The Logical Status of Physical Probability Assertions

by Joseph F. Johnson

In this paper we offer a definitive solution to the major problem in that part of the philosophy of probability which is primarily logical, the question of the meaning of probability. We abstract severely from all epistemological considerations, we do not adopt a positivistic approach, and the applications to the problems of quantum mechanics are dealt with separately in another paper using the earlier approach of von Plato instead of the present approach. This approach as well as von Plato’s seems to allow one to solve the problem of scientific induction as well, this is reserved for an intended sequel. It is well known that each of the three main rival theories of the meaning of probability, the frequency theory, the Keynesian theory, and the de Finetti-Savage (‘making book’) theory, have fatal drawbacks and so there is literally no consensus among philosophers as to the status of this problem. (That decades later Kolmogoroff went off on a tangent and evolved his own idiosyncratic theory of the logical status of probability statements is beside the point.)

In particular the attempt to analyze the logical structure of either propositions asserting probability or propositions “asserted” probabilistically, has not met with the same

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1 ‘Thermodynamic Limits, Non-commutative Probability, and Quantum Entanglement’ in, Quantum Theory and Symmetries III, Cincinnati 2003, ed. by Argyres et al, Singapore, 2004, pp.133-143.

2 “Ergodic Theory,” in Skyrms, Harper, eds., Causation, Chance, and Credence, Proceedings of the Irvine Conference on Probability and Causation, 1985 vol. 1, Dordrecht, 1988 pp. 257-277.

3 For example, the frequency theory has been acutely criticised by Kolmogoroff in his contributed chapter to Aleksandrov, Kolmogorov, and Lavrent’ev, ed.s, Mathematics its content, Methods, and Meaning, 2nd ed., Moscow, 1956, we cite from a Cold War translation published in 1963 in Cambridge, Mass., p. 239, “ . . . it is clear that this procedure will never allow us to be free of the necessity, at the last stage, of referring to probabilities in the primitive imprecise sense of this term.”
success as, say, Frege and Russell’s (or Wittgenstein’s) analysis of the propositions of mathematical logic (or physical science). The aim of this paper is to show that this is because the analysis is not in the least parallel. We will propose a logical analysis of probability assertions which takes its cue from recent developments in statistical mechanics and deterministic chaos. Therefore, assertions of the probability of a state of affairs are to be unpacked as having reference to an (implicit) approximative model of reality—in contrast to normal propositions, which refer to reality directly. This view is almost forced on one if one recasts the mathematical physics of today on statistical mechanics into the framework of Russell-Whitehead and Bourbaki.

We will accomplish this by taking into account Kolmogoroff’s critique of the logical circularity of the frequency theory, otherwise the most attractive candidate. We can remove this circularity by passing to the limit, but of course we recognise that ‘limit’ is not a word with precise meaning in general, so we have to specify what kind of limit. Kolmogoroff’s critique\(^1\) can be met by the Darwin-Fowler concept of the thermodynamic limit.

The philosophy of probability has many aspects, but we choose to focus on the philosophy of science and probability within the realm of its usage in physics. This paper will concentrate on the purely logical problem of the meaning of a statement that such and such physical event’s occurrence has, had, or will have, probability \(x\) where \(x\) is a real number in between 0 and 1. We will abstract from all epistemological questions and all questions of what is the use of such a statement, except that a few remarks will imme-\[\]

\(^1\) *op. cit.*, p. 253f., he remarks about vague talk of limits, “Such definitions roughly correspond to the ‘definition’ in geometry of a point as the result of trimming down a physical body an infinite number of times, each time decreasing its diameter by a factor of 2.”
diately follow from our solution of the purely logical problem of meaning. This seems to solve the problem of induction (following Wiener’s ideas about induction and his theory of prediction).¹

The point of this paper is to completely subsume the meaning of such a statement within the Russell-Wittgenstein, Bourbaki framework of mathematical logic as applied to physical science. This is reductionist, but it works. All the usual scientific uses of the concept of probability and all the mathematical theorems about it are validated by this philosophical interpretation, obviously a desideratum of any such, and no new philosophical concepts or difficulties are raised: the fatal criticisms of frequentism are remedied.

Now I wish to emphasize that the point of this paper is to successfully complete an experiment. Can a purely deterministic, classically logical, reduction of the notion of probability be made? This requires a rather severe focus. Only immanent objections are relevant to this question, which is not exactly the same as the question of whether this account of the meaning of probability is ‘true.’ Of course it has to account for the uses made of it in science and apply to individual events, or else it is not the ‘notion of probability.’ This said, success along these lines renders the rival theories rather pointless: historically, their motivation was always the perceived failures of such experiments as this one.

Of the three main rivals, and other less important theories, as to the logical meaning of probability statements, the frequency theory is the most attractive but the most logically

¹ Wiener, *Extrapolation, Interpolation, and Smoothing of Stationary Time Series With Engineering Applications*, p. 47: “This allows us to identify averages made on the observable past of a time series with averages to be subsequently obtained from the now unattainable future . . . It is this step which constitutes the logical process of induction.”
flawed. Nevertheless, it will be our starting point since most scientists go ahead and use it without worrying about its problems, and this paper will, in a way, justify that. The frequency theory is, of course, incapable of being stated coherently without involving a logical vicious circle, as Kolmogoroff famously pointed out. I would like to call this the Rosencrantz and Guildenstern effect, after the opening scene of Tom Stoppard’s play.

