The Problem of Quantum Measurement

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Abstract

We derive the probabilities of measurement results from Schroedinger’s equation plus a definition of macroscopic as a particular kind of thermodynamic limit. Bohr’s insight that a measurement apparatus must be classical in nature and classically describable is made precise in a mathematical sense analogous to the procedures of classical statistical mechanics and the study of Hamiltonian heat baths.

Quantum Measurement as Thermodynamic Limit

It is not necessary to modify the axioms of non-relativistic Quantum Mechanics in order to solve the puzzle of Quantum Measurement. In order to do so, all we need to do is to take Schroedinger’s equation without any modifications as basic, imitate the procedures of classical Statistical Mechanics, and use an explicit Hamiltonian dynamics that models the amplification process. No measurement apparatus dispenses with some form of amplification, and it should not be a surprise that the key to the problem is to use the physics of real measurement apparatuses.

Introductory remarks

Quantum Mechanics has various axioms, say about six or so. Three of them are logically of the same structure as the formulation of Hamiltonian mechanics, and are deterministic. Famously, the time-evolution of a quantum system, as long as it is unobserved, is governed by the wave equation of Schroedinger, which is in its mathematical structure, a deterministic equation. It describes a deterministic dynamics. But during the measurement process, a stochastic dynamics supervenes, and only probability laws describe the result of a measurement and the state of a particle as it escapes the measurement apparatus. The problem of Quantum Measurement can be roughly stated as does Bell in a
famous paper, “Against Measurement,” as well as other papers, where do we put the cut in Nature that describes when one dynamical law rules, and when the other law rules?

There have been, perhaps, three main obstacles to solving the problem. Only one has been purely physical. That one is the lack of writing down an explicit, time-independent, matrix Hamiltonian which, even in a toy model, captures the properties of the amplification process which are physically relevant to measurement apparatuses. This is achieved for perhaps the first time in this paper. The other two are merely formal: as experts in the philosophy of science know, there has been heretofore no widely accepted definition of the concept of “probability” as it occurs in science, and therefore as it occurs in the axioms of quantum measurement. We adopt one very close to Jan von Plato’s, and it is a powerful argument in its favour that it, originally advanced purely in the context of classical mechanics, works just as well in this new setting. It is close to the usual working but inadequate frequency theory of probability, which again should be no surprise, since the frequency theory of probability has worked well in science in spite of its logical circularity (a circularity fixed by von Plato’s adjustments).

The last obstacle has been common misconceptions about the logical structure of classical Statistical Mechanics, but fortunately there are in some standard texts and standard and influential papers an adequate discussion of this so we need only imitate the procedure of, for example, the celebrated paper of Ford–Kac–Mazur. Each of these philosophical points will also be treated in detail in the appropriate section of this paper.

A more careful examination of the logic of Quantum Mechanics and how the idea of measurement could be rigourously defined within it shows, as we will see, that the crucial
missing ingredient is a rigorous definition of “macroscopic.” This seems not to have been even clearly noticed before. Imagine, as usual, that the amplifying apparatus doing the measuring registers the result of the measurement by having a needle move along a dial, pointing in a macroscopically visible fashion to different numbers. If we formulate our task, as it has often been formulated, to be that of deriving the quantum-mechanical measurement postulates from Schrödinger’s equation as applied to the joint system of microscopic particle being measured, and amplifying apparatus measuring it, it has always been said that the apparatus has various possible states corresponding to different positions of, say, its needle in the gauge. These visibly different numbers or positions of the needle are called, “pointer positions.” The dynamic variable of the apparatus that discriminates between different pointer positions is called a (or the) “pointer variable.” It seems not to have been realised that it was a wide open question what sort of mathematical object should model such pointer variables, or what was the physical basis for making assumptions about their properties. Our view is that no ad hoc assumptions are allowable, we must give a rigorous definition of pointer variable in terms of the basic notions of quantum mechanics and derive from Schrödinger’s equation alone whatever properties of pointer variable we wish to rely on—always in a way according with experimental results and physical intuition, of course. This is the main, or even only, novelty in this paper. Every other idea has appeared in print before, only not combined with the other ones, or even unfortunately in combination with unsuitable ideas on the other ingredients.

