Quantum mechanics is a fundamentally probabilistic theory (at least so far as the empirical predictions are concerned). It follows that, if one wants to properly understand quantum mechanics, it is essential to clearly understand the meaning of probability statements. The interpretation of probability has excited nearly as much philosophical controversy as the interpretation of quantum mechanics. 20th century physicists have mostly adopted a frequentist conception. In this paper it is argued that we ought, instead, to adopt a logical or Bayesian conception. The paper includes a comparison of the orthodox and Bayesian theories of statistical inference. It concludes with a few remarks concerning the implications for the concept of physical reality.
1. INTRODUCTION

This paper is about probability. It was originally stimulated by some conversations with Chris Fuchs concerning the foundations of quantum mechanics. These conversations had a major impact on my thinking; they made me wonder if there might, after all, be something to be said for the Copenhagen interpretation.

I did at first doubt whether it is appropriate to dedicate these remarks to the memory of Jim Cushing, who expended much energy on the task of challenging the Copenhagen hegemony. But after reflection I decided that it is very appropriate. Jim has spent a great deal of time and effort studying Copenhagen ideas; far more than I have myself. I feel that he would have to be interested at least in the question. What he would think of my (tentative) answer is, unfortunately, impossible now to know. But what I do know is that there is no one whose opinion I would more eagerly have sought.

Jim was, before he was anything else, a friend of reason. He had no objection to someone who adopts Copenhagen assumptions provisionally, in a spirit of free enquiry, to see where they lead. His objection was to dogmatic Copenhagenism: the insistence that Bohr and his colleagues settled the question, completely, once and for all, and that there is nothing more of any interest to be said on the subject.

Nowadays this attitude would be quite unusual. Even those who are sympathetic to Copenhagen ideas would mostly agree that there are many remaining obscurities. There are, besides, numerous other interpretations on offer, all well represented in the pages of the leading journals. However, it was not always so. The climate which existed 30 years ago is well illustrated by the editorial comment in which the editor of Reviews of Modern Physics defends his decision to publish Ballentine.

It can be seen from this comment that there was then a substantial body of opinion which held that Einstein’s ideas regarding the interpretation of quantum mechanics should not be discussed in the scientific literature. It is this refusal even to consider the question which Bell aptly describes as Copenhagen “complacency”.

Fortunately those days are now over. We seem to have finally got back to the situation which existed in the 1920’s, where the Copenhagen case has to be argued. We owe this happy state of affairs to the efforts of Jim Cushing and others who have struggled to convince the physics community that Bohr’s word should not be taken as final. I think that even those who favour the Copenhagen approach owe him a debt. An atmosphere of bland orthodoxy, in which stimulating discussion is not to be had, is unlikely to foster new ideas.

Fuchs (p. 173) advocates the Copenhagen interpretation in the following terms:

“I have this ‘madly optimistic’ (Mermin called it) feeling that Bohrian-Paulian ideas will lead us to the next stage of physics. That is, that thinking about quantum foundations from their point of view will be the beginning of a new path, not the end of an old one.”

Proposed like that, not as a piece of dogmatism, but as an invitation to serious thought, the Copenhagen interpretation, to my mind, suddenly becomes interesting.

This does not mean that I find the Copenhagen interpretation satisfactory, as it stands now. Bell (pp. 173–4) argues that the Copenhagen interpretation is “unprofessionally vague and ambiguous”. I think he is right. I also think he is right to complain that quantum mechanics, when interpreted in traditional Copenhagen terms, seems to be “exclusively concerned with ‘results of measurement’ and [seems to have] nothing to say about anything else”. I share Bell’s conviction that the aim of physics is to understand nature, and that counting detector “clicks” is not
intrinsically any more interesting than counting beans. If prediction and control were my aim in life I would have become an engineer, not a physicist.

Fuchs has not caused me to see clarity where I formerly saw obscurity, or realism where I formerly saw positivism. What he has done is to make me wonder if it might be possible to construct a greatly improved version of the Copenhagen interpretation, to which these objections would not apply. If I am asked to accept Bohr as the authoritative voice of final truth, then I cannot assent. But if his writings are approached in a more flexible spirit, as a source of insights which are not the less seminal for being obscure, they suggest some interesting questions. I do not know if this line of thought will be fruitful. But I feel it is worth pursuing. I also feel that Jim Cushing would consider it worth pursuing.

I will make a few comments concerning the question of realism in Sections 4 and 12. However in this paper I will mainly be concerned with probability. Fuchs and his co-workers have made a number of significant conceptual innovations (they are, I believe, the first members of the Copenhagen tendency since the 1930’s who, not content merely to reiterate pieces of received orthodoxy, seriously try to advance the theory at a basic conceptual level). One such innovation is their analysis of probability, and its relevance to the interpretation of quantum mechanics (also see important work by Hardy [10, 11], Pitowsky [12] and Perey [13]).

Quantum mechanics is a fundamentally probabilistic theory. Of course, probability theory plays an essential role in classical physics too. However, in classical physics the uncertainties can, in principle, be made arbitrarily small. In quantum physics they are ineluctable\(^1\). So it is not unreasonable to suggest that, to properly understand quantum mechanics, we need first to straighten out our ideas regarding probability, and its physical significance.

Whereas the interpretation of quantum mechanics has only been puzzling us for \(\sim 75\) years, the interpretation of probability has been doing so for more than 300 years [10, 17]. Poincaré [18] (p. 186) described probability as “an obscure instinct”. In the century that has elapsed since then philosophers have worked hard to lessen the obscurity. However, the result has not been to arrive at any consensus. Instead, we have a number of competing schools (for an overview see Gillies [19], von Plato [20], Sklar [21, 22] and Guttman [23]).

The majority of 20\(^{th}\) century physicists subscribed to a frequency interpretation of probability. But in the 19\(^{th}\) century a very different view was widely held. It is exemplified by the following remark of Maxwell’s:

> They say that Understanding ought to work by the rules of right reason. These rules are, or ought to be, contained in Logic; but the actual science of logic is conversant at present only with things either certain, impossible, or entirely doubtful, none of which (fortunately) we have to reason on. Therefore the true logic for this world is the calculus of Probabilities, which takes account of the magnitude of the probability which is, or ought to be, in a reasonable man’s mind. [J. Clerk Maxwell, quoted Jeffreys [24], p.1]

Maxwell here espouses what I am going to call an epistemic view of probability. As Maxwell sees it a probability statement has a normative, or logical significance. It does not directly assert a fact about the way things are in the world. Instead it regulates our expectations concerning the world. Under the influence of Bayes and Laplace [25] this way of looking at probability was common for a large part of the 19\(^{th}\) century.

\(^1\)Or so it now seems. This statement might need to be modified if Valentini’s [14, 15] ideas were empirically confirmed.
Suppose that Maxwell’s demon is interested in the question, whether a (classical) molecule is going to hit its shutter during the interval \((t, t + \Delta t)\) at some specified time \(t\) in the future. Suppose that to begin with the demon only knows the temperature, pressure and volume of the gas. On that basis the demon calculates that the probability = (say) 0.001. Suppose, however, that the demon then acquires detailed information regarding the positions and velocities of all the gas molecules at some time \(< t\); and suppose that on the basis of that new information, and Newtonian mechanics, it calculates that a molecule is certain to arrive at its shutter during the interval \((t, t + \Delta t)\). Then the probability changes discontinuously from 0.001 to 1 (the probability distribution might be said to “collapse”). But this discontinuous change in the probability does not reflect any change in the state of the gas. All that has changed is the state of the demon’s knowledge. That is what is meant by calling the probability epistemic.

However, towards the end of Maxwell’s life the epistemic view began to go out of fashion, and throughout the 20th century it was very unfashionable indeed. In physics it was largely replaced with the frequentist conception, according to which a probability can be identified with a relative frequency in some suitably defined ensemble. The attraction of this view is that, by removing all reference to the knowledge and/or beliefs of some cognitive agent (human or otherwise), it promises to make the concept of probability purely objective.

Fuchs et al. consider that this was a mistake, and that we need to go back to an epistemic conception. When I first became acquainted with their ideas I resisted this suggestion. However, the more I have thought about it, the more I have become convinced that they have to be right. This paper is the fruit of those cogitations.

Fuchs et al. subscribe to the epistemic theory proposed by de Finetti, (also see Ramsey). I should say that I do not entirely agree with them about that. Although I fully acknowledge the depth and importance of de Finetti’s insights, it seems to me that he misses some essential points. My feeling is that a completely satisfactory theoretical account has yet to be formulated. However, that has no bearing on my argument here. In this paper I am simply concerned to argue that the epistemic conception is, in one form or another, unavoidable.

Much of the paper concerns the theory of statistical inference, which has a crucial bearing on the question. An essential part of the frequentist position is that probabilities are, not only objective, but also in some sense observable. If the proposal was instead that probabilities, though purely objective, are empirically unknowable—if, in other words, probabilities were conceived as hidden variables—then the frequentist idea would lose most, if not all, of its attractiveness.

The method of inference originally proposed by Bayes and Laplace is unsatisfactory from the frequentist point of view because the inferred probability distribution depends, not only on the empirical data, but also on a prior probabilistic assumption. Suppose, for example, that a coin comes up heads on 500 out of 1000 successive tosses. Then, with the appropriate choice of prior assumption, the inferred distribution can be strongly concentrated in the vicinity of any probability in the interval \([0, 1]\). This is not a problem if one looks at it from the epistemic point of view that must be fair as a general statement. Nevertheless, the epistemic view has continued to engage the interest of a very active minority. 20th century advocates include Keynes, Jeffreys, Carnap, Lewis, de Finetti, Savage, Bernardo and Smith, Jaynes, Howson and Urbach, and Earman (this list is not intended to be complete). The list might be sub-divided into those who think that probability involves a new kind of non-deductive logic and those who take a Bayesian approach. However, this classification is somewhat arbitrary. Jeffreys, in particular, straddles the two categories. Jaynes should be singled out for special mention because he is primarily concerned with applications to physics.
view which Maxwell describes in the passage I quoted above. However, from a frequentist point of view it is a very serious problem. For the frequentist programme to work a statistical inference needs to be something like a measurement: it must be possible to read the inferred distribution directly off from the data, without the assistance of any prior assumption.

In the opening decades of the 20th century Fisher, Neyman and Pearson and others accordingly developed a new statistical methodology, in conscious opposition to the ideas of Bayes and Laplace. This is the approach based on confidence intervals and hypothesis tests which, for want of a better term, I am going to call the orthodox methodology. The attraction of the orthodox methodology is that, unlike the Bayesian methodology, it seems to make statistics purely empirical, and purely objective. One of the main conclusions of this paper is that it fails in that purpose. Not only is it, as de Finetti (vol. 2, p. 245) says, *ad hoc*. It is no less dependent on prior probabilistic assumptions than the Bayesian methodology.