Rosencrantz, or perhaps it was Guildenstern, opens the play by tossing a fair coin sixty-four, or perhaps it was sixty-five, times in a row and getting the same result—heads, or perhaps it was tails—every time (but the last). The probability that any given toss gives a given result is one-half. The probability that the play-opening tosses-event would occur is small but positive. Evidently the meaning of the statement ‘the probability that this coin yields heads is fifty per cent.’ does not imply that the frequency with which heads occurs in any finite set of trials will be anything in particular. This is a problem with the frequency theory. It is circular to say that nevertheless, it asserts that the probability that the expected frequency will be attained or departed from is what the calculus of probabilities allows us to calculate it to be, because this involves an endless regress. It is simply false, due to mathematical ignorance, to assert that in an infinite number of trials the frequency will definitely be fifty per cent., because an infinite number of trials gives us an infinite probability space, in fact a continuum of possibilities, and so events with zero probability are not impossible. It is entirely possible that every toss of an infinite sequence of tosses of a fair coin (conceding the physical meaningfulness of this for argument’s sake) will be heads. To bring up its probability is again a circular regress.

Of course there is some validity to the intuition that, ‘in the limit’ the expected fre-
frequency will be obtained and that this is what we mean when we say that the probability
that a toss will be heads is one-half. But as we saw above, it does not mean that it is
impossible that the expected frequency will fail to obtain, and further, no one has satis-
factorily defined what sort of limit. The word limit has no physical or mathematical
meaning in isolation. There are various kinds of limits, one for each context, and they
have various properties. This paper will remedy the problem identified by Kolmogoroff,
the Rosencrantz and Guildenstern effect, and will give a logically well-defined meaning to
the intuition ‘in the limit’ by introducing the logically rigorous treatment of the ther-
modynamic limit pioneered by Darwin and Fowler, and applied to Brownian motion by
Wiener and Kolmogoroff, as exposited in the book by Khintchine, *The Mathematical Foun-
dations of Statistical Mechanics*, New York, 1949. There are some similarities to a proposed
time of the meaning of probability by Professor Jan von Plato of Helsinki University,
and there are some similarities to one of the many contradictory proposals thrown out by
Aristotle in his discussions of chance and happenstance. Probability statements require
to be unpacked in a way analogous to Russell’s famous theory of descriptions, but vastly
more complicatedly, and we will argue that their logical structure is quite deeper than their
superficial grammar. This means that ‘probability,’ like ‘variable’ and ‘the’ is a linguistic
artifact or facon de parler, and the statements in which it occurs can always be unpacked
into unstraightforward but reducible determinate statements about the physical world.

It is well known from classical statistical mechanics, especially the theory of Brownian

1 Fowler, *Statistical Mechanics*, Cambridge, 1929.
2 “Ergodic Theory,” in Skyrms, Harper, eds., *Causation, Chance, and Credence. Pro-
ceedings of the Irvine Conference on Probability and Causation, 1985* vol. 1, Dordrecht,
1988 pp. 257-277.
motion\footnote{Heims, Steve, \textit{John von Neumann and Norbert Wiener: From Mathematics to the Technologies of Life and Death}, Cambridge, Mass., 1980, pp. 72-78 is a nice popularisation.} that stochastic models are useful approximations to deterministic models: indeed, the one can approximate the other to within any desired degree of approximation, so that experimentally they are indistinguishable. The logic of approximate statements, i.e., propositions that such and such a quantity is definitely inside a precise and determinate range of possibilities, is unglamourous precisely because nothing new except measure theory is introduced into philosophy or science, but it proves sufficient to answer all the puzzles about probability. Indeed, such approximate statements are simply precise propositions on the same logical footing as the usual propositions about points and exact equalities, and this has always been felt. (The dictionary definition of ‘centum’ includes such an approximate meaning quite explicitly: when, too, we talk in round numbers, this convention is understood. There is nothing new or revolutionary here and no changes in classical logic are required.) Then, too, the fact that the calculus of probabilities has already been successfully incorporated within the classical mathematical logic without any changes needed is a sort of omen that the philosophy might follow the same course.

One of Aristotle’s many suggestions about happenstance is that it arises when one deterministic system interacts with another deterministic system and there exists no overarching combined system which keeps track of the interaction terms and is deterministic. Evidently he was not familiar with Jacobi’s work on dynamics. Ever since Jacobi’s time, a revolutionary change in our attitude to combined systems had occurred. It used to be taken for granted that one could not unify all systems into an overarching dynamic. But Jacobi’s work showed that in mechanics, at least, one could practically formalise the
process. Slowly the belief in a grand unified theory has taken over the physical sciences because of the compelling vision that there will always be an interaction term which takes it all into account.