Einstein had already suggested that the probabilities of quantum mechanics arose from a fundamental deterministic dynamics in analogy with the way the probabilities of classi-
cal statistical mechanics arose from Newtonian mechanics. Bohr already suggested that measurement apparatuses and pointer positions were classical objects, not quantum, and classically describable. Schroedinger had already suggested that Schroedinger’s equation would not need to be replaced by a different deterministic dynamics. Daneri–Prosperi–Loinger had already suggested that amplification by a macroscopic device in a meta-stable state was physically key to measurement, and an ergodic principle of some sort was at work. H.S. Green had already suggested that a device in a state of negative temperature was an appropriate model for the measurement apparatus. Schwinger had already suggested that a negative temperature Brownian motion would amplify quantum motion to a macroscopic level of classical motion in which the quantum uncertainties would be negligible (although this was not in the context of quantum measurement).

But, for example, Einstein’s idea may have been irrelevantly tied up with the idea that hidden variables were essential to classical statistical mechanics, an idea later refuted by Darwin and Fowler. Green’s treatment of measurement involved postulating a probability distribution on the measurement apparatus, which is an unduly naïve way to ground Statistical Mechanics. The Coleman–Hepp model of measurement both lacks the notion of amplification and imports the techniques of Quantum Statistical Mechanics. (The ideas of Quantum Statistical Mechanics are physically inappropriate to the problem of Quantum Measurement if Bohr’s insight that measurement apparatus are classical in nature is correct. Because the thermodynamic limit taken in Quantum Statistical Mechanics is still a quantum system, not a classical system.) Bohr’s suggestion was usually phrased in terms of accepting a dualism in physics or even the ‘cut’ in Nature Bell complained about. He
failed to connect it with a thermodynamic limit, any definite dynamical content, or a precisely defined approximation procedure. He once more carefully specified what he meant by saying that the measurement apparatus in classical. He meant that it could be usefully described in an approximation in which Planck’s constant was neglected.

Precise statement of the problem

It is helpful to focus on Wigner’s formulation of the problem, rather than Bell’s. To do this, we state the six axioms of Quantum Mechanics in Dirac’s formulation—assuming, as usual, for simplicity, that only discrete eigenvalues with multiplicity one occur, etc. etc. The first three axioms are the same as those of Hamiltonian Mechanics, with only technical mathematical differences. The first one is that each closed physical system is described by a Hilbert space and a Hamiltonian operator on that space, which is characteristic of that system. That is, to each system is associated a complex separable Hilbert space $\mathcal{H}$ and a skew-adjoint operator $H$ defined on a dense subspace. These mathematical objects give us all the physical information about that system. The second axiom is that the possible physical states of the system are given by non-zero elements $\psi$ of $\mathcal{H}$, called wave-functions, and if and only if $\psi_1 = c\psi_2$ with $0 \neq c \in \mathbb{C}$ do they describe the same physical state.

The third axiom says that if the system is in the state $\psi_o$ at time $t = 0$ then it will be in the state

$$\psi_t = e^{it\frac{\mathcal{H}}{\hbar}} \cdot \psi_o$$

at time $t$, where $\hbar$ is Planck’s constant, roughly $9 \times 10^{-37}$ hp-sec$^2$.

If Quantum Mechanics were really the same as Classical Mechanics, this would be all the axioms needed. One would operationalise somehow the lab procedures needed to
measure the various $\psi$ of various types of physical systems. But instead of this, Quantum Mechanics introduces two new undefined, primitive concepts into the axioms. In Classical Mechanics, what one measured were the states, directly. This is no longer true, one now introduces a new undefined concept, supplementing that of physical state. One also makes measurement a specific concept, although primitive and undefined, it is specifically different from other physical processes.

There are three axioms about the measurement process. The first one is that to every possible measurement process, there corresponds a self-adjoint operator $Q$ such that its eigenvectors, or eigenstates, form an orthonormal basis of $\mathcal{H}$. Let the orthonormal basis be $\{\psi_i\}$ and let the associated eigenvalues be $\lambda_i$ so that we have $Q\psi_i = \lambda_i \psi_i$. The only possible results of the measurement process are the eigenvalues, $\lambda_i$. The next axiom states that if the system being measured is in the state $\psi$ and if the Fourier decomposition of $\psi$ is given by

$$\psi = \sum_i c_i \psi_i$$

then the probability that the result of the measurement process will be $\lambda_i$ is $|c_i|^2$ if we assume, as we may after an inessential normalisation factor is inserted, that $||\psi||^2 = 1$ or, as we say, that $\psi$ is normalised.