Hume famously argued that one cannot validly infer an “ought” from an “is”. A similar principle applies to probability statements: one cannot validly infer a “probable” from an “is”. This principle is closely related to the conclusion to Hume’s argument for inductive scepticism. It means that probability judgments are not purely empirical. It also means that a probability statement cannot be identified with a fact about the world, as it exists independently of us.

It was easy for Maxwell to accept an epistemic interpretation of probability because he was thinking in terms of a world populated by classical atoms and fields, whose objective reality was not in doubt. But if one translates the idea to a quantum mechanical context, and suggests that the quantum state must be interpreted epistemically, then the concept becomes very disturbing. It certainly becomes disturbing to me (although I find Fuchs’s ideas stimulating, they also worry me). On the face of it, taking an epistemic view of the state vector amounts to giving up on the idea of physical reality altogether.

I remain very uncertain about this. An epistemic interpretation of the state vector is, it seems to me, impossible to reconcile with Einsteinian realism: the proposition that “the programmatic aim of all physics is the complete description of any (individual) real situation (as it supposedly exists irrespective of any act of observation or substantiation)” (p. 667). However, I feel it may be consistent with a much more subtle and interesting kind of realism, which is obscurely intimated in the writings of Bohr, but which has yet to be properly articulated. I will touch on this in Section 12.

The plan of the paper is as follows. Sections 2 and 3 concern the frequency interpretation. Section 4 concerns the idea of a propensity. Sections 5–8 are the core of the paper and are concerned with the theory of statistical inference. Section 9 (also Section 3) concerns the idea of impossibility FAPP (“for all practical purposes”). Section 10 contains some final criticisms of the frequentist view, based on the argument in Sections 5–9. Section 11 concerns the epistemic view. Section 12 concerns the question of physical realism.

2. Frequentism: Infinite Ensembles

Physicists naturally tend to favour a frequency interpretation of probability. According to this conception the proposition “the probability of this coin coming..."
up heads = 1/2” is a straightforward factual assertion about the number of heads in a long or infinite sequence of tosses. The idea is attractive because it promises to give probability statements a purely objective significance. It is originally due to Venn [37]. The best known 20th century proponents are von Mises [38, 39], Reichenbach [40], Popper [41] (in the first part of his career) and van Fraassen [42].

Hájek [50] (endnote 2) says that among philosophers frequentism is now largely confined to the “closet”. But he also remarks that it continues to pervade much scientific thinking on the subject. It is not difficult to understand why. Once acknowledge that frequentism is untenable, and one is forced to re-assess some of the most basic assumptions of physical theory. Physicists have more of an investment.

If one wants to maintain that there is an effective logical equivalence between a statement about the probability of a coin coming up heads, and a statement about the actual frequency of heads in a sequence of tosses, then it is clearly essential that the sequence be infinite. There is a problem with this, however: for it is (to say the least) doubtful whether a coin physically could be tossed an infinite number of times. Leaving aside the problems of corrosion, and mechanical wear, there is the problem that we expect the sun eventually to become a red giant. Supposing the coin to survive that vicissitude one then has the problem that the lifetime of the proton, not to mention the universe, may be finite.

von Mises’s approach is to define the probability counter-factually, as the limiting relative frequency which would be obtained if the coin were tossed an infinite number of times (see, for example, von Mises [38], p.15). This might be acceptable if the aim was only to provide a convenient way to think about probabilities (though I would question whether it really is all that convenient). But, as Jeffrey [52] says, it is clearly unacceptable if the aim is to identify a probability with an actually existent physical quantity, out there in the world. If probabilities are to objectively exist, then it is essential that the sequences in terms of which they are defined should objectively exist.

The universe may have finite 4-volume. In that case, on an infinite frequentist definition, there would not be any probabilities. At least, there would not be any probabilities of directly observable events, having non-zero spatio-temporal extension.

However, that is not the only difficulty with the infinite frequentist idea. Suppose we grant, for the sake of argument, that the universe has infinite 4-volume. A theory which makes the probabilities of events now, here in the Milky Way, critically dependent on events more than $10^{10^{10}}$ years in the future, or more than $10^{10^{10}}$ light years distant, cannot be considered empirically relevant.

Let $S$ be some suitably enormous, but still finite region of space-time containing ourselves. Let $E$ be the finite ensemble of $^{226}$Ra nuclei whose world-lines intersect $S$, and let $E_\infty$ be the ensemble consisting of all $^{226}$Ra nuclei in the universe (assumed infinite). Let $f$ be the proportion of nuclei in $E$ which decay in proper time 1602 years, and let $f_\infty$ be the proportion in $E_\infty$ which do so (as defined by some appropriate limiting procedure). Suppose it should happen that $f = 1/2$, but $f_\infty = 1/10$. Then, on an infinite frequentist definition, we are obliged to say that the true decay-probability is 1/10. It would, however, seem more natural to say

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4 The equivalence cannot be strict even in the case of infinite sequences. It is true that if the probability of heads = 0.5 then, by the strong law of large numbers [54] (vol. 1, pp. 203–4), the set of sequences in which the limiting relative frequency of heads either does not exist, or exists but $\neq 0.5$, has measure zero (relative to the product measure on the space of infinite sequences). But that is not equivalent to the proposition that such sequences are impossible.
that the half-life of $^{226}$Ra is 1602 years in our part of the universe, but that it takes different values elsewhere.

It is natural to ask if the parameters defining the standard model depend on spatio-temporal position. If so one would expect half-lives typically to depend on spatio-temporal position. But on an infinite frequentist definition that suggestion is not even meaningful.

Suppose that a roulette wheel in London is spun 37,000 times, and each number comes up approximately 1,000 times. Clearly, this has no implications for the fairness of a different, completely unrelated roulette wheel in Rio de Janeiro. By the same token, it has no implications for the fairness of the roulette wheel in London in 1000 years time (supposing it still to exist).

Similarly, a determination of the half-life of $^{226}$Ra on the Earth now has, in itself, without extra assumptions, no implications for what the half-life of $^{226}$Ra is at a point $10^{10^{100}}$ light-years distant, or will be at a time $10^{10^{100}}$ years in the future.

Measuring the relative frequency in a finite ensemble in London in the year 2004 AD does, in itself, without additional assumptions, tell one nothing about the limiting relative frequency in some (purely hypothetical, empirically inaccessible) embedding ensemble extending over an infinitely large spatio-temporal region. Similarly, if per impossibile one knew the “true” probability in the sense of an infinite frequentist definition, this would tell one nothing about the relative frequencies to be expected in the finite ensembles of actual interest. This is, in essence, just the point of Hume’s argument for inductive scepticism [47, 48].

Probabilities in the sense of an infinite frequentist definition may perhaps exist. But they have nothing to do with the probabilities which we infer from our observations, and on which we base our practical decisions. They are empirically and practically irrelevant.

Suppose we discovered that the universe does have finite 4-volume. This would not affect practical probabilistic reasoning in any way. Tabulations of nuclear half-lives would not suddenly be rendered meaningless.

3. Frequentism: Finite Ensembles

If one wants to give a frequentist definition of the probabilities which occur in empirical reasoning, then the definition had better be in terms of finite ensembles.

The shift to finite ensembles necessitates a significant weakening of the frequentist position. The proposition “the probability of heads = 0.5” is consistent with any sequence of outcomes in a run of (say) 1000 tosses. It is admittedly very unlikely that the coin will come up heads on each of 1000 tosses. But the probability of this happening is $> 0$.

The usual response to this difficulty is to argue that very small probabilities count as practical impossibilities. On those grounds it is suggested that the proposition “the probability of heads = 0.5”, though not strictly equivalent, is for all practical purposes equivalent to the proposition “the relative frequency of heads will be extremely close to 0.5 in a sufficiently long sequence of independent tosses”.

In the philosophical literature an even weaker position is often advocated. According to Popper [11] (also see Gillies [19] [53] and, for a critical discussion, Howson and Urbach [35]) the proposition “the probability of heads = 0.5” is not confirmed by the outcome of any finite sequence of tosses. It is, however, practically falsified if the sequence is sufficiently long, and if the relative frequency of heads is sufficiently different from 0.5. Gillies [19] (p. 147) argues that this coincides with the principle on which statistical hypothesis testing is based.

\(^5\)As I remarked in footnote 4 it is logically consistent in the infinite case also. However, in the infinite case there is zero probability of obtaining a limiting relative frequency $\neq 0.5$.\n
There is a problem with this line of thought. Even a probability of $2^{-1000}$, though it can be neglected for many practical purposes, cannot be neglected for all.

Suppose a coin comes up heads on each of 24 successive tosses. Then, if one judges by the standards usual in statistical hypothesis testing, one will reject the hypothesis that the coin is fair.

It is tempting to see this reasoning as a “for all practical purposes” or FAPP version of an argument in formal logic. Suppose

$$P \land Q \Rightarrow \bar{R}$$

(where $\bar{R}$ signifies “not R”). Suppose it is known that $P$ and $R$ are both true. Then it follows that $Q$ is false.

The argument concerning the 24 coin tosses looks, superficially, like a “wobbly” version of this, in which the logical rigours have been slightly relaxed. Let $P$ = “the coin is tossed 24 times, the tosses being independent”, $Q$ = “the coin is fair” and $R$ = “24 heads obtained”. Then it is tempting to think

$$P \land Q \Rightarrow \bar{R}$$

(where $\bar{R}$ signifies “FAPP not R”). We know (or assume) that $P$ and $R$ are both true. So we conclude that $Q$ is false.

However, that badly misrepresents the real logic of the argument. Instead of 24 coin tosses consider a lottery with $2^{24}$ tickets (this is about the number of tickets in the British national lottery). Let $P$ = “Alice buys one ticket”, $Q$ = “the lottery is fair” and $R$ = “Alice wins”. Then, if one accepts the reasoning in the last paragraph, one must apparently accept that here too

$$P \land Q \Rightarrow \bar{R}.$$  

Suppose, now, that Alice does buy one ticket, and the ticket does win. Then, if the reasoning in the last paragraph is valid, it would follow that here too $Q$ is false FAPP. However, that conclusion is clearly not justified. One cannot reasonably infer that a lottery is unfair just on the grounds that somebody wins it.

The fallacy in this argument is the assumption leading to Eq. (3): that if $R$ is highly improbable, then $R$ is effectively impossible. This assumption is justified in some situations, but not in others. The problem is to decide exactly when it is justified.

Let us note that the problem has nothing essentially to do with the exact size of the probabilities. The argument based on Eq. (3) would still be invalid if Alice had won a cosmic lottery having $10^{100}$ tickets. The microstate of the air in the room where I am now writing is even more improbable: but it still happened.

