justifying the use of probabilities in this kind of scenario which suggests that subjective uncertainty plays a role of any significance, but the issue has since become somewhat contentious. One way of providing a justification for grounding the Everettian probabilities (or rather the usual probabilities but understood in the Everettian picture) in uncertainty is by way of adopting a temporally non-local notion of personhood, or personal identity.[28] But the price to be paid, whether this approach is seen as metaphysical or merely providing a "semantics" of uncertainty, is perhaps to make Everettian probabilities obscure to all but a rather special cohort of the philosophically initiated. And more to the point, it is unclear that such an uncertainty-based view of probability is really needed in this context.[29] I myself doubt it. One might then wonder whether it is really genuine probability that is being applied in the Everettian picture. But whatever it is, it is operationally identical to standard probability, both in the two-link and one-link views of probability outlined in section 2 above, which may be all that really matters.

The Everett interpretation of quantum mechanics may, of course, prove to be wrong. Although the ontological extravagance of the many worlds no doubt puts many people off, it is not clear that the situation is any better in the rival de Broglie-Bohm pilot-wave theory[30], and anyway the extravagance is but little compared to the pictures that are emerging from quantum cosmology (more particularly from a combination of the inflationary model of the early universe and string theory). We must not forget too how extravagant the size of the universe must have seemed to early readers of Copernicus. No, the faults of the Everett interpretation, if any, probably lie elsewhere. But right or wrong, it invites us to re-examine the nature of probability, that "curious and sublime" concept, and in doing so raises doubts about the apparently rock-solid connection between it and uncertainty. No doubt David Hume would have found the latest twist amusing.

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the crystal (to the extent that it is well-defined in the theory); the effect of the beam splitter can be modelled in the single-body Schrödinger equation governing the neutron by a periodic potential associated with passage through the atomic planes of the crystal.

[28] This position has been defended by Saunders in [20], Chapter 6, section 3. Suppose Alice is the observer in a quantum experiment involving various possible outcomes. Then, argues Saunders, "there are *many* Alices present, atom-for-atom duplicates up to [branching], each behaving in exactly the same way and saying just the same words. If that is the right metaphysical picture for [Everettian quantum mechanics], then Alice *should* be uncertain after all — *each* Alice should be uncertain — for each as of [branching] does not (and as a matter of principle, cannot) know which of these branching persons she is." (p. 192, *ibid.*).

[29] See [25].

[30] See [26], and the debate in [20], pp. 476-517.

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