The last is the reduction of the wave packet: it states that after the measurement process is over, the system being measured is in the physical state $\psi_i$ corresponding to the eigenvalue $\lambda_i$ which was observed.

(Dirac’s original line of argument for it was based on reasoning using the principle of continuity, Most physicists have given up interpreting this axiom literally. For this reason,
we reserve discussion of this axiom for a projected sequel.)

Although many discussions of the problem of Quantum Measurement have focussed on this last axiom, Wigner’s influential discussion does not. Most discussions or questions that occur to the beginner about the problem of Quantum Measurement involve hidden assumptions, often of a philosophical nature, in addition to the axioms. For example, by now it has been realised that although the process is called ‘measurement,’ it must not be assumed that there is, physically, some ‘value’ which is being measured in the sense that it is pre-existing.

J.S. Bell has influentially intervened in this project several times. For us, the two most relevant times are in his comments on the Coleman–Hepp model and, by extension, most statistical approaches. And in one of his last articles, ‘Against Measurement’ in which he critiques the standard approaches even more forcefully.

For us, his critiques come down to three points: The orthodox approach is not physically precise if it cannot give a physical definition of measurement apparatus in terms of wave functions and Hilbert Spaces and Hamiltonian operators. (It should not be left to the experience and tact of the theoretician.) The theory, in order to even *be* a theory, must also be capable of being logically precise: assumptions must be distinguished from theorems, and primitive undefined concepts must be carefully laid out at the start, with all further concepts defined in terms of them. And the theory should be ‘about’ reality, all of reality, including the universe as a whole with all measurement apparatuses included in it.

(In particular about being logically precise, Bell makes two points which we will
answer: the first one he phrases in terms of a cut in the world, although we will follow
Wigner’s phrasing of this problem, below, instead. That is, Bell points out that the usual
way of using the axioms of Quantum Mechanics involves introducing a cut in Nature, on
one side of the boundary, we apply the first three axioms, and on the other side of the
boundary, we apply the second three axioms. Bell claims that this is not logically precise
until the position of the cut in Nature is rigourously specified by its own axiom, specified
once for all and in advance . . . we prefer Wigner’s formulation of the illogicality involved,
which Wigner calls a dualism instead of a cut. The second point is that the measurement
processes should be either defined, or primitive, but not both . . . )

Notice that the orthodox interpretation of the wave function’s *values* at a particu-
lar point (as being related to the probability of finding the particle at that point in space
or whatever) is *not* part of the axioms. Indeed, when Born first introduced this in-
terpretation, it was not immediately accepted: the founders of Quantum Mechanics had
been working successfully with the axioms alone and without any interpretation of the
wave function except what we have given here, that it represents the physical state of the
system. Taking Bell’s point seriously, one must decide whether the Born interpretation
is an interpretation or a theorem. It is known that it follows from the measurement ax-
ioms, so if we succeed in showing that the measurement axioms follow as approximations
from the first three axioms, we will have clarified the status of the Born interpretation as
well, showing that if it is not assumed, nevertheless some approximation to it follows as a
consequence of the usual procedures of Classical Statistical Mechanics.

Wigner’s discussion has the merit of isolating a purely logical question, which has
physical significance, and no excess philosophical baggage. We will, in recalling his discus-

Consider the physical situation underlying a measurement process which obeys these
axioms. There is a ‘microscopic’ system, or ‘incoming particle’—we use these terms purely
for mnemonic convenience, for us they have no conceptual significance. In practice, the
system being measured is usually a photon or an electron approaching the apparatus from
the left, say, and if it were a closed system not interacting with the apparatus, it would
be thought of as a particle described by a wave function of its own. If, then, it were a
closed system, by axioms 1-3 it would be described by a wave function $\psi_o$ in a Hilbert
Space $\mathcal{H}_o$ with an intrinsic, time-independent Hamiltonian operator $H_o$. Here and from
now on, the subscript “nought” refers to the microscopic system being measured, not to an
initial time or anything like that. From now on for definiteness we refer to the apparatus
as an amplifying apparatus so as not to prejudge the question of measurement. Now the
amplifying apparatus would, if it were a closed system, also have its own mathematical
objects, $\psi_m$, $\mathcal{H}_m$, and $H_m$. Of course some states of the apparatus are suitable for detecting
the particle, and others are not. For example, a Geiger counter may have just been
discharged, and unable to detect. Or unplugged, or broken . . . . We suppose that $\psi_m$ is a
state where it is ready to detect.