Nevertheless, there is one barrier which must remain: the interaction between an individual particle, such as the dust mote in Brownian motion, and a Hamiltonian heatbath, must be resistant to this Jacobian imperative: the heatbath is not a dynamical system at all, but is a thermodynamic limit. The barrier between the finite and the infinite clearly marks off the realm of dynamics from another realm. For two reasons: firstly, a finite system is a dynamical system in the technical sense of the word: there is a symplectic phase space as is usual in Hamiltonian mechanics, Liouville measure, invariant under the dynamical flow of states, etc. But even though physicists have various ad hoc ways of mathematically modelling infinite systems, they are not dynamical systems in the same sense. Secondly and more important from a logical point of view (since after all one could wonder whether the first, technical barrier could not be overcome by suitable generalisation of the definition of dynamical system), ‘the’ thermodynamic limit is not an object. The logically rigorous treatment of the process of ‘passing to the thermodynamic limit’ (below) shows that it is not analogous to, say, passing to the limit of a sequence of polygons and obtaining a circle. The linguistic similarity is highly misleading, although it does form the working basis for the typical sloppy undergraduate way of thinking of statistical physics. In this sense, then, this paper offers a validation of Aristotle’s insight. Probability is a linguistic phenomenon that arises when we wish to discuss the interaction, as in Brownian motion, between two systems which cannot be subsumed under a common overarching framework
of the same dynamical type as each of them separately.

The ignorance interpretation of probability has never been satisfactorily interpreted in the (above mentioned) framework of logic. Naturally enough, for it is observer dependent and not linguistically based. It is not a feature of language. The interpretation we will put forward is linguistic, although contextual: It depends not only on the explicit proposition, but a model (which is a set of propositions). The ignorance theory also does not allow for real randomness, and so is rejected by Quantum Mechanics. Our theory will allow the expression of real randomness—as an approximation to a deterministic reality. No logical theory can very satisfactorily allow real randomness—as a very matter of reference, because of the following objection.

Suppose the logic allows assertions $q$ such as ‘$p$ has probability $\frac{1}{2}$,’ $p$ being a physical event. What of the meta-language? The suggestion that there are meta-propositions about $q$ which are random is evidently absurd. But the resulting inhomogeneity between a probabilistic logic governed by a fully classical meta-logic is rather unsatisfactory. The natural conclusion would be that the stochasticness of the logic is generated by the physicality of (some of) its objects. But this would leave the logic without any role in coming to grips with the stochasticness.

For this reason, indeed, Kolmogoroff has been able to show that the mathematical structure of probability theory is completely ordinary and can be reduced to (classical) mathematical logic (say, Principia Mathematica). Of course it is understood by all that the mathematical definition of probability is merely a synonym, and not a philosophical definition of probability.
Since our proposed interpretation of probability propositions can account for the apparent stochasticness of reality, it would seem, then, that there is no need for stochasticness in the logic to be generated by physics. This justifies the superficial observation that a probability proposition must still, after all, be a certain assertion of its meaning. Before proceeding, it is important to stress that the probabilities of classical statistical mechanics do not arise out of ignorance, hidden variables, or coarse-graining and that these concepts are not mentioned at all in this paper. Hence, the meaning of probability has nothing to do with these sloppy undergraduate ways of approaching thermodynamics.

Our thesis is that the concept of probability is an artifact of a certain approximation procedure. It is thus neither linguistic (or logical) nor physical. It is only mathematical. We must begin by discussing the logical structure of statistical mechanics in a way that does not involve the notion of probability. This structure is now apparent; the belief that it can be proved vigorously in the imaginable future is current, pace von Mises, even though this is not accomplished yet.

We discuss the concrete case of classical Brownian motion, for which the rigorous results are in place. When Einstein gave such an impetus to the theory of Brownian motion, it was conceived classically, so we flesh out a Newtonian picture for our discussion. Suppose that in a closed region (with reflective walls) we have \( n \) small billiard balls and one large sphere, much larger than the balls. Call this dynamical system \( X_n \) and its phase space \( V_n \) of all possible positions and velocities of all the balls and the sphere. Then \( V_n \) is \( 6(n + 1) \)-dimensional. For a fixed total energy of \( X_n \) only a hypersurface \( E_n \subset V_n \) is relevant.
The problem is to describe the motion of the sphere, $S_n$. To make this well defined, one must specify the initial conditions of $X_n$ at time $t = 0$, call this point in $E_n$, $v_0$. Then let $X(t, v_0)$ be the resulting path of $S_n$ in the system $X_n$ with initial conditions $v_0$, regarded as a function of $t$ for fixed $v_0$. It is impractical to solve this problem exactly. Nevertheless, this is a deterministic problem, so for each $v_0$, $X(t, v_0)$ is a function of $t$. The set $\{X(t, v_0)|v_0 \in E_n\}$ is called the space of “sample paths.” It is a set of functions. Since each function has the physical interpretation of a “path,” it can be interpreted as a set of paths.