In considering this question one needs to distinguish two different kinds of probabilistic argument, which I will call predictive and retrodictive.

A predictive argument is one in which conclusions are drawn regarding an unknown event (“predictive” because, although the event may be in the past, the discovery as to whether it happened lies in the future). For instance, Alice judges that she is very unlikely to win the lottery. So she works on the assumption that she will still be in salaried employment next month.

A retrodictive argument moves in the reverse direction. After the data has come in, one uses the information to revise, or update one’s original probability assignment. For instance, after observing a run of 24 heads, one rejects the hypothesis that the coin is fair.

The two examples discussed above—the 24 coin tosses, and Alice’s lottery ticket—seem to differ very little so far as the predictive aspect is concerned (though I will argue in Section 11 that the concept of FAPP impossibility involves some subtleties
even in the predictive case). On the other hand there seems to be a major difference from the retrodictive point of view. It is on that that I now want to focus.

The first response of a convinced frequentist might be that the difference is due to the fact that, whereas in the first case one has an ensemble of 24 events, in the second case one is dealing with a singular event. However, that cannot be correct. It is easy to think of situations where one can validly draw retrodictive conclusions from singular events.

For instance, the probability of a single $^{226}$Ra nucleus decaying in the next hour is $5 \times 10^{-8}$—about the same as the chance of Alice winning the lottery. Suppose that a single nucleus, initially believed to be $^{226}$Ra, and held in a trap, actually does decay in less than 1 hour. This might well cause one to doubt whether it really was a $^{226}$Ra nucleus—whether the decay probability really was $5 \times 10^{-8}$.

What is the difference between this example and the example of Alice’s lottery ticket? In both cases one has a singular event, initially judged to be very improbable. In the second case the occurrence of this event leads us to question the initial probability assignment whereas in the first it does not. Why is that? What is the underlying logical principle on which the decision depends?

I am going to argue (here, and in Sections 5–8) that the difference has to do with our background assumptions. We know that it is easy to mis-identify a single nucleus and so, when the supposed $^{226}$Ra nucleus decays much sooner than expected, that naturally increases our readiness to embrace the alternative hypothesis, that the experimenter made a mistake. On the other hand, the suggestion that the lottery might have been biased specifically in Alice’s favour (as opposed to one of the other $2^{24}$ ticket holders) initially strikes us as comparatively implausible.

One’s judgment regarding the lottery might be different if one knew more about it. Suppose, for instance, one knew that Bob, who runs the lottery, is Alice’s best friend. Then one might see the event, that Alice wins the lottery, as cause for suspicion.

Suppose that Alice’s winning ticket is number 5,592,405. This would not usually be seen as suspicious. Suppose, however, one noticed that in base 4 her ticket number is 11111111111, and suppose one then discovered that the lottery outcome is generated by tossing a tetrahedral die 12 times. Then the fact that this was the winning number might seem very suspicious.

We get some further insight into the role of background assumptions from a modification of Goodman’s well-known “grue” argument. Goodman uses this idea to analyze inductive reasoning. However, a related argument applies to retrodictive probabilistic reasoning (which can be seen as a generalized form of inductive reasoning).

Goodman defines “grue” to be the property ordinarily called “green” before a certain time $t$, and the property ordinarily called “blue” afterwards. Before $t$ every observed emerald has been grue (which is to say green). A naive inductive argument would then suggest that observed emeralds will also be grue (which is to say blue) after time $t$.

To apply this idea in the case of interest here, choose some infinite sequence $x = (x_1, x_2, \ldots)$ of 0’s and 1’s. For the sake of definiteness, let us take $x$ to be the digits in the binary expansion of the fractional part of $\pi$ (so $x = (1, 1, 0, 0, 1, 0, 0, 1, 0, \ldots)$). On the $n$th toss define “heils” to be the event “heads” if $x_n = 1$ and “tails” if $x_n = 0$. Define “taads” to be the event “not heils”.

Suppose now that in 24 tosses we obtain 24 heils and no taads. In conventional terms this is the sequence HHTTHTHTTTTHHHHHHTHTHT consisting of 13 heads and 11 tails, and it would not usually be seen as significant. But if retrodictive inferences worked in the way that Popper and Gillies think it would have to be seen
as highly significant. If the hypothesis that the coin is fair is FAPP falsified by 24 heads in succession, then it must, on their principles, also be FAPP falsified by 24 heads in succession.

The reason we would not normally see 24 heads in succession as grounds for doubting a coin’s fairness has, I believe, to do with our background beliefs. We attach significance to 24 heads in succession because we can envisage a physical mechanism by which the coin plausibly might be biased in favour of heads. By contrast, a bias in favour of heads seems, on physical grounds, comparatively implausible.

Our assessment might be different if our background knowledge was greater. Suppose, for instance, we knew that the coin was ferromagnetic, and suppose we also knew that there was a powerful electromagnet in the vicinity which was switched on when \( x_n = 0 \), and off when \( x_n = 1 \). In that case we might consider 24 heads in succession to be no less significant than 24 heads in succession.

Probabilistic reasoning is, in short, very sensitive to context. I will develop this point in Section 5–9. I will resume my discussion of the frequentist idea in Section 10.

4. Propensities

Although Popper began as a frequentist he later switched to a propensity interpretation [55, 56, 57]. A substantial part of the philosophical community has followed him in this (see Gillies [19, 53] and references cited therein).

Popper saw this development as an evolution in his thought (albeit an important evolution), not a clean break with the past. Furthermore, it is a step which von Mises to some extent anticipated, as Howson and Urbach [35] (p. 221) remark. According to von Mises [38] (p.14)

“The probability of a 6 is a physical property of a given die and is a property analogous to its mass, specific heat, or electrical resistance. Similarly, for a given pair of dice (including of course the total setup) the probability of a ‘double 6’ is a characteristic property, a physical constant belonging to the experiment as a whole and comparable with all its other physical properties. The theory of probability is only concerned with relations existing between physical quantities of this kind. ”

There are, of course, some important differences between Popper and von Mises. In particular, Popper admits objective single-case probabilities. Gillies [19] (p. 114), however, argues that this is not essential to the propensity concept. Furthermore, the fact that von Mises defines probabilities counter-factually (see Section 2) shows that he is really thinking of them as dispositional properties (defined contextually, relative to the “experiment as a whole”, as he puts it in the above passage). In short, it seems to me that, although von Mises is usually described as a frequentist, he is in fact a propensity theorist.

I doubt whether there can ever really have been a pure frequency theorist: i.e. a frequentist who actually denies that “the probability of a 6 is a physical property of a given die . . . (including of course the total setup)” Such a position would have some very peculiar consequences.

The pure frequentist position would (presumably) be that a probability just is a (limiting) relative frequency in some ensemble: absolutely any ensemble, irrespective of how it is defined. It could be the ensemble which consists of all the throws of a particular coin. But it could equally well be the ensemble which consists of \( 10^4 \) throws of one coin, followed by \( 10^4 \) throws of a different coin. It could even be the ensemble which consists of \( 10^4 \) throws of a coin, followed by \( 10^4 \) Stern-Gerlach measurements.
Suppose one really did make no distinction between these cases. Then it would not only be legitimate to argue

because this coin has come up heads on approximately 50% of the last $10^4$ tosses, therefore this same coin may be expected to come up heads on approximately 50% of the next $10^4$ tosses.

It would be equally legitimate to argue

because this British penny has come up heads on approximately 50% of the last $10^4$ tosses, therefore that American dollar may be expected to come up heads on approximately 50% of the next $10^4$ tosses.

It would even be legitimate to argue

because this British penny has come up heads on approximately 50% of the last $10^4$ tosses, therefore a Stern-Gerlach arrangement may be expected to give the result “spin up” in approximately 50% of the next $10^4$ measurements.

I believe that no frequentist would argue like this. I think that must mean that every supposed frequentist is in fact tacitly operating with some kind of propensity notion.

None of this detracts from the importance of Popper’s shift to a propensity approach. Popper took an idea which, though tacitly present all along, had never been sufficiently emphasized, and he placed it centre stage. This was a significant step.

The concept of a propensity is clearly implied by the way that physicists talk. For instance half-lives are typically tabulated next to masses, as if they were just one more physical property. However, I do not think it is simply a matter of language. It seems to me that the concept plays an essential role in the internal logic of current physical theories.

Suppose, for instance, Alice takes a large sample of $^{228}\text{Ac}$ nuclei in her laboratory in London, and finds that approximately 50% of them have decayed after 6 hours. Then she can legitimately infer that

approximately 50% of a sample of $^{228}\text{Ac}$ nuclei in Calcutta may be expected to decay in 6 hours.

But she cannot legitimately infer that

approximately 50% of a sample of $^{226}\text{Ra}$ nuclei in London may be expected to decay in 6 hours.

The fact that this point is obvious should not be seen as detracting from its importance. Basic logical principles generally do seem obvious. Suppose one wanted to programme a robot to understand logical arguments, and perform physical experiments. Then the robot would go badly wrong if one failed to programme it with modus ponens (the principle that, if $P$ is true, and $P \Rightarrow Q$, then $Q$ is true). It would also go badly wrong if one failed to programme it with the information that a half-life is tied to the nuclear species, not to the place it was measured.

Of course, in other contexts a probability can be tied to a spatio-temporal location. This happens in quantum field theory, for example. The point I am making is simply that the idea of a probability being logically tied to some non-probabilistic physical entity plays an essential logical role in our current physical theories. And

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Footnote: The reader may question my use of the word “logical”. Science depends on the procedure by which we uncertainly infer, from observations of one set of events, predictions about other events. Since it is a matter of drawing inferences this procedure may fairly be described as “logical”. Keynes attempted to explicate probabilistic reasoning in terms of a novel kind of non-deductive logic. Ramsey criticized that idea. In so far as it is directed at Keynes’s specific...
it is that concept, of a logically bound probability, which I take to be what is essentially meant by the term “propensity”.

As I am presenting it here the concept of a propensity is, first and foremost, a logical concept. Under the influence of Popper philosophers have tended to view propensities as physically real properties. But the logical concept, as I have formulated it here, is consistent with an epistemic point of view.

The trouble with thinking of propensities in the way that Popper suggests is that it prompts questions like: what exactly is the difference between (1) a $^{228}$Ac nucleus which decays after 26 hours in spite of having had a propensity to decay much sooner and (2) a $^{228}$Ac nucleus which simply decays after that many hours, without being troubled by any conflicting propensity? It seems clear that any difference there may be is unobservable.

If one looks at propensities as objectively real properties they are likely to seem empirically irrelevant. If however, one looks at them from a logical perspective then it can be seen that they are highly relevant, even though they are unobservable.