We no longer believe that there is a cut which divides the quantum world from the
classical world, so we now believe that the amplifying apparatus itself is subject to axioms
1-3, as indicated, even if it is ‘macroscopic’ in the common-sense meaning of the word.
Furthermore, the axioms seemingly apply to the joint system as well. As follows: as usual
in Quantum Mechanics, the Hilbert space describing the joint system is just

$$\mathcal{H}^c = \mathcal{H}_o \otimes \mathcal{H}_m$$

where the superscript $c$ stands for “combined.” If the microscopic system is in the state $\psi_o$ and the apparatus is in the state $\psi_m$, then the combined system is in the state given by the wavefunction $\psi_o \otimes \psi_m$. If there were no interaction, the dynamics of the joint system would be given by the joint Hamiltonian given by

$$H^c = H_o \otimes I + I \otimes H_m$$

where $I$ means the (appropriate) identity operator. Since there is an interaction, then, tautologically, we have that the Hamiltonian of the joint system can be written

$$H^c = H_o \otimes I + I \otimes H_m + H^{int}$$

where $H^{int}$ is a linear operator on $\mathcal{H}^c$ which is thought of as the interaction term.

Wigner pointed out what he called a dualism in Quantum Mechanics: the same physical set-up which we are discussing can be analysed in two different ways, and although there is no logical contradiction or disagreement between these two ways, that is only because there is no way to compare them, either. Since the joint system is, at time $t = 0$, in the state $\psi_o \otimes \psi_m$, the joint system is, at any time $t$, necessarily in the state described by

$$\psi_t = e^{2\pi(iH_o \otimes I + I \otimes H_m + H^{int})} \cdot (\psi_o \otimes \psi_m).$$

On the other hand, the amplifying apparatus is fitted with a dial, labelled with the eigenvalues 0,1, say, for spin up and spin down (or vice-versa) and has a definite probability for being in one or the other.
The only reason these two analyses have not led to contradictory results is that there is no way to interpret the one in terms of the other, and hence no way to compare them. There is experimental support for both. Interpreting the probabilities as frequencies, as usual in classical Statistical Mechanics, the measurement axioms are extremely well supported provided only that one has a practical sense of when something is a measurement and when not. Recent developments in technology are eroding our sense of this divide: it used to be clear that measurement apparatuses were macroscopic and quantum systems were microscopic, one never observed peculiarly quantum effects such as superposition and entanglement except for microscopic systems. But with the advance of mesoscopic engineering and quantum teleportation, this dividing line, really it was a demilitarised zone, is finally being populated and explored, so the usual practical sense is less of a useful guide in this regard. The axioms involving Schroedinger’s equation are very well verified, experimentally, so much so that it would be an act of desperation to introduce changes in the equation (such as have been proposed) such as non-unitarity, non-linearity, or stochasticity merely in order to solve the problem of Quantum Measurement and not based on experimental observation of new forces or effects.

One way of looking at the problem, a less helpful one than the one we will adopt, has been to say that the first three axioms describe a linear deterministic dynamics which applies to all systems as long as they are not observed or measured. Or, as long as the systems undergo processes which are not measurement processes. They govern all physical processes except measurement processes. The second triad of axioms govern measurement processes and are non-linear, discontinuous (approximately) and stochastic. This way
of looking at the problem sneaks in some unwarranted assumptions. This will become clear if we take Einstein’s point of view (minus whatever notion he may have had about hidden variables, which are now, and rightly, regarded as against our physical intuition, and so are rejected by the program of this paper). In classical Statistical Mechanics, one had deterministic dynamical axioms, and from these, by means of purely mathematical methods, one derived probabilistic approximations. Our program, then, is to derive the three stochastic measurement axioms from Schroedinger’s equation without making any new postulates, simply using statistical mathematical methods of analysis of a quantum system with a large number of component parts and degrees of freedom.