The procedure of Einstein and Wiener was rather to obtain an approximate solution to this problem, valid if $n$ is very large. Wiener constructed a different space, $W = \{X(t, \alpha)\}_{-\infty < t < \infty, 0 \leq \alpha \leq 1}$ the Wiener process. For fixed $\alpha$, $X(t, \alpha)$ is a continuous function (such that $X(0) = 0$, as can be assumed). Every such continuous function arises as an $X(t, \alpha)$ for some $\alpha$. We put Euclidean (Lebesgue) measure on the interval $0 \leq \alpha \leq 1$ and transfer this to $W$, so $W$ becomes a probability space in the sense of Kolmogoroff. (Wiener showed that as such, the $X(t, \alpha)$ satisfied the properties of Einstein’s model—Einstein had treated it postulationally, not constructively.)

The intuitive way people think about this is (illogical—we will soon replace it with a rather different way) as follows. The $\alpha$ parameter is thought of a analogous to $v_0$. The $W$ is the limit of $V_n$ as $n$ approaches infinity (‘in the thermodynamic limit’—i.e., the energy and pressure are held constant so the mass of each billiard ball shrinks proportionately). For each $v_0$, $X(t, v_0)$ is differentiable almost everywhere in $t$. (I.e., everywhere except at collisions with the balls.) But as we imagine the billiard balls to get smaller but more
numerous, the collisions are more frequent. So, corners or kinks (of non-differentiability) in the path become more frequent. In the limit, they are almost everywhere. Thus, almost all the $X(t, \alpha)$ are (continuous but) non-differentiable almost everywhere in $t$.

Now in the limit-model, the stochastic model, physicists tend to think intuitively of each sample path (i.e., each $X(t, \alpha)$ for fixed $\alpha$) as the motion of a sphere under “random impacts” from the surrounding medium—this was Einstein’s view.

The thesis of this paper is rather that the probabilistic aspects are purely artifacts of $W$. Now $W$ only has validity as a mathematical approximation device for studying certain aspects of $V_n$ when $n$ if very large.\footnote{Cf. Wiener, “Logique, Probabilite et Methode Des Sciences Physiques,” Toutes les lois de probabilite connues sont de caractere asymptotique . . . les considerations asymptotiques n’ont d’autre but dans la Science que de permettre de connaitre les proprietes des ensembles tres nombreux en evitant de voir ces proprietes s’évanourir dans la confusion resultant de las specificite de leur infinitude. L’infini permet ainsi de considere des nombres tres grands sans avoir a tenir compte du fait que ce sont des entites distinctes.” That is, it is a method, not an object.} It is only $V_n$ that is a positivistic model of physical reality in the sense that there is a correspondence between objects of $V_n$ and physical objects, between relations (or structures, such as functional dependence) in $V_n$ (between objects of $V_n$) and physical relations between physical objects. The model $W$ is only about reality indirectly: it is ‘about’ $V_n$ which is about reality. (Strictly speaking, it is not even about $V_n$: arguably, it is not really a model, since it is not about anything.)

Some questions that can be asked about $V_n$ can be approximately answered by studying $W$—the physical question of Einstein’s original series of papers, for example. Other features of $V_n$ are not preserved by the limiting process and so are not present in $W$. For example, $V_n$ satisfies Poincare recurrence for each $n$. But $W$ does not. A rigorous treatment of this approximation procedure is still lacking in general statistical mechanics. It
has been done by Ford-Kac-Mazur\(^1\) (although not exactly for billiard balls and a sphere, but rather for phonons and a harmonic oscillator), although a complete characterisation of all those properties of \(V_n\) which smoothly approach a limit property of \(W\) is still lacking. But enough has been done to render unlikely von Mises’s flat statement that a justification of statistical mechanics is inconceivable.

The following is in its logical, modellic structure, essentially due to Darwin and Fowler. The details of their proofs were simplified by Kolmogoroff and, independently, Wiener, and exposited by Khinchine (\textit{loc. cit.})

Let \(P\) be a propositional function of models such that for all \(n\), \(V_n\) is in its domain. But \(W\) need not be. By saying that \(P\) can be approximately answered by a study of \(W\) we mean that \(P\) is of the form \(|f| = 0\) where \(f\) is a function of \(V_n\) with values in a metric space (or, for simplicity, we may assume \(f\) is a numerical function) and the proposition \(Q\), “for every \(\varepsilon > 0\) there exists \(N\) such that for all \(n > N\) we have \(|f(V_n)| < \varepsilon\)” is equivalent to a proposition \(Q’\) about \(W\). (The words “study” and “about” are vague, and correlatively so. The original statement, “\(P\) can be approximately answered by a study of \(W\)” is not a formal proposition. If, however, \(V_n\) is some sort of function of an additional parameter, say \(H\), and so is \(W\), then we can ask, for example, that for \(P\) independent of \(H\), \(Q’\) should be also. Similarly if \(P\) depends on \(H\) only through algebraic operations on \(f\), etc. These are examples of how to make “study” and “about” have a precise meaning.)