It is not, in general, necessary for an entity to be directly observable in order for it to be empirically relevant. It is enough that it be non-redundantly embedded in a structure of empirical thought. The state vector is not directly observable; nor are logical relations. But they could not be considered empirically irrelevant.

5. RETRODICTIVE INFERENCE: THE BAYESIAN METHODOLOGY

I argued in Section 2 that retrodictive inferences depend on background assumptions. Those assumptions are themselves probabilistic in character. Their role emerges most clearly in the Bayesian approach, discussed in this section. I will discuss the orthodox theory of statistical inference in the next section.

The term “Bayesian” tends to be associated with an epistemic interpretation of probability. However, there are objectivists who favour the Bayesian methodology. I argued that von Mises, though usually described as a frequentist, is really a propensity theorist. He is, besides, a Bayesian (see von Mises [38], pp. 117–25, 157–9 and von Mises [39], chapters VII and X).

I myself take an epistemic view. However, for the purposes of this paper it is more appropriate to present the Bayesian methodology from an objectivist perspective, proposal it seems to me that Ramsey’s criticism is justified. On the other hand Ramsey (possibly) and de Finetti [31] (definitely) have suggested that probabilistic reasoning can be based purely on the principles of deductive logic (via the Dutch book construction). I am not persuaded by that proposal either.

At the beginning of the 1930’s Wittgenstein [58, 59] took to using the word “grammar” in preference to the word “logic” (in this connection it may be worth noting that Wittgenstein cites Ramsey as a major influence on his thinking in the preface to Wittgenstein [60]). I choose not to use the word “grammar” here because grammar has nothing specially to do with reasoning, and because grammatical principles are, to a considerable extent, arbitrary (pace Chomsky). Nevertheless, the concept I have in mind is at least as close to the concept Wittgenstein intends by the word “grammar” as it is to the concept Keynes intends by the word “logic”.

In contrasting this with the idea that a propensity is an objectively real property I do not mean to suggest that it is therefore subjective. I think the subject-object dichotomy is potentially very misleading. I do not believe the world genuinely is sundered, absolutely, quite in the manner this terminology suggests. Like Bohr I think that is the single most important lesson of quantum mechanics.

Wittgenstein [58] (pp.126–7) asks “How do I know that the colour red can’t be cut into bits?” This is surely not an empirical observation. However, although the proposition can hardly be described as an observed fact, or something inferred from observed facts, it seems to me that it cannot appropriately be described as “subjective”.

I would argue that logical relationships are constitutive of what we normally think of as reality (c.f., for example, Wittgenstein [58], p.116, where he speaks of the “logical form” of a patch in the visual field).
such as that of von Mises. I want to expose the deficiencies of the objectivist idea. The best way to do that is to adopt objectivist assumptions, and see where they lead.

Also, I want to compare the Bayesian approach with what is now the orthodox approach. This was developed by Fisher and others in the first few decades of the last century in conscious opposition to the Bayesian methodology. If one wants to understand Fisher’s reasons for rejecting the Bayesian approach one needs to look at it through objectivist eyes.

Let \( s_i = 0 \) (respectively 1) if the coin comes up tails (respectively heads) on the \( i \)th toss. Let \( s = (s_1, s_2, \ldots, s_n) \) be the sequence describing the result of the first \( n \) tosses. Let \( p \) be the true probability of heads (I am following von Mises’s objectivist account, so I am assuming that there does exist a real, objective probability of heads). Then, if the tosses are independent, the probability of obtaining the sequence \( s \) is

\[
P(s|p) = p^h(1 - p)^{n-h(s)}
\]

where \( h(s) = \sum_{i=1}^{n} s_i \) is the number of heads.

Now if it was a certain fact that the coin was fair we would have \( p = 1/2 \) and every sequence \( s \) would have the same probability \( 1/2^n \). Suppose, however, that the coin was randomly selected from a large population of coins, not all of which are fair. Let \( f_i(p)dp \) be the probability that the probability of heads is in the interval \((p, p + dp)\). Then the unconditional probability of obtaining the sequence \( s \) is

\[
P(s) = \int_0^1 P(s|p)f_i(p)dp .
\]

Suppose, now, that we do toss the coin \( n \) times, and suppose we obtain the sequence \( s \). Then, by an application of Bayes’s theorem, the probability that the probability of heads is in the interval \((p, p + dp)\), conditional on this new information, is

\[
P(p|s)dp = \frac{P(s|p)f_i(p)dp}{\int_0^1 P(s|p)f_i(p)dp} .
\]

Setting \( f_i(p) = P(p|s) \) this gives, in view of Eq. (4),

\[
f_i(p) = Kp^{h(s)}(1 - p)^{n-h(s)}P_i(p)
\]

where \( K \) is a normalization constant. \( f_i(p) \) is usually called the prior (or a priori) distribution, and \( f_i(p) \) the posterior (or a posteriori) distribution.

It will be observed that von Mises explicitly appeals to the concept of a second-order probability: a probability of a probability. Savage \( ^{32} \) (p. 58), among others, has criticized this idea. However, if one accepts von Mises’s objectivist viewpoint the concept must be regarded as legitimate\(^6\). As we saw von Mises, though usually described as a frequentist, is in fact a propensity theorist. That is he thinks that the probability of a coin coming up heads is a physical property of that particular coin (plus the tossing method) \( \text{von Mises} ^{33}, \text{p. 14} \). If one makes that assumption, and if it is legitimate to consider the probability that the mass \( m \) of a randomly selected coin lies in a certain interval, then it must be equally legitimate to consider the probability that the probability \( p \) of the coin coming up heads lies in a certain interval.

To understand the significance of Eq. (4) (as von Mises sees it) consider, for simplicity, a case where there are only two possible values of \( p \). Imagine a bag

\(^6\)Of course, from de Finetti’s viewpoint it makes no sense to interpret \( f_i(p)dp \) as a probability (on de Finetti’s assumptions a second order probability would have to represent a belief about one’s own belief, not a belief about the object of interest). de Finetti has found a most ingenious way round this difficulty. In his scheme the weight function \( f_i(p) \) encodes a probability distribution, without itself being a probability distribution.
containing a large number of fair coins, with \( p = 0.5 \), and a much smaller number of biased coins, with \( p = 0.9 \). For the sake of definiteness suppose that the proportion of biased coins is \( 10^{-4} \).

Now consider the experiment which consists in shaking the bag, selecting a coin at random, tossing the coin 30 times, and then replacing it. Suppose this experiment is repeated infinitely many times. Let \( S_i \) be the set of all such experiments, and let \( S_f \) be the subset obtained by selecting just those cases where the coin came up heads on each of the 30 tosses. Then \( S_i \) is described by the distribution

\[
f_i(p) = 0.9999 \delta(p - 0.5) + 0.0001 \delta(p - 0.9) \tag{8}
\]

while \( S_f \) is described by the distribution

\[
f_f(p) = 0.0002 \delta(p - 0.5) + 0.9998 \delta(p - 0.9) \tag{9}
\]

(as follows from Eq. 7). In the set of all experiments the coin is fair in 99.99% of cases. But in the subset of experiments in which the coin comes up heads on each of 30 tosses, the coin is biased in 99.98% of cases.

It will seen from Eq. 7 that, in a Bayesian inference, the conclusion to the argument (the distribution \( f_f \)) is produced by the interplay between a probabilistic premise (the distribution \( f_i \)) and a set of factual observations (the sequence \( s \)). It is this interplay which explains the point I made in Section 3, that some observations force a major change in our beliefs while others, though equally improbable on our starting assumptions, do not.

It seems to von Mises that the Bayesian methodology provides a perfectly clear, fully objective theory of retrodictive probabilistic reasoning. He is at a loss to understand why it is not more widely accepted \(^8\) (pp.158–9):

\[\text{I do not understand the many beautiful words used by Fisher and his followers in support of the likelihood theory . . . }\]

\[\ldots \text{We can only hope that statisticians will return to the use of the simple, lucid reasoning of Bayes’s conceptions.}\]

\(^8\): de Finetti’s epistemic interpretation of probability is at the opposite pole from von Mises’s objectivism. However, he and von Mises are at one in their attitude to orthodox statistics. de Finetti \(^3\) (p. 245) says, for example,

\[\text{“Those who reject the Bayesian approach cannot base their inferences on the posterior distribution even if they wished to—it does not make sense so far as they are concerned. As a result, they are forced to have recourse to } \text{ad hoc criteria, and hence to open the floodgates to arbitrariness. . . . The best they can . . . do is to base themselves on the likelihood function; failing that, they simply resort to playing with formulae that are without any real foundation.”}\]

I will argue below that these criticisms of the orthodox methodology are justified. However, I first want to examine Fisher’s reasons for rejecting the Bayesian approach.

Fisher \(^6\) (also see Fisher \(^5\)) gives a detailed discussion of the Bayesian methodology\(^9\). He is at pains to defend it against most of the attacks which have been made on it. He argues \(^5\) (pp. 8–38) that criticism has mainly been directed at inappropriate applications of the method, which fail to respect the conditions of Bayes’s theorem, and that the method itself is perfectly sound. He also says \(^6\) (p. 48) that there can be no fundamental objection to second-order probabilities:

\[\text{von Mises } \tag{8}\]

\[\text{Fisher emphatically avoids all reference to Bayes’s solution of the problem of inference; this is for him a matter of principle.} \]

\[\text{von Mises presumably wrote this for the first edition of his book, before he had read Fisher } \tag{9}\]

\[\text{At any rate, it is a misapprehension. Fisher does not dismiss the Bayesian methodology out of hand, without argument.}\]
probabilities of probabilities. His only objection to the Bayesian method is that the prior \( f_i \) is, in practice, usually unknown. This means that, in most cases, Eq. (7) merely expresses one unknown in terms of another.

It is generally true that the Bayesian can only get a probabilistic judgment out, as the conclusion to a piece of retrodictive reasoning, if s/he begins by feeding a probabilistic judgment in, as an initial assumption. Fisher’s point is that, in most (though not all—see Fisher [61], pp. 18–20 and 127–132) applications, that initial assumption cannot be based on known facts about the objective situation. In his view that makes the Bayesian method, in most applications, scientifically useless.

von Mises [39] (pp. 339–45) (also see von Mises [38], pp. 122–4) has a response to this objection. If \( n \) is large then
\[
p^{h(s)}(1 - p)^{n - h(s)} = K'\delta(p - p_0)\]
where \( p_0 = h(s)/n \) and \( K' \) is a normalization constant. Inserting this expression in Eq. (7) one obtains, on the assumption that \( f_i \) satisfies certain conditions (which von Mises explicitly states)
\[
f_i(p) \approx \delta(p - p_0)
\]
independently of \( f_i \). von Mises infers that if one obtains \( r \) heads in \( n \) coin tosses, and if \( n \) is sufficiently large, then it is nearly certain that \( p \approx r/n \).