From this point of view, the dualism Wigner points to is a gap, the lack of a definition of measurement process in terms of the first three axioms, and even more important, the lack of a definition of the phrase “the result of the measurement process is $\lambda_i$.” We would like to say that the measurement apparatus, call it $M_\infty$, possesses a pointer variable $f_\infty$. Probably $M_\infty$ will be modelled by a mathematical space of some sort and, if so, then probably $f_\infty$ will be some sort of function (or maybe an operator on a space of functions) defined on $M_\infty$. (The mathematical nature of $M_\infty$ and $f_\infty$ must remain further unspecified in order to avoid introducing unwarranted assumptions.) We would probably like to have that the statement “the result of the measurement process is $\lambda_i$” is modelled by the behaviour of $f_\infty$, perhaps by its taking the value $\lambda_i$ or something like that, but these desiderata have to remain rather vague, because their further specification involves making various physical and mathematical commitments, and we need to keep these commitments separate from each other and give each successive commitment explicit scrutiny.
A later section of the paper will be devoted to analysing, at this level of abstraction, the classical idea of the thermodynamic limit in the works of Darwin–Fowler, Khintchine, and Ford–Kac–Mazur.

Wigner himself makes the experimentally unwarranted but philosophically tempting assumption, quite explicitly, which he calls, ‘psycho-physical parallelism’ and von Neumann, after him, does the same. For only this reason, Wigner goes further in specifying the problem, and makes it insoluble. He assumes that our perception of the macroscopic measurement apparatus’s giving the result \( \lambda \) must correspond to a wave function of the apparatus or a more or less well-defined set of wave functions. Furthermore, Einstein’s program suggests the opposite, since the thermodynamic limit of a sequence of dynamical systems can be, and usually is, a dynamical system with a totally different state space, a state space which is not in any obvious sense a limit of the other state spaces.

Wigner, instead, goes on to further concretely specify the problem as one of entanglement. Suppose the incoming particle is described by a two-dimensional Hilbert space spanned by orthonormal wave functions \( \psi_{\varepsilon} \).

Assume that the particle has a spin up state described by the wave function \( \psi_1 \) and a spin down state described by \( \psi_o \). Assume that the measurement apparatus, when plugged in, calibrated, charged up and ready to detect the particle, is in state \( \varphi^m_o \). Wigner postulates that if the particle is in state spin up, then the apparatus moves, after a time period, its pointer to point to ‘u’ and its wave function is then \( \varphi^m_u \) whereas if the particle was in state spin down, then the apparatus evolves to \( \varphi^m_d \), say. Hence the joint system if in the state \( \psi_o \otimes \varphi^m_o \) evolves after unit elapsed time to \( \psi_o \otimes \varphi^m_d \) but if in the state
\( \psi_1 \otimes \varphi^m \), to \( \psi_1 \otimes \varphi^m \). Wigner points out that the linearity of Schroedinger’s equation forces that a superposition of states on the part of the particle leads to entanglement. That is, if \( v = c_1 \psi_o + c_2 \psi_1 \), then \( v \otimes \varphi^m \) evolves to a state which cannot be written as \( \psi \otimes \varphi^m \) for any choice of states of the particle and the apparatus. Such tensor products are called decomposable, and for them, and only them, do the particle and the apparatus have separate identities. What we actually get is \( c_1 \psi_o \otimes \varphi_d + c_2 \psi_1 \otimes \varphi_u \) and this is called an entangled state. The axioms of quantum mechanics actually forbid us to interpret this as if it were a classical mixture, as if it meant the joint system had a probability \( |c_1|^2 \) of being in the decomposable state \( \psi_o \otimes \varphi_d^m \) and a probability of \( |c_2|^2 \) of being in the decomposable state \( \psi_1 \otimes \varphi_u^m \). To go somehow from this analysis *to* the forbidden classical mixture interpretation has been called by J.S.Bell, “the Philosopher’s Stone of Quantum Measurement.” But to pose the problem in precisely these terms is to commit the fallacy of misplaced concreteness.

It is a misplaced concreteness to assume that pointer positions are well defined sets of wave functions of the apparatus. We will see that, granting that the measurement axioms are only approximately valid for constructible amplifying apparatuses obeying the laws of reality, this fallacy is much the same as assuming that the only approximations which have physical validity are those describable in terms of the norm topology. But the approximations of classical statistical mechanics, which Einstein had in mind, do not fit into this misplacedly concrete paradigm.