For example, we could take \(P\) to be “The average motion of the sphere after ten seconds (at a fixed energy, etc.) where the average is taken with respect to a (given)
weighting of initial conditions \( v_0 \in E \), is \( \sqrt{10} \).” Then \( Q' \) is “\( \int_0^1 X(10, \alpha) w(\alpha) \, d\alpha \) is \( \sqrt{10} \)” where \( w \) depends on the weighting mentioned in \( P \). We can also let \( E \) and the relative sizes of the balls and sphere, etc., vary and obtain families of \( P \) and \( Q' \).

Note that the intuitive view of \( X(t, \alpha) \) cannot be formalized: Since the Wiener process is not in fact a dynamical system. Admittedly, \( X(t, \alpha) \) satisfies a so-called stochastic differential equation. But an analysis of the linguistic structure of this reveals its utter lack of parallelism to \( V_n \), or any \( X_n(t, v_0) \). This follows: for fixed \( t \), \( X_t(\alpha) = X(t, \alpha) \) is obviously a function of \( \alpha \) (with values in a three dimensional space of vectors). And \( \alpha \) has as its domain a probability space. In fact, \( X_t \) is what is called a random variable. The definition of a stochastic differential equation is far removed from that of a differential equation, but even granting part of the intuitive view (which we need not), which follows, we still will carry our point.

An ordinary differential equation is thought of as a way of relating a variable at one point in time to a variable at a neighboring point in time (by means of a limiting process)—similarly for stochastic differential equations, in particular the one which the Wiener process \( \{X_t\}_{-\infty < t < \infty} \) satisfies. (But this parallel is specious only to those who are ignoring the fact that a so-called “random variable” is not a variable. A random variable is an object, but a variable is a linguistic construction (hence it could be an object only for the meta-language). In particular, a random variable is a measurable function from a probability space to the real numbers. A random variable can have properties, and a pair of them can stand in relation to each other—e.g. we can ask if \( X_t \) and \( X_{t+1} \) are independent or not—which is absurd for variables.)
Thus, for each instant in time $t$ we have that the position of a sphere is governed by a random variable, $X_{t_0}$, which evolves from the previous random variables $\{X_t\}_{-\infty < t < t_0}$ by a law analogous (seemingly) to a law of motion. That it is impossible to consistently adopt this intuitive interpretation is manifest now, since this is incompatible with that part of the intuitive interpretation which we exposited previously, viz., that for fixed $\alpha$, the sample path $\{X(t, \alpha)\}_{-\infty < t < \infty}$ represents the motion of the sphere under “random impacts.” In the one case, we view $X(t, \alpha)$ with variable (unsaturated), in its dependence on $t$, $\{X_t\}$. That is, we fill in $t$ first (saturated). In the other case, that of viewing the sample path, we fill in $\alpha$ first, and then $t$. These are incompatible. In particular, the sample path is not a solution to the stochastic differential equation: e.g. every continuous function (satisfying the initial condition $X(0) = 0$) is a sample path. So the stochastic differential equation imposes no conditions at all on the sample paths.

In $V_n$, au contraire, $X(t, v_0)$ is a solution to the equations of motion no matter whether $v_0$ is fixed or variable. The popular fallacy regarding the Wiener process, exposed above, has probably arisen through two causes: Firstly, the false parallelism between random variables and variables, (most physicists have never learned the Peano-Frege-Russell analysis of variables and quantifiers and still think, with Euler and Cauchy, that there are two types of real numbers: constant ones, and variable ones), and secondly, an unjustified carryover of the interpretation of $V_n$ to the model $W$.

In a sense, of course, the above critique of the naive interpretation of a stochastic process is essentially the same as the critique of the naive interpretation of a random variable $Y(\alpha)$. Naively, one thinks of the $Y(\alpha)$ as describing an outcome, its value, that
is random, i.e. not determined by anything. But of course the linguistic analysis of $Y(\alpha)$ does not bear this out: its value is a function of $\alpha$, so after all it is deterministic.

But as long as there was no concrete issue at stake, this critique was never convincing. The Wiener process is “quite possibly the single most important object in all of modern probability theory,”\(^1\) but also typical of statistical mechanics. The concrete issues discussed above can be regarded, then, as decisive objections to the naive analysis of a random variable as well as of probabilistic statements.

Some such view as advanced in this paper is necessary in order to make sense of von Mises’s ensemble or frequency interpretation of probability statements. Von Mises himself, in referring a probability statement (about a single event), for its meaning, to an (implicit) statement about an ensemble, is using a structurally similar ‘unpacking’ style of analysis as we have already advanced. To make his notion rigorous, it is of course necessary to include an infinite ensemble and a limiting process (as well as a measure), as he practically does. The only step missing from his analysis, which we supply, is an explicit recognition of the infinite ensemble (limit)—which is unphysical—as a mathematical approximation device for making calculations (approximate ones) about a fixed finite stage in the limiting process. This amendment to von Mises is necessary if one wants to insist that meaningful propositions are about reality, as long as one thinks that reality is necessarily finite. That is, talk of potential trials is not able to fit into the framework of Russelian logic. But on the other hand, in practice we see that the actual trials are always a finite ensemble—even if not always, our theory has to cover the vast majority of cases where they are finite. (Naturally, I am not denying that infinite sets are not used in Russelian logic—

\(^1\) Stroock, *Probability Theory From an Analytic Point of View*, Cambridge, 1993, p. xi.
but they are sets of actual objects, never of potential objects. This is, in fact, just the same difficulties of random variables all over again. If anyone tried to invent a new logic with a new kind of variable, an $r$-variable which in the interpretation was “random” but linguistically was a variable, how could they talk about an infinite sequence of independent identically distributed trials? E.g., a Bernoulli process? Objects are distinct because they have different properties. Random variables are discovered independent—or not, as the case may be—by comparing their structure as functions of $\alpha$.)