His response is inadequate for two reasons. In the first place \( f_i \) might not satisfy von Mises’s conditions. If, for example, \( f_i(p) = \delta(p - 0.5) \), then \( f_i(p) = \delta(p - 0.5) \) whatever the values of \( r \) and \( n \). von Mises would doubtless argue that his conditions are very plausible. However, the fact is that they are empirically unfounded. No set of observations could ever contradict the assignment \( f_i(p) = \delta(p - 0.5) \).

The second problem is that, even if we accept von Mises’s conditions, there is no empirical criterion to tell us how large \( n \) must be for it to be nearly certain that \( p \approx r/n \). Suppose, for instance, we have obtained 60 heads in 100 tosses. For some choices of \( f_i \) this would be very strong evidence that the coin is biased; for others it would not. The question, as to which alternative applies, cannot be decided empirically.

In the case of a die von Mises [38] (p. 123) suggests that 500 throws should be enough to give a reliable estimate of the true probabilities. But he does not adequately explain where this number is coming from. It cannot be inferred just from the observations, without additional assumptions.

Fisher, like von Mises, takes an objectivist view. I think he must be correct to think that, seen from that perspective, this feature of the Bayesian methodology is unacceptable. The trouble is that the orthodox methodology, which Fisher and others devised in an attempt to get round the problem, does no better.

6. RETRODICTIVE INFERENCES: THE ORTHODOX METHODOLOGY

A Bayesian argument takes an initial probability assignment \( P \) (the prior \( f_i \)), adds to it some factual data \( F \), and from this derives a new probability assignment \( Q \) (the posterior \( f_i \)). Formally:
\[
P \land F \Rightarrow Q.
\]
Fisher’s problem is that all we observe is the factual data \( F \). He consequently thinks that, if probability assignments are not to float free of any empirical attachment, it is essential that Bayesian inferences of the form [11] be supplemented with a new kind of inference having the general form
\[
F \Rightarrow Q
\]
where a probability assignment \( Q \) is inferred directly from the factual data \( F \), without the assistance of any prior probabilistic assumption (see, for example, Fisher [61], pp. 54–5). In this section I hope to convince the reader that valid inferences of this form do not exist.
I believe that Fisher is entirely correct in thinking that the dependence of Bayesian inferences on prior probabilistic assumptions is a serious difficulty for one of his philosophical persuasion. Where he goes wrong is in thinking that this is a reason for rejecting the Bayesian methodology. It is not just Bayesian inferences which have the form of Eq. (11), but every retrodictive probabilistic inference, without exception.

Consider

**Alice’s argument:** Alice spins a roulette wheel once and obtains the number 13. She concludes that the wheel is fair.

This argument is clearly invalid. A single spin of a roulette wheel tells one virtually nothing about the underlying probability distribution. The fact that the number 13 came up once, in a single spin of the wheel, does not imply that the other numbers are even possible, much less that they each have probability 1/37. It also seems a little strange to argue that, because 13 did occur, therefore 13 is not very likely to occur—an anti-inductive argument, as it might be called.

Now compare

**Bob’s argument:** Bob tosses a coin 1000 times and obtains 500 heads. He concludes that (0.459, 0.541) is a 99% confidence interval for the probability $p$ of the coin coming up heads.

It may appear that Bob has solid reasons for this conclusion. But in fact, if Bob is claiming to extract his conclusion just from the empirical data, without additional assumptions, then his claim is no better founded than Alice’s.

A sequence of 1000 coin tosses is equivalent to 1 spin of a big roulette wheel, divided into $2^{1000}$ sectors. Let $b$ be the particular sequence which Bob obtains. Then, on the basis of one spin of the equivalent roulette wheel, Bob is arguing

Because sector $b$ did occur, therefore each of the sectors which did not occur has probability $\geq 6 \times 10^{-339}$

He is also arguing anti-inductively:

Because $b$ did occur, therefore $b$ is very unlikely to occur.

If Bob really was basing himself just on the observed facts, and nothing else whatever, his argument would have the same extraordinary features as Alice’s argument. It would clearly be invalid.

Of course, Bob is not really basing himself just on the factual data. He is supplementing the factual observation

$F =$ “coin tossed 1000 times and sequence $s$ obtained”

with the prior probabilistic assumption

$P =$ “tosses are independent, and probability of heads is constant”

$P$ is logically equivalent to the statement that, for some fixed $0 \leq p \leq 1$, the probability of obtaining an arbitrary sequence $s$ is

$$P(s) = p^{h(s)}(1-p)^{1000-h(s)}$$

(13)

(where $h(s)$ is the number of heads in the sequence $s$, as before).

The set of all probability distributions on the space of sequences of 1000 heads and tails is a $(2^{1000} - 1)$ parameter family. The assumption $P$ restricts the class of admissible distributions to the one parameter family specified by Eq. (13). That is a severe restriction. Without such a restriction no valid, non-trivial retrodictive inference is possible.

If the class of admissible distributions is suitably restricted, then one can legitimately draw probabilistic conclusions from a single spin of an ordinary roulette wheel.
To see this let $H_l$ be the hypothesis
\[
P(r) = \begin{cases} 
\frac{1}{18} & \text{if } l \leq r \leq l + 9 \\
0 & \text{otherwise}
\end{cases}
\]
where $l = 0, 1, \ldots, 28$ and $P(r)$ is the probability of obtaining the number $r$ when the wheel is spun once.

Now let $F$ = “wheel was spun once and number 13 was obtained” and let $P$ be the disjunction
\[P = \bigvee_{l=0}^{28} H_l\]
$F$ on its own has virtually no probabilistic implications (beyond the obvious implication, that 13 was possible and that none of the other numbers was certain). However, the conjunction $P \land F$ does have substantial probabilistic implications\(^\text{10}\), namely, the proposition $Q = \bigvee_{l=4}^{13} H_l$.

If Alice is permitted to assume $P$ then she can validly argue, for example,

(a) because 13 did occur, therefore 12 and 14 are not both impossible. and

(b) because 13 did occur, therefore 13 has probability $1/10$.

However, she is only getting these probabilistic conclusions out because she began by feeding a probabilistic assumption in. In particular, the superficially anti-inductive character of inference (b) is not mysterious. Alice only arrives at the conclusion, that 13 is unlikely to occur, because she began by assuming that 13 is unlikely to occur. If $P$ is true, then the probability of 13 is necessarily $\leq 1/10$.

Alice is, in fact, assigning the maximum probability consistent with her starting assumptions.

This example—Alice’s modified argument, as I will call it—is admittedly artificial. However, the same idea of assuming a disjunction $P$, and then using the observations to narrow it down to a smaller disjunction $Q$ is at the root of many, if not all orthodox statistical inferences.

Let us go back to the example of 1000 coin tosses. Let $H_p$ be the hypothesis “tosses are independent, and the probability of heads is $p$ on every toss”. If $H_p$ is true the probability of obtaining the sequence $s$ with $h(s)$ heads is $p^{h(s)}(1-p)^{1000-h(s)}$. Unlike the distributions in Alice’s modified argument, this distribution is everywhere non-zero (unless $p = 0$ or 1). However, it is sharply peaked at $h(s) = 1000p$. Orthodox statisticians take it that, away from this peak, the distribution may be regarded as effectively zero. They think this entitles them to proceed in the same manner as Alice, in her modified argument. They begin by assuming the disjunction $P = \bigvee_{p \in [0,1]} H_p$, and then use the observed sequence $s$ to narrow this down to a disjunction $Q = \bigvee_{p \in I_c} H_p$, where $I_c$ is a confidence interval.

For example, suppose as before that a sequence $b$ containing 500 heads is obtained. Let $E_-$ (respectively $E_+$) be the event that the number of heads is $\leq 500$ (respectively $\geq 500$). Let $P(E_+|H_p)$ be the conditional probability of $E_+$ given $H_p$. Then
\[P(E_+|H_p) \leq 0.005\] when $p \leq 0.459$ while
\[P(E_-|H_p) \leq 0.005\] when $p \geq 0.541$. If $P(E_+|H_p) = 0$ whenever $p \leq 0.459$ and $P(E_-|H_p) = 0$ whenever $p \geq 0.541$ we would be in the same situation as Alice, in her modified argument. We could conclude that $p$ certainly $\in (0.459, 0.541)$ (modulo the qualification in

\(^\text{10}\) At least, it does if one is allowed to assume that zero probability events are impossible.
footnote 10). As it is the conditional probabilities \( \neq 0 \), and so certainty would not be justified. The probabilities are, nevertheless, small. Orthodox statisticians consider that this entitles us to be, in some sense, confident that \( p \in (0.459, 0.541) \).

The orthodox analysis depends, not just on the factual data, but also on the probabilistic assumption \( P = \bigvee_{p \in [0,1]} H_p \). This assumption is no more empirical than the choice of Bayesian prior.

In Section 3 I appealed to the idea that a coin might be systematically biased in favour of heils: i.e. biased towards heads on some tosses and tails on others. This suggestion may have seemed fanciful. But it does in fact describe what a classical determinist like Laplace believed to be true in objective reality. Laplace thought that for a superhuman being, who had complete knowledge of the objective situation, the probability of heads on any particular toss would always be either 0 or 1. In other words, he thought that if “heils” and “taads” are appropriately defined then, for such a being, the probability of heils is always 1.

The de Broglie-Bohm theory indicates that the empirical predictions of quantum mechanics are consistent with complete determinism at the micro-level. Consider, for example, a measurement of \( \sigma_z \) on a succession of particles each initially in an eigenstate of \( \sigma_x \). We cannot, at present, empirically exclude the possibility that, for each particle that passes through the apparatus, the measurement outcome is fully determined by the initial conditions. Consequently there is, at present, no way to empirically decide between the hypotheses:

- **A:** For every particle the probability of obtaining the result “spin-up” is \( 1/2 \)
- **B:** For every particle the probability of obtaining the result “spin-up” is either 0 or 1.

Nor is there any obvious way to empirically exclude

- **C:** For every particle the probability of obtaining the result “spin-up” is either 0.4 or 0.6.

—not to mention other, more complicated possibilities.

Analogous considerations apply to the proposal that the measurement outcomes are statistically independent. Let \( X_n \) be the \( n^{th} \) measurement outcome. There is no obvious way to empirically discriminate

- **A’:** \( P(X_{2n} = X_{2n+1}) = 1/2 \) for all \( n \).
- **B’:** \( P(X_{2n} = X_{2n+1}) = 0 \) for some values of \( n \) and 1 for others
- **C’:** \( P(X_{2n} = X_{2n+1}) = 0.4 \) for some values of \( n \) and 0.6 for others.

(not to mention other, more complicated possibilities).