In summary, we do not adopt the exact viewpoint of some on the problem. We do not say that we have to define when the linear dynamics is valid and where the cut is which
determines when the stochastic non-linear dynamics becomes valid. Instead, we wish to
derive the second three axioms (or approximations thereto) from the first three axioms.
The approximations, as usual, will be good approximations within a certain domain of
parameters, and less good or even downright bad, elsewhere. That is the point. We
wish to give a rigourous definition in physical terms and in terms of the behaviour of
its Hamiltonian, of which physical processes are measurements and which are not. The
definition will be, that a physical process is a measurement when the sort of approximations
we will derive are useful descriptions. This is totally non-subjective and non-circular. In
principle, this will lead to experimental consequences as mesoscopic engineering develops,
which can discriminate between this solution and others which have been proposed. In
particular, we accept that entanglement persists. Our model provides, in principle, a
theoretical basis for determining which observables will be able to detect macroscopic
superpositions and entanglements, and which will not be able to detect them.

Wigner’s paper’s formulation of the problem has already been outlined. We outline
his solution and show why it does not quite fit into our framework.

Wigner has proposed that the definition of measurement process is that a conscious-
ness is involved, and that when a consciousness is involved, in reading off the position
of the pointer on the dial, then a new, still undiscovered non-linear dynamical equation
governs the process. He has not generally been followed in this. His solution does not fit
into our framework because he does not derive this non-linear equation from the linear
one. But his acceptance of the philosophical dualism that separates consciousness from
unconscious matter, a dualism that goes back to Descartes and Malebranche and Leibniz
in modern times, is extremely interesting, and will have a role to play in philosophy.

Wigner assumes without proof that the measurement apparatus must have pointer positions that are describable by wave functions. The axioms do not assert this. Experimental practice would be unchanged if this assumption were abandoned or contradicted. There is a vast difference between ‘interpretation’ and ‘operationalisation’ and we will change the interpretation of pointer position from Wigner’s without changing its operationalisation in terms of laboratory procedures. The axioms do not force Wigner’s interpretation (which is the usual one) on us since the axioms do not interpret themselves. The phrase, “the result of a measurement process” is, just as is the concept of ‘measurement,’ an undefined one, a primitive one. Operationalisation is the way we turn physical concepts or the mathematical models of them into concrete laboratory procedures. If one adopts a particular operationalisation this may well impose various constraints on the interpretations that can be consistent with it, but it by no means determines them uniquely. (And vice-versa, of course.) We will end up by abandoning the philosophical assumption of psycho-physical parallelism in favour of a much looser but still physically precise correlation. This would involve modifying the Cartesian dualism *cum* parallelism to a dualism between mind and matter which is not precisely parallel, but admits of various rigorous correlations which are not one-to-one mappings. This is just as consistent with the experimental evidence as is Wigner’s philosophical assumption. The assumption of psycho-physical parallelism, which is a philosophical assumption, is a fallacy of misplaced concreteness.

Of course if we accept Einstein’s program as valid, we have to accept that the measurement axioms are approximations to physical reality analogously to the status, in classical
physics, of the approximations of Statistical Mechanics such as temperature and phase transition, which are not exactly valid for finite systems but only are well-defined in the thermodynamic limit. Probabilistic formulas are approximations to deterministic reality.

It should be pointed out that once we accept that the measurement axioms may be satisfied approximately instead of exactly, there are indeed many kinds of approximation: there is no experimental evidence to force us to choose the strong topology. (In this paper, we are going to adopt the same sort of approximation as used in the classical statistical mechanical papers of Darwin–Fowler and others. Coleman–Hepp adopt the same sort of approximation as used in Quantum Statistical Mechanics and the theory of local algebras and inductive limits of $C^*$-algebras.) Some of Bell’s criticisms are misguided on this issue, he reasons as if only the strong topology is acceptable. But there are reasons why the strong topology on the space of observables cannot possibly be of physical significance for measurement, based on the Araki-Wigner-Yanase theorem (that observables with eigenvalues closer and closer together require larger and larger, without bound, measurement apparatuses to measure them with a fixed degree of accuracy). We will discuss this issue in the next subsection.