There is another possible objection that can be made. There is a widespread misimpression that the success of Quantum Physics has definitively established stochasticity as a fundamental feature of science, so that it is perverse to try to explicitly define the notion of probability in terms of classical logic and classical, deterministic, philosophy of science. This is not so. There has been a recent flourishing of work on the problem of Quantum Measurement and the old positivistic consensus has completely broken down. Advances in Deterministic Chaos and the Gibbs program of statistical mechanics, even in the quantum context, have opened up the question again, even though hidden variables is not going to be accepted as the answer. For this reason, it is useful to try the experiment of trying to account for the probabilities that arise in classical thermodynamics etc. in a logically careful way. This clears the ground for a re-examination of Einstein’s question, whether the probabilities of Quantum Mechanics arise in the same sort of way from Schroedinger’s equation as the probabilities of classical mechanics arise from the Hamilton-Jacobi equations. Because Einstein himself and most physicists have had a philosophically inadequate definition of probability and a sloppy undergraduate understanding of the logical structure
of statistical mechanics. A logically careful account of the classical statistical mechanics was first adumbrated by Darwin and Fowler in the 1920’s and never penetrated into the discourse. A logically unexceptionable reductionist account of probability is the accomplishment of this paper. This experiment will put Einstein’s question in a completely new light. Since we do not use ignorance, hidden variables, or coarse-graining at all in our analytical ‘unpacking’ of the structure of probability assertions, the traditional understanding of the issues posed by Einstein must be wrong.

Now our analysis of Brownian motion shows the stochastic aspects can arise as an artifact of the passage from $V_n$ to $W$. Hence so, then our claim would be practically justified. That is, assertions as to the probability of an individual event have, not a physical, but a linguistic meaning. On analysis, they ‘unpack’ as asserting that such and such an approximation technique yields such and such an answer, and the approximation is valid—an assertion which may sometimes fall short of the truth—but only for exactly the same reasons as other approximations break down, and not for any reasons especially peculiar to the concept of probability. (In particular the failure would not be “random.”)

Our thesis is related to the analysis of counterfactual conditionals. On my view, a counterfactual conditional is meaningless without a (more or less implicit, in practice) supplementary dynamical theory of causality, which allows of varied initial conditions ($v_0$). To assert the conditional is to assert that from $v_0$, the dynamics lead, in theory, to the consequence.

Our thesis accounts for the probabilistic statements of ordinary language if such implicit theoretical structures can be taken to be part of the shared discourse of the speech-
act-community in which the statements are made. (If the theories are false, we may as well suppose the statements are false.) If more than one possible theory could be so regarded, we have just the typical ambiguities of ordinary language, nothing more.

I should anticipate one plausible objection before concluding. With reference to statistical physics, the validity of the approximation techniques relies (so far) on the ergodic theorem. But, letting $f$ represent a dynamic variable or observable of the system, the ergodic theorem only establishes the validity of the technique for almost all $f$, i.e. all $f$ except for a set of measure zero. Does this instantiate a reliance on a concept of probability which does not fall under our paradigm, but rather the popular one? The popular one would, that is, interpret “almost all” as meaning “probability one,” thus one is practically certain that the approximation is valid for the particular observable $f$ under consideration.

It need not. Firstly, the measure space of all $f$ does not, generally, have measure one—so, technically, it can not be a probability space. Secondly, even if this technical obstacle could be overcome, $f$ can not be the argument of a random variable. The objection envisions “validity” as the value of a random variable whose argument would be $f$. But $f$ is chosen explicitly by the scientist, so this is clearly not an appropriate model.

Let $C$ be the (algebra) of all $f$. Let $A$ be the aforesaid set of measure zero and $B = C \setminus A$. The situation is rather that at present there is no way to tell whether $f \in B$ except to calculate the predictions of the approximation, perform the experiment, and compare the two. This is not an unusual situation in science and so we are not tempted to invoke the popular notion of probability to explain it.

Another misunderstanding to be cleared up, one fostered by the spate of books on
chaos theory, is that the probabilistic nature enters into the models $V_n$ prior to passage to the limit. But this is not true. The probabilistic assertions require use of thermodynamic functions such as temperature—now these are only defined in the limit. Then, too, physicists, especially those working on the $C^*$-algebra approach to statistical fallaciously pass over the distinction between mixed states and states. Not only is this distinction absolutely crucial from a Russell-Wittgenstein point of view, their blurring it is completely gratuitous when the ergodic theorem is available. For in this case, “phase averages” can be simply a technical device and need not be given any special physical interpretation.