Similar remarks apply to the coin-tossing example.

It may be suggested that the assumption \( P = \bigvee_{p \in [0,1]} H_p \) seems very plausible. I would agree with that: it seems very plausible to me also. However, that has no bearing on my argument here. I am here only making the simple logical point, that \( P \) represents an additional assumption, not contained in the empirical data.

Hume argued that we have no sufficient empirical reason for expecting the sun to rise tomorrow. The contrary proposal, that the sun will probably not rise tomorrow, does—of course—seem very implausible. But Hume does not deny that. His point is not that our ordinary belief is not very plausible, but only that it cannot logically be derived purely from the observed facts, without any non-empirical input. The same is true of conclusions reached by the kind of generalized inductive argument considered here.

One might try to argue that the assumption \( \bigvee_{p \in [0,1]} H_p \) can be justified by appealing to the results of previous coin-tossing experiments. However, any conclusions drawn from those previous experiments would themselves have to depend on previous prior assumptions. Trying to justify probability by probability is like
trying to justify induction by induction: it cannot be done, unless the pump has first been primed, by some initial non-empirical assumption.

7. Orthodoxy v. Bayesian

In the last section I argued that the orthodox methodology relies on prior, non-empirical assumptions, just like the Bayesian methodology. However, it may look as though the orthodox methodology is still preferable because it does not require us to make so many such assumptions. As we saw in Section 5, von Mises’s objectivist version of the Bayesian methodology, as applied to the coin-tossing example, relies on the prior assumption \( \forall p \in [0,1] H_p \), just as the orthodox approach does. But it also relies on the prior distribution \( f_i \). By contrast, it may appear that the orthodox approach does not make any non-empirical assumption additional to \( \forall p \in [0,1] H_p \).

However, it will be found on closer examination that this is not correct. Assumptions corresponding to the distribution \( f_i \) play an essential role in the orthodox approach. The only difference is that, whereas in the Bayesian approach these assumptions are explicitly built into the formalism, in the orthodox approach they are tacit, and often unrecognized. All that the orthodox statistician achieves by suppressing the function \( f_i \) is to produce a misleading appearance of greater objectivity, at the price of a serious loss of logical coherence. In particular, the orthodox methodology obscures the point which emerged from the discussion in Section 3: that retrodictive inferences are critically dependent on our background knowledge and beliefs.

I noted at the end of Section 5 that the Bayesian approach fails to give a purely objective criterion for deciding how many tosses are needed to tell whether a coin is biased. Suppose, for instance, a coin comes up heads on 60 out of 100 successive tosses. For some choices of prior distribution \( f_i \) this will imply that there probably is a substantial bias, for others it will not. On these grounds Fisher rejects the Bayesian methodology. Yet the alternative methodology which he advocates is no more objective.

On the hypothesis that the coin is fair (and the tosses independent) the probability that heads will come up more than 60 times in 100 successive tosses is 0.028. So if we perform a one-tailed test the hypothesis, that the coin is fair, will be rejected at the 95% level, but not at the 99% level of significance. If, on the other hand, we perform a two-tailed test the hypothesis will not be rejected even at the 95% level (though it will be rejected at the 90% level). So do we accept that the coin is biased or not? That, it seems, is up to the subjective decision of the statistician.

Fisher [61] (p. 45) has this to say, regarding the choice of significance level:

“no scientific worker has a fixed level of significance at which from year to year, and in all circumstances, he rejects hypotheses; he rather gives his mind to each particular case in the light of his evidence and his ideas”

In other words, the choice of significance level depends on exactly the same factors which determine the choice of Bayesian prior: namely, the background knowledge and beliefs of the statistician. Similarly with the decision as to whether to use a one-tailed or a two-tailed test.

The orthodox methodology might be considered superior to the Bayesian methodology if the statement, that \( H \) is rejected at the 95% level, meant that \( H \) is false with probability \( \geq 0.95 \). However, the statement cannot validly be interpreted in

\[ \forall p \in [0,1] H_p \]

In de Finetti’s epistemic version Eq. 4 is derived by a different route, in which \( \forall p \in [0,1] H_p \) is replaced by the assumption of exchangeability. The end mathematical result is the same, but the conceptualisation is quite different (see, however, Howson and Urbach [35], pp. 232–3).
this way, as orthodox statisticians are at pains to emphasize. A significance level is not a probability (as is already apparent from the fact that it depends on whether the test is one-tailed or two-tailed).

The fact that orthodox statistical conclusions are not purely objective tends to be obscured by the fact that orthodox statisticians standardly choose to work at only a small number of different significance levels (typically 95% or 99%). However, the fact that everyone judges the same is not, in itself, a reason for taking a judgment to be objective. In any case, Bayesian statisticians could achieve an equal degree of unanimity by the simple expedient of always working in terms of the uniform prior $f_i(p) = 1$ (as, indeed, was originally recommended by Bayes and Laplace [25]).

The orthodox methodology is not superior in point of objectivity. On the other hand it is clearly inferior in point of logical cogency.

The argument we have been considering may be summarized as follows:

**Argument 1:**

If the coin is fair (and the tosses independent) the probability of more than 60 heads in 100 tosses is $\leq 0.05$.

The coin did come up heads on 60 out of 100 tosses.

*therefore*

The hypothesis, that the coin is fair, is rejected at the 95% level.

Now compare this with the example I discussed in Section 3, where Alice wins a lottery having $2^{24}$ tickets. Suppose one were to reason as follows:

**Argument 2:**

If the lottery is fair the probability of Alice winning = $6 \times 10^{-8}$.

Alice did win.

*therefore*

The hypothesis, that the lottery is fair, is rejected at the 99.99999% level.

If argument 1 is valid just as it stands (if the conclusion does not tacitly depend on some additional, inexplicit assumptions), then it is hard to see what objection there can be to argument 2.

It might be suggested that argument 2 is invalid because the conclusion is based on a singular event. However, as we saw in Section 3, it is easy to think of cases where one can validly draw retrodictive conclusions from singular occurrences. Besides, if one wants to erect it as an absolute principle, that retrodictive conclusions must be based on repeated trials, one has to decide just how many repetitions are needed. It is hard to see how the decision can be other than arbitrary.

In any case one can find sufficiently many logical obscurities in the orthodox analysis of the coin-tossing problem. Argument 1 depends on grouping the particular sequence containing 60 heads which did occur together with all the other sequences containing 60 or more heads which did not occur. It is hard to see, on orthodox principles, any compelling reason for adopting this procedure. However, if we did try basing ourselves just on the sequence which actually occurs it would lead to some strange conclusions.

Suppose, for instance, that a coin is tossed 100 times, and a sequence $s$ containing 50 heads is obtained. This would usually be seen as favouring the hypothesis that the coin is fair. However, if retrodictive inferences really were based just on “the resistance felt by the normal mind to accepting a story intrinsically too improbable” (Fisher [61], p. 43) it is hard to see what objection there could be to
Argument 3:

If the coin is fair (and the tosses independent) the probability that $s$ will occur = $7.9 \times 10^{-31}$.

$s$ did occur.

Therefore

The hypothesis, that the coin is fair, is rejected at the 99.99···9% level.

(see Howson and Urbach [35], p. 123).

Of course, no orthodox statistician would argue in that fashion. The question is: why? I do not see how it is possible, on orthodox principles alone, to explain, in clear, logically compelling terms, why argument 1 is valid while arguments 2 and 3 are not.

Although Jaynes [46] has argued that the orthodox methodology is often not the most effective way to extract conclusions from statistical data, I believe no one has questioned the actual validity of conclusions reached by orthodox means. However it is, I think, difficult to disagree with de Finetti [31] (p. 245) when he says that orthodox statistics relies on a multiplicity of ad hoc decisions, whose logical basis is often far from clear.

Jeffreys [24] (p. 393), contrasting his Bayesian approach with Fisher’s orthodox one, comments

I have in fact been struck repeatedly in my own work, after being led on general principles to the solution of a problem, to find that Fisher had already grasped the essentials by some brilliant piece of common sense.

In other words Fisher, notwithstanding his lack of logical system, generally gets the right answer due to the power of his intuition (Jaynes [46] (p. 199) also pays tribute to the depth of Fisher’s intuitive insight). Bell [9] (p. 174) remarks that the Copenhagen formulation of quantum mechanics, in spite of the obscurity of its basic concepts, is still enormously successful on a practical level due to the “discretion and good taste” of its practitioners. The orthodox approach to statistics is equally reliant on these qualities of discretion and good taste.

If one was simply interested in practical problems of error-analysis, epidemiology and the like, the orthodox approach might be satisfactory (see, however, Jaynes [46]). However, the reader of this article is likely to be interested in the foundational problems of science (as, it should be said, is Fisher). From that point of view the logically unsystematic character of the orthodox approach is a serious disadvantage, for it tends to obscure the real character of probabilistic reasoning. The Bayesian methodology is greatly preferable.

If one looks at it in Bayesian terms it is easy to see why the conclusion to argument 1 is valid, whereas the conclusions to arguments 2 and 3 are both invalid.

Let us first formulate the Bayesian approach in general terms. Suppose we have a set of hypotheses $H_1, H_2, \ldots$, (for simplicity assumed discrete), and suppose we assume that $\bigvee_i H_i$ is true, so the prior probabilities satisfy $\sum_i P(H_i) = 1$. Let $E$ be the observed outcome. Then the posterior probability of $H_i$ given the data $E$ is

$$P(H_i|E) = \frac{P(E|H_i)P(H_i)}{P(E)}$$  \hspace{1cm} (17)
where \( P(E) = \sum_{i} P(E|H_i)P(H_i) \). The conditional probability \( P(E|H_i) \) is often called the \emph{likelihood} of \( H_i \) relative to the data \( E \).

There are two points to notice about this formula: (1) the posterior depends on an interaction between the likelihoods \( P(E|H_i) \) and the priors \( P(H_i) \); and (2) the factor \( P(E) \) in the denominator may be, and often is very small—which means that the posterior \( P(H_i|E) \) may be, and often is appreciable even when the likelihood \( P(E|H_i) \) is very small.

Orthodox statisticians neglect both these points. Their desire to fit the theory onto a Procrustean bed of pure objectivism makes them try to get everything from the likelihoods \( P(E|H_i) \) alone. The effect is to mutilate the logical structure of the theory.

Let us now specialize the formula to the case of Alice’s lottery ticket. This will give a formal basis to the intuitive considerations of Section 3.