Bohr proposed that measurement apparatuses were classical. We explicitly implement this old idea in what is a novel way: we find a thermodynamic limit of quantum systems which is a classical system. This is in sharp contrast to Coleman-Hepp and every other statistical mechanical approach to the problem of Quantum Measurement, which studies a thermodynamic limit system which is still quantum. Bohr on numerous occasions asserted (without proof) that the measurement apparatus and pointer positions implicit in the ax-
ioms had to be a classical apparatus, or at least classically describable. In conversation with Rosenfeld, he once explicated this further: the apparatus has to be such, that its pointer positions are such, that it is a valid approximation to neglect Planck’s constant, that is the precise meaning he was pushed to giving to his more famous phrase, “classical in nature.” We implement this by introducing a renormalisation in our limiting process that sends Planck’s constant to zero. As always (prior to the C*-algebra approach), the thermodynamic limit itself is unphysical, a mere mathematical convenience for easily obtaining good approximation to a physical system which has a large but finite number of degrees of freedom. For an actual amplifying apparatus with a large but finite number of components, and a small but non-zero value of Planck’s constant, the measurement axioms calculated by considering instead the limit as the number of components grows without bound and as Planck’s constant decreases to zero without an upper bound to its reciprocal, are a good approximation for the purposes of pointer positions (although not, say, for irrelevant aspects of the amplifying apparatus such as the radium paint foolishly used to decorate the dial . . . ). But there are other considerations which suggest that the measurement axioms are exactly valid only about an unphysical thermodynamic limit of some sort, besides the correspondence principles and Bohrian tradition. These will be discussed separately in the next subsection.

The goal of this paper is to show that Einstein’s program can be accomplished in a way hitherto assumed, without sufficient proof, to be impossible. We show, by using a more foundational approach to classical statistical mechanics, that hidden variables are unnecessary. Referring to the list of desiderata and avoiderata in “Against Measurement”,
this paper avoids all use of the banned terms environment, reversible, irreversible, information, measurement. And, I might add, dissipative, decoherence, random, mixture, density matrix. (For us, all systems are closed systems, all states are pure states, and all superpositions are coherent, always.) The banned terms system, apparatus, microscopic, are merely used as mnemonic labels and are not relied on to derive substantive conclusions. The banned terms macroscopic, and observable are precisely defined in terms of acceptable notions. By observable, we will mean an abelian observable as in classical Hamiltonian mechanics, and we give this a precise meaning in terms of the axioms of quantum mechanics. The usual primitive notion of quantum mechanical observable is not used. Nor do we study linear operators and their eigenvalues. The Hamiltonian is not regarded as an observable at all, but simply, as in classical mechanics, as the infinitesimal generator of the dynamics. We derive the usual probabilistic axioms about the results of measurements from the axioms which do not involve the concept of measurement or observable. In particular, although the whole approach of this note is statistical, we do not make any statistical hypotheses. We make only deterministic hypotheses as is usual in any Hamiltonian dynamical system, but use statistical methods to study those systems. This will become clearer by example.

Schwinger has written an important paper on the subject of quantum Brownian motion. He does not make any connection to the topic of Quantum Measurement. Nevertheless, there are important results claimed (but not proved explicitly) in the paper which turn out to be relevant. He derives formulae for the interaction of a microscopic particle with a Hamiltonian heat bath at a negative temperature, which produces a negative
temperature Brownian motion in the particle. He remarks that this motion amplifies the quantum motion of the particle to the classical level, where quantum uncertainties and non-commutativities are negligible. Hence, every key idea of this paper (writing down an explicit Hamiltonian for an amplifying apparatus may not have been done before, but it is routine) has been anticipated except one, the definition of macroscopic observable.