Now it is time to point out one of the advantages of introducing the passage to the thermodynamic limit. We need not in fact assume that the dynamics is ergodic. As Khinchine shows, the full strength of the ergodic theorem is not necessary in the thermodynamic limit. That is, there will be a wide class of functions $f$ (only, not necessarily almost all) for which the phase average is a better and better approximation of the time average, as one proceeds to the thermodynamic limit. We can obviously interpret ‘probability’ in this context, mutatis mutandis.

In conclusion, we summarize and exemplify the theses of this paper. We assume it is well known that popular, traditional treatments of probability, such as reichenbach’s, von Mises’s or the more implicit (and inchoate) subtext of most physics papers, are linguistically incoherent and essentially irreconcilable with a Russell-Wittgenstein treatment of logic. (Either because of the Rosencrantz and Guildenstern effect, or because they rely on probability as a primitive notion.)

But the frequency or ensemble theory itself shows (not unequivocally), and a careful

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1 Khinchine, *op. cit.*, New York, 1949, p. 62f.
linguistically-corrected recapitulation of classical statistical mechanics shows (much more clearly)—that probability statements arise as features peculiar to a (concrete) approximation device, without a physical objective correlative. Thus “probability” has no (direct) correlative.

For example, suppose $V_N$ is a classical dynamical system, and $f$ an observable. The object is to calculate the time average of $f$ over a long but finite time interval. Instead of this, we calculate the infinite length time average $\langle f \rangle$. We hope that this will be a good approximation to our real object. The dynamics of $V_N$ determines the measure we will use implicitly in what follows, so we do not, in any sense, interpret the invariant measure $d\mu$ on phase space as a probability. We then hope that the initial conditions, $v_0$, of the actual system studied and $f$ are such that the ergodic theorem applies, saying $\langle f \rangle$ is equal to the phase average of $f$, $\int f \, d\mu$.

But we cannot calculate $\int f \, d\mu$, so we use an approximate technique. We embed $V_N$ and $f$ into a sequence of dynamical systems $V_n$ and observables $f_n$, possessing a thermodynamic limit $W$ (and $f_W$). (Note that $v_0$ has disappeared.) This has only a logical status. We cannot interpret $W$ physically, or intuitively as a limit of the $V_n$. Firstly, it is not a dynamic system at all. Secondly, although there is a sense in which $f$ can be made variable with $n$, there is no sense in which $v_0$ can be. Now since $W$ is stochastic whereas $V_n$ are deterministic, this is where probabilities enter into the activity of the scientist, the model-builder and analyst.

Then it is a fact that $\int f_n \, d\mu_n$ converges to a limit which can be calculated by means of $W$. As explained in the introduction to Ford-Kac-Mazur, the classical program of
statistical mechanics ever since Gibbs has been to establish this fact as a theorem, as is happening in a growing number of special cases, such as those by Ford-Kac-Mazur, Hudson-Parthasarathy, Lewis-Maassen,\(^1\) and many others.

The thesis of this paper is that the analytic interpretation, of unpacking, of these probability statements is preferable to the naive alternative, to mythologically attribute physical reality to each and every mathematical manipulation.

A theory of the meaning of probability statements with many similarities to the present one was put forth by Jan von Plato,\(^2\) but for some reason not included in his *Creating Modern Probability*, Cambridge, 1994. The main difference, unfortunately for philosophical purposes this is crucial, is that he defines probability as meaning the time average of a functional. Whereas we, following the explicit statement of Wiener\(^3\) (who was a logician),

\(^1\) Lewis, Maassen, “Hamiltonian Models of Classical and Quantum Stochastic Processes,” *Lecture Notes in Mathematics*, Berlin, 1984, vol. 1055, pp. 247-276