Let \( N \) be the number of tickets. Let \( H_0 \) be the hypothesis that the lottery is fair, and let \( H_i \) be the hypothesis that ticket \( i \) is certain to win, for \( i = 1, 2, \ldots, N \). Suppose we take \( P(H_0) = 1 - \epsilon \) and \( P(H_i) = \epsilon/N \) for \( i \geq 1 \) and some small \( \epsilon \) (corresponding to a situation where we think there is a small probability of the lottery being rigged in someone’s favour, but have no idea who that someone might be). Let \( a \) be Alice’s ticket, and let \( E \) be the event that Alice’s ticket wins. Then

\[
P(H_0|E) = P(H_0) = (1 - \epsilon). \tag{18}
\]

Under these conditions the event, that Alice wins the lottery, does not change our assessment of the probability of the lottery being rigged—in agreement with ordinary intuition. Note that \( P(H_0|E) \) is close to 1 even though the likelihood \( P(E|H_0) \) is very small. This is because \( P(E) \) is also very small.

Suppose, on the other hand, we attached the same low, prior probability \( \epsilon \) to the hypothesis, that the lottery is biased, but were sure that if it is rigged in anyone’s favour that someone is going to be Alice (corresponding to a situation where we think the lottery organizer is \emph{probably} honest, but happen to know that Alice is his wife). The prior probabilities now are \( P(H_0) = 1 - \epsilon \) and \( P(H_i) = \epsilon\delta_{ia} \) for \( i \geq 1 \). The posterior probability that the lottery is fair, given that Alice won, is then

\[
P(H_0|E) = \frac{1}{1 + N\epsilon/(1 - \epsilon)}. \tag{19}
\]

If one took \( \epsilon = 1/N \) this would give \( P(H_0|E) \approx 1/2 \)—meaning that, even though we start out with a strong conviction that the lottery organizer is \emph{probably} honest, the event of his wife winning makes us very suspicious. This also agrees with ordinary intuition.

The paradox which argument 3 apparently represents can also be resolved by analyzing the problem in Bayesian terms. Let \( H_p \) be the hypothesis “tosses are independent and probability of heads is \( p \) on every toss”, and let \( E \) be the event that the particular sequence \( s \) containing 50 heads and 50 tails is obtained (as in argument 3). Suppose we take \( P(H_p) = 1 \) for all \( p \). Then Eq. (17) becomes

\[
P(H_p|E) = \frac{P(E|H_p)}{P(E)} \tag{20}
\]

where \( P(E|H_p) = p^{50}(1-p)^{50} \) and \( P(E) = \int_0^1 P(E|H_p)dp = 9.8 \times 10^{-32} \). Argument 3 depends on the fact that the likelihood \( P(E|H_{0.5}) = 7.9 \times 10^{-31} \) is very small. If one bases oneself on a supposed “primitive” or “elemental” resistance to accepting highly improbable stories (Fisher [21], p. 46) this means that the occurrence of \( E \) is grounds for rejecting \( H_{0.5} \). But if one bases oneself on the posterior \( P(H_p|E) \), as one logically should, the paradox dissolves. \( P(E|H_p) \) and \( P(E) \) are both very small.
Consequently, their ratio $P(H_p|E)$ is a well-behaved probability density, with total area = 1, and 96% of its area concentrated in the interval (0.4, 0.6).

8. No “probable” from an “is”

I have several times mentioned Hume’s argument for inductive scepticism. Hume is also well-known for his fact-value distinction: the principle that one cannot validly infer an “ought” from an “is”. As he puts it [47] (p. 469):

In every system of morality, which I have hitherto met with, I have always remark’d, that the author proceeds for some time in the ordinary way of reasoning . . . when of a sudden I am surpriz’d to find, that instead of the usual copulations of propositions, $is$, and $is$ not, I meet with no proposition that is not connected with an ought, or an ought not. This change is imperceptible; but is, however, of the last consequence. For as this ought, or ought not, expresses some new relation or affirmation, ’tis necessary . . . that a reason should be given, for what seems altogether inconceivable, how this new relation can be a deduction from others, which are entirely different from it.

The point Hume is making here, that one cannot get a moral injunction out, at the end of an argument, unless one began by feeding a moral assumption in, at the start, is nowadays usually taken for granted. However, when Hume first advanced this proposition it seemed shocking.

Hume’s point is that there exists no valid inference of the form

$$F \Rightarrow N$$  \hspace{1cm} (21)

where $F$ is a statement of fact and $N$ is a moral injunction. If $N$ is validly inferred by an argument having $F$ among its premises, then the inference must be of the form

$$M \land F \Rightarrow N$$  \hspace{1cm} (22)

where $M$ is a prior moral assumption.

In Sections 5–7 we saw that a similar principle applies to probabilistic reasoning. A probability assignment $Q$ cannot be inferred directly from a factual proposition $F$ without other input. A retrodictive probabilistic inference must, instead, be of the form (c.f. Eq. (11))

$$P \land F \Rightarrow Q$$  \hspace{1cm} (23)

where $P$ is a prior probabilistic assumption.

In short: one cannot get a “probable” from an “is”. Probability statements are, in most respects, quite unlike moral statements. However, they have this logical feature in common.

9. Predictive Inferences

Physical thinking has been much influenced by the idea that extremely improbable events are FAPP (“for all practical purposes”) impossible. The idea is attractive because, by converting extreme probability statements into statements of effective fact, it seems to circumvent the need to give a clear physical interpretation of probability as such.

I have argued that in a retrodictive context this idea is unacceptable. On the other hand, in predictive reasoning I think it (probably) is true that a sufficiently low probability event counts as FAPP impossible. However, one needs to be careful.

13Except in the trivial case, where one merely infers, from the fact that $x$ occurred, that $x$ was not impossible, or from the fact that $x$ did not occur, that $x$ was not certain.
For instance, the probability of Alice winning the lottery is $6 \times 10^{-8}$. It is tempting to conclude that she has, from a predictive point of view, effectively no chance of winning. However, it will appear on further reflection that matters are less straightforward. It is true that Alice will, if she is wise, take the event of her winning to be impossible FMPP (“for most practical purposes”). She will not, for example, make heavy financial commitments which she could not meet, except in the event of her winning the lottery. But if she really did think it to be impossible for all practical purposes, she would not have taken the practical step of buying a ticket in the first place.

This example may appear frivolous. So let us consider a less frivolous one. The geological record suggests that the probability of the Earth being hit by a 10 km asteroid some time in the next 50 years is $\sim 10^{-6}$. This is significantly greater than the probability of Alice winning the lottery, but still rather small. If we judged the probability to be (say) $\sim 0.5$ then we might consider it worth devoting a substantial fraction of the world’s GDP to the problem of trying to avert this potential catastrophe. But as it is the probability is small, and so we judge it more appropriate to expend most of the world’s resources on concerns that seem more pressing. Nevertheless, the US government does expend some of its resources on the task of tracking asteroids. At least in the view of the US government, a chance of $10^{-6}$ is not FAPP equivalent to a prediction, that the event in question will certainly not happen.

However, a probability of $10^{-6}$ is still comparatively large. It is difficult to see how considerations of the kind just adduced could apply to probabilities of $10^{-60}$, or $10^{-600}$. We do not, for instance, consider it worth insuring against a macroscopic violation of the second law of thermodynamics.

I am therefore inclined to think that, once a probability is shrunk below a certain point, the event in question does indeed count as FAPP impossible so far as prediction is concerned. This point is not very sharply defined. But it appears to me that a probability of $10^{-6}$ is rather clearly on one side, and that a probability of $10^{-60}$ is no less clearly on the other. However, that does not entirely settle the question. We need to ask where these numbers $10^{-6}$ and $10^{-60}$ are coming from.

I have been appealing to the idea of a fair bet, where one trades a small stake for a potentially large gain, and a fair insurance, where one trades a small premium for protection against a potentially large loss. The trade need not be conceived in financial terms. This kind of reasoning plays an essential role in medicine (where one has to balance the debilitating effects of, say, chemotherapy against the potential gain in health), and in theoretical research (where, when choosing a project, one has to balance the labour to be expended against the intellectual value of the result, should the investigation bear fruit). Indeed, I would say that, one way or another, it plays an essential role in just about every department of life.

The conditions of human life constrain the size of any appropriate stake or premium. I am inclined to think that the significance of the numbers $10^{-6}$ and $10^{-60}$ derives from such facts as:

- The GDP of the world is $\sim 10^{16}$ US cents.
- The age of the species Homo Sapiens is $\sim 10^{13}$ seconds.
- The volume of the solar system (out to the heliopause) is $\sim 10^{46}$ cm$^3$.

An alien species, which lived for times greatly in excess of $10^{10}$ years, and whose sphere of interest extended over regions much more than $10^{10}$ light years across, might have very different ideas as to what counts as FAPP impossible.

The concept of something being impossible “for all practical purposes” is relative to the practical purposes of some particular cognitive agent. So it is, in that sense, subjective.
10. FREQUENTISM REVISITED

The analysis in Sections 5–8 shows that probability assignments cannot be reduced to factual statements about the way things are in the world. Probability statements and factual statements have a fundamentally different logic.

In particular, the proposition

\[ Q: \text{"The probability of this coin coming up heads is 0.5"} \]

is not, as finite frequentists think, FAPP equivalent to any factual proposition concerning the number of heads that will occur in a sufficiently long sequence of tosses.

Furthermore, \( Q \) cannot, as falsificationists like Popper think, be FAPP falsified by any factual proposition concerning the number of heads that will occur in a sufficiently long sequence of tosses.

The bare fact

\[ F: \text{"The coin came up heads on each of 1000 successive tosses"} \]

is no more reason for thinking that \( Q \) is false than the bare fact

\[ F': \text{"The coin came up heads on 500 out of 1000 successive tosses"} \]

Of course, if a coin did in reality come up heads on each of 1000 successive tosses, one would in practice take that as very strong evidence that the coin was biased. However, this conclusion would be based, not on the bare fact \( F \), but on the dressed fact \( P \wedge F \), where \( P \) is a prior probabilistic assumption.

We have become so habituated to the frequentist way of thinking that this assertion may appear paradoxical. But in fact the point, that \( F \) is perfectly consistent with the coin being fair, is something that is taught in every elementary textbook.

As is well-known, gamblers are prone to believe that if a coin has come up heads on (say) 2 successive tosses, then the probability of heads on the next toss is \(<1/2\). One of the first things students are taught is that this is a fallacy. Provided the tosses are independent, the probability of heads on the next toss continues to be \(1/2\), irrespective of how many times heads has come up on the preceding tosses.

This statement is an elementary consequence of the assumption of independence. It is, of course, true that in practice hardly anyone would continue to believe that a coin is fair if it kept on coming up heads in toss after toss. But that only shows that, in practice, hardly anyone would seriously believe that the tosses are independent\(^{14}\).