The self-restraint of the adopted program of this paper rules out the postulating of open systems, since they are not allowed by the axioms. By definition, in the foundations of the classical epistemological approach to physics, system means closed system. The axioms are for closed systems only. Nevertheless, something should be said in this section about the open systems approach to the problem of Quantum Measurement. Zurek et al. and Zeh et al. among many others are prominent proponents of the open systems approach, which makes the peculiar (from the point of view of Wigner and the axioms) properties of Quantum Measurement depend upon the interaction of the joint system of microscopic system being measured *cum* measurement apparatus with the environment in a way reminiscent of the phenomenological approach to Hamiltonian heat baths, Brownian motion, and other thermodynamic limit phenomena. Up to now, they have no experimental support for their proposals, and also have phenomenological parameters which they adjust in order to achieve this. They merely postulate a master equation of some sort, but do not derive it from an underlying deterministic dynamics. This is entirely analogous to the difference between the Langevin approach to Brownian motion and the Wiener approach. Wiener’s approach allows us to derive the stochastic differential equation from Newton’s equations, as shown by Ford–Kac–Mazur in an immensely influential paper. Wiener’s approach is
adopted here, in imitation of the beginning parts of that paper. Bell would criticise the open systems approach because of its use of undefined notions such as decoherence, and this is a foundationally important issue to clear up: it is important to know whether or not the problem can be solved without introducing new primitive notions. But whether Nature uses one solution or the other cannot be specified a priori by foundational desiderata. Although from our viewpoint it seems counter-intuitive to make the reduction of the wave packet, or the decoherence of the entanglement of the pointer pointing to zero with the pointer pointing to 1, a macroscopic superposition of states, depend on interaction with a thermally stable environment coupled by only relatively weak forces to the apparatus, this is a question for experiment. Recent success in quantum teleportation shows that entanglement persists over dozens of miles with no specially quantum efforts to screen the system from interaction with the environment.

This means that it is up to future experiments to determine whether the peculiar behaviour of measurement apparatuses is due to their interaction with the environment, or due to their interaction with the system being measured.

The Paradox of Degeneracy

In this sub-section we adduce new considerations, a kind of thought experiment in taking the six axioms completely literally, which suggest two things: the norm topology on operators is not physically significant for observables, and, the notion of observable might be the result of a limiting procedure of some sort, only approximately true in any finite-sized laboratory. We connect this with results of Araki-Wigner-Yanase. The similarities with the status of phase transitions in classical Statistical Mechanics is striking, and suggests that
the mathematical behaviour of an observable should be derived by a mathematical limiting procedure (such as the thermodynamic limit) from physically realisable phenomena.

The principle of continuity has a long history in Physics, and underlies the notion of a real variable. It states that small variations in the physical set-up of the lab should produce only small changes in the experimental results. This has implications for the mathematical model used to model the physical set-up and the experimental results. It might be thought that this principle has been discarded by Quantum Mechanics, but this is not so. Dirac, for example, appeals to it in justifying the axiom of the reduction of the wave packet, in his analysis of repeated measurements. The impression that the principle is refuted or discarded is based on a misunderstanding of what are the experimental results in Quantum Mechanics, in the real world. No particular quantum jump or discontinuity is ever predicted by the calculations of Quantum Mechanics, only probabilities or, equivalently, expectation values. These probabilities do indeed vary continuously with continuous variations in the physical parameters of the lab set-up. More contortedly, the observation that the measurement process resulted in \( \lambda_i \) “this time” is not, strictly speaking, an experimental result. Experimental results are, by definition, replicable, and this is not replicable (except in the special case that the probability was unity). Observations that are replicable include “it can happen that the result is \( \lambda_i \),” or, “the probability that the result is \( \lambda_i \) is in between .1 and .15,” and things like that. The result of a single physical process, occurring once, is not an experimental result unless it is replicable. Nature and Heisenberg have taught us that only the probabilities are replicable, therefore, only the probabilities are experimental results. If Heisenberg taught us anything, it is that a physical theory should not be
criticised except for disagreement with experimental results, and it follows from this that
a physical theory does not have to explain more than the probabilities. Therefore the
principle of continuity only applies to continuous variation of probabilities and expectation
values with continuous variation in physical set-up. There is no reason, yet, to suppose
that Quantum Mechanics violates the principle of continuity. But in the following thought
experiment, it will be seen that the mathematical model of the measurement axioms does
indeed violate the principle of continuity in a hitherto unsuspected way. We are going to
construct a family of observables which vary continuously in the norm topology, but yield
an experimental set-up where a certain expectation value varies discontinuously.

Briefly, if $Q$ is an observable with degenerate eigenvalues and if $Q_\epsilon$ is a family ($\epsilon > 0$)
of perturbations of $Q$ which remove the degeneracy, then we consider the reduction of
the wave packet produced on the one hand by measurement of $Q$ and on the other by
measurement of $Q_\epsilon$ as $\epsilon$ approaches zero. We obtain a discontinuous variation