\(^2\) *op. cit.*

\(^3\) Masani, Wiener, “Non-linear Prediction,” in *Probability and Statistics, The Harald Cramer Volume*, ed. U. Grenander, Stockholm, 1959, p. 197: “As indicated by von Neumann … in measuring a macroscopic quantity \(x\) associated with a physical or biological mechanism … each reading of \(x\) is actually the average over a time-interval \(T\) which may appear short from a macroscopic viewpoint, but it is large microscopically speaking. That the limit \(\overline{x}\) as \(T \to \infty\), of such an average exists, and in ergodic cases is independent of the microscopic state, is the content of the continuous-parameter \(L^2\)-Ergodic Theorem. The error involved in practice in not taking the limit is naturally to be construed as a statistical dispersion centered about \(\overline{x}\).” Cf. also Khintchine, *op. cit.*, p. 44f., “an observation which gives the measurement of a physical quantity is performed not instantaneously, but requires a certain interval of time which, no matter how small it appears to us, would, as a rule, be very large from the point of view of an observer who watches the evolution of our physical system. … Thus we will have to compare experimental data … with time averages taken over very large intervals of time.” And not the instantaneous value or instantaneous state. Wiener, as quoted in Heims, *op. cit.*, p. 138f., “every observation … takes some finite time, thereby introducing uncertainty.” The finiteness operates in two distinct ways. Since it is not instantaneous, it introduces time-averages. This brings us into the realm of the ergodic theorem, statistical mechanics, and hence probability. Since it is not infinite, there is some error involved in replacing it by infinite-duration time averages. And Benatti, *Deterministic Chaos in Infinite Quantum Systems*, Berlin, 1993, “Trieste Notes in Physics,” p. 3, “Since characteristic times of measuring processes on macrosystems are greatly longer than those governing the underlying micro-phenomena, it is reasonable to think of the results of a measuring procedure as of time-averages evaluated along phase-
interpret the time average of a functional as ‘measurement.’ Professor von Plato attributes his definition to Einstein but this must be a mistake. Einstein’s statement to that effect in his early papers is not to be taken literally as a piece of philosophy, it is evidently merely a physicist’s Ansatz in order to fudge various irrelevant difficulties and make room for progress on one difficulty at a time. Einstein was not a logician, and his path-breaking work on statistical mechanics preceded by several decades Darwin’s rigourous solution of the difficulties which Einstein wished to push to one side by means of this Ansatz.

Finally we remark that the structural ‘jumps’ in our unpacking correspond very well to the naive scientist’s idea of the meaning of probability, and this is evidence in favour of our proposal. For example, Kolmogoroff, loc. cit., analysed the frequency theory of probability as being of rough practical value nevertheless, in spite of the fact that it could not give a reduction of the concept of probability to other concepts. The first feature of this to be noted is that the frequency “predicted” is merely asserted to be close to what will happen, where close is a vague notion. In our structural unpacking, this corresponds to two features: firstly, measurements are in practice always finite, whereas the ergodic theorem only holds exactly for infinite time averages. Modelling measurements by very large time averages is exact, but the approximation procedure replaces them by infinite time averages, and this introduces as an approximative vagueness the vagueness which Kolmogoroff thought must be due to the primitive, undefinability of probability. Wiener has also remarked explicitly on the importance of the fact that all our measurements or observations are of finite time intervals, not infinite ones, and the unavoidable imprecision trajectories corresponding to given initial conditions.” (underscoring in the original). And Pauli, *Pauli Lectures on Physics, volume 4, Statistical Mechanics*, Cambridge, Mass., 1973, p. 28f., “What is observed macroscopically are time averages . . . ”
this introduces into our predictions. But our proposal makes it clear that this vagueness or imprecision is not a logical one, but merely of the same epistemological category, whatever that is, of traditional approximate measurements in classical mechanics. Secondly, the ergodic theorem holds only for almost all initial conditions. There is an exceptional set of measure zero. Now for finite observations, time-averages over finite time intervals, the set of initial conditions or trajectories for which the observed value differs appreciably from the phase average (predicted value) is actually of positive measure: it shrinks to measure zero only in the limit of infinite time averages. So this is the second source of imprecision: the exceptional set has positive measure. Our proposal removes this imprecision from the philosophical realm of probability because of the extra layer of indirection we have added, when compared to the naive frequency theory. The statement that the probability of observing the event is $x$ remains true even when the actual observation turns out to belong to or be close to the exceptional set and so leads to a measurement that differs appreciably from the probabilistic expectation. Because, according to our proposal, the statement was not directly about the observation, but was about the modelling procedure leading us to predict the expectation, and was thus about a whole statistical ensemble of counter-factual conditionals besides the actual realised observation. It thus remains true no matter which conditional is realised.

The second roughness in the naive view has to do with the insufficiently formalised notion of what is meant physically by saying that the trials are independent. This corresponds to the following fold in our unpacking proposal: we interpret the making of a probability assertion as implicitly making assertions that there is an underlying dynamics to determine the event and that this dynamics has various properties (either ergodicity,
which, by the way, is usual in quantum dynamics and easier to establish there than classically, or some sort of large-number condition which is sufficient for the truth of the result of the ergodic theorem even when ergodicity is lacking).

Finally, we point out that the third roughness in the usual view, again, rather acutely formulated by Kolmogoroff, is that even if one sneaky observer somehow gathered enough initial conditions data about the coin toss to be able to predict it, we would somehow want to still say that its probability was objectively or subjectively one-half when the event occurs in the context of a specification or description that leaves this extra knowledge out of account. This is accomplished by the proposal of this paper in the same way as the explanation of the meaning of independence of trials, above. That is, the probability assertion is taken by us as implicitly including a specification of the entire phase space of the system, including its Liouville measure, indeed, the phase spaces of each member $V_n$ of an infinite sequence of dynamical systems. The statement then being about this sequence, the meaning is left intact even if we in fact all knew the exact initial condition $v_0$ which obtained. The meaning is clear-cut and logical, its connection with observations and scientific practice is formulatable in precise terms, and yet the imprecisions we are used to are preserved in the harmless form which they have in many other places in classical epistemology, quite apart from notions of probability.

**Bibliography**

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