In practice most people would start out with the belief that the probability of heads on any given toss \(\approx 1/2\). But they would also start out with the belief that, if, for example, heads should occur on each of the first \(n\) tosses, for some large number \(n\), then that would mean that the probability of heads on the \((n+1)\text{th}\) toss \(\approx 1\). Symbolically:

\[ P(E_r) \approx 1/2 \quad \text{for all } r \quad (24) \]

and

\[ P(E_{n+1}|E_1 \wedge \cdots \wedge E_n) \approx 1 \quad (25) \]

where \(E_r\) is the event “heads on the \(r\)th toss”. That pair of propositions is inconsistent with the proposition, that successive tosses are independent.

\(^{14}\)I should say that there is an ambiguity here, if one looks at it in von Mises’s objectivist terms. As von Mises sees it, there is a true probability of heads \(p\). If we knew the value of \(p\) the probability of obtaining a sequence \(s\) would be given by the conditional distribution \(P(s|p)\) defined by Eq. 4. In that case the probability of heads on the \(n\)th toss is independent of what occurred on the preceding tosses. However, if we do not know the value of \(p\) then the probabilities have to be calculated using the unconditioned distribution \(P(s) = \int \! P(s|p)f_p(p)dp\). In that case the tosses are typically not independent —as can be seen from Eq. 24 below.
The logical basis for these intuitive assumptions is best understood by looking at the problem in Bayesian terms. From Eq. (5), the prior probability of obtaining, in the first \( n \) tosses, a sequence \( s \) containing \( h(s) \) heads is

\[
P(s) = \int_0^1 p^{h(s)}(1-p)^{n-h(s)} f_i(p)dp.
\]

If the prior distribution \( f_i \) is symmetric about the mid-point \( p = 1/2 \)

\[
P(E_r) = \int_0^1 p f_i(p)dp = 1/2 \quad \text{for all } r
\]

in agreement with Eq. (24). Suppose it is also the case that (for example) \( f_i \) is continuous and \( f_i(1) > 0 \). Then

\[
P(E_1 \land \cdots \land E_n) = \int_0^1 p^n f_i(p)dp \approx f_i(1)/(n+1)
\]

for sufficiently large \( n \). Consequently

\[
P(E_{n+1}|E_1 \land \cdots \land E_n) \approx \frac{n+1}{n+2} \approx 1
\]

for sufficiently large \( n \)—in agreement with Eq. (26).

However, the value of \( n \) at which \( P(E_{n+1}|E_1 \land \cdots \land E_n) \) becomes close to 1 will depend on the choice of \( f_i \). For some choices of \( f_i \) a comparatively short run of heads will be enough to convince us that the coin is probably biased. For other choices a much longer run will be needed. And if \( f_i = \delta(p - 0.5) \) then nothing will convince us. For that choice of \( f_i \)

\[
P(E_{n+1}|E_1 \land \cdots \land E_n) = P(E_{n+1}) = 1/2
\]

for all \( n \). So we would continue to believe that the probability of heads on the next toss is 1/2 even if (per impossibile) the coin had come up heads on each of the preceding 1000 tosses.

The distribution \( f_i \) represents our background assumptions. In other words, it represents the probabilistic context. The inference is sensitive to that context. For some contexts \( P \) the conjunction \( P \land (\bigwedge_{i=1}^{10^3} E_i) \) implies that the coin will almost certainly come up heads on the next toss. For others it implies that the probability of this happening \( \approx 1/2 \). And if one considers the fact \( \bigwedge_{i=1}^{10^3} E_i \) in isolation, devoid of any context, then no conclusion is possible. A bare fact has no (interesting) probabilistic implications.

It is the contextuality of retrodictive probabilistic inferences which defeats the frequentist programme.

Finally, let us note that on this account of the matter nothing is falsified. The proposition \( P(E_{n+1}|E_1 \land \cdots \land E_n) \approx 1 \) is built into our starting assumptions. So if, after \( E_1 \land \cdots \land E_n \) has actually occurred, we then believe that \( E_{n+1} \) is close to certain, we are not believing anything that contradicts our starting assumptions.

11. Probability is Epistemic

Before the outcome is known we consider that Alice is most unlikely to win the lottery. But then she does win. We do not conclude that we were wrong. Instead, we conclude that Alice was very lucky. So what, exactly, is meant by saying that Alice is most unlikely to win the lottery?

The frequentist strategy, of replacing a single lottery draw with a long sequence of draws, does nothing to answer that question. All it achieves is to replace the statement, that something is very unlikely, with the statement, that something else
is even more unlikely. It does not take us any nearer to understanding what it actually *means* to say that something is very unlikely.

Nevertheless, probability assignments are clearly not devoid of content. To see what their content is, consider the proposition “Alice was very lucky to win the lottery” asserted after it is known that she did in fact win the lottery. This means something like: though Alice *did* win the lottery, she could not reasonably have *expected* to win. The proposition is not unrelated to empirical facts about the lottery. But the primary focus is on Alice, and her subjective attitude.

Suppose Alice confidently asserts “I am going to win the lottery”, before the result is known. If she then wins the lottery, we have to agree that her statement was factually correct. But we do not have to agree that her confidence was justified. And that is the important sense in which one can meaningfully describe something as very unlikely, even though it has actually happened.

Compare:

**Case 1:** Alice neglects to make any pension contributions on the grounds that, when the time comes, she can always buy a lottery ticket. When she reaches the age of 65 she buys one lottery ticket, the ticket wins, and she enjoys a comfortable retirement.

**Case 2:** Bob neglects to make any pension contributions on the grounds that he has £10,000,000 invested in several large companies listed on the Stock Exchange. Shortly before he retires all the companies go bankrupt. He lives out his remaining years in penury.

Even though it is Alice’s expectations that are actually fulfilled, we would still (most of us) consider that Bob’s expectations were more reasonable.

The statement, that Alice’s behaviour is unreasonable, expresses some kind of value judgment. De Finetti would doubtless object to the word “unreasonable”. He would prefer to describe Alice’s behaviour as “crazy” (de Finetti [30], p. 175). However, he would still be expressing a negative evaluation.

In this paper I have been making the negative point: that probability statements do not reduce to purely factual statements, concerning events out there in the world. I remain very uncertain regarding the positive question, as to how probability statements should be interpreted.

However, although I have no definite opinion about many of the details, I feel that in general terms Maxwell must be right when he says that the theory of probability has an essentially normative significance. We use it to evaluate alternative beliefs and proposed courses of action: to decide what, in given circumstances, it is best or most appropriate to think and do.

12. Reality: Einsteinian or Bohrian?

For a long time Einstein strongly objected to the indeterminism of quantum mechanics. As he put it in a letter to Born [65] (p. 91), written in 1926:

> Quantum mechanics is certainly imposing. But an inner voice tells me that it is not yet the real thing. The theory says a lot, but does not really bring us any closer to the secret of the ‘old one’. I, at any rate, am convinced that He is not playing at dice.

He expressed the same view in another letter (*ibid* p. 149) written as late as 1944. I think people often find it difficult to understand why Einstein was so emphatic in his rejection of a dice-playing God. Quantum mechanics presents many obstacles to the understanding. But the concept of an objective chance seems intuitively very natural. The commonsense world is full of entities endowed with propensities...
of one kind or another. At least as judged by the standards of commonsense, if
anything is paradoxical, it is the rigid determinism of classical physics.

Einstein apparently came to feel this himself in the end. In 1954 Pauli \cite{65}
(p. 221) reports him as \textit{disputing} that he uses as criterion for the admissibility
of a theory the question: \textit{Is it rigorously deterministic?"}. However, it seems
to me that Einstein gave in too easily. There are very strong objections to the
dice-playing aspect of quantum mechanics, if one approaches it from Einstein’s
philosophical standpoint.

The commonsense world is indeed full of real chances. But it is also full of other
things, such as real colour \textit{qualia}. It has been accepted since the 17\textsuperscript{th} century
that colour \textit{qualia} are not in fact physically real at all. According to Newton \cite{66}
(p. 124–5):

\begin{quote}
\textit{\ldots the Rays to speak properly are not coloured. In them there
is nothing else than a certain Power and Disposition to stir up a
Sensation of this or that Colour}"
\end{quote}

and

\begin{quote}
\textit{\ldots Colours in the Object are nothing but a Disposition to reflect
this or that sort of Rays more copiously than the rest; in the Rays
they are nothing but their Dispositions to propagate this or that
Motion into the Sensorium, and in the Sensorium they are Sensa-
tions of those Motions under the Forms of Colours}"
\end{quote}

In short: colour \textit{qualia} are mental, not physical.

Interestingly Newton makes no attempt to justify this assertion, either on exper-
imental, or on any other grounds. Instead he proposes it as a \textit{definition}: something
we should just accept. However, I think it is worth asking why.

The obvious answer to this question is that real \textit{qualia} would not fit in with the
kind of mechanical picture which Newton is trying to construct. However, I want
to focus on a different point: namely, that perceived colour \textit{qualia} clearly depend
on physiological peculiarities of the human eye-brain system. For instance, there
are conventionally said to be 7 colours in the visible spectrum. The number 7 is,
perhaps, a little arbitrary. But I believe no one experiences, say, $10^6$ distinct \textit{qualia}.
The explanation for this must presumably lie with properties of the human eye and
brain—not with properties of the electromagnetic spectrum.

It follows that, if one were to construct a theory in which the \textit{qualia} were repre-
sented as physically real, then one would be building into one’s picture of physical
reality features which actually derive from ourselves.

Newton’s standpoint, and Einstein’s standpoint in his later years, was that the
aim of physics is to construct a a picture of things as they are intrinsically, in
themselves, without any trace of subjective contamination. From that point of
view the concept of a real colour \textit{qualium} is unacceptable.

If that is your point of view, then the concept of a real probability, or propensity,
must be equally unacceptable. According to the analysis in the preceding sections a
probability statement is a statement about what, in given circumstances, one may
reasonably think or do. This reference to our own cognitive processes means that
the concept can have no place in the purely objective world-view which Einstein
was trying to construct.

In short, it seems to me that Einstein, given his conceptual standpoint, had
every reason to reject the notion of a dice-playing God. If one chooses to follow
the Einsteinian road then one had better look for a fully causal interpretation of
quantum mechanics, such as the de Broglie-Bohm interpretation.

However, there is another possibility. One could, instead, choose to go down
what might be called the Bohrian road. Nothing with quantum mechanics built
into it could be called commonsensical. But a Bohrian view of the world would be like commonsense in as much as it would make essential reference to the cognitive agent, whose view it is. It would be colour-full and value-laden.

As it stands now the Bohrian view is, to my mind, very obscure. But it may have within it the potential for fruitful development.

Acknowledgements. I am grateful to C.A. Fuchs (who originally excited my interest in this question) and to H. Brown, P. Busch, J. Butterfield, M. Donald, L. Hardy, T. Konrad, C. Timpson and J. Uffink for stimulating discussions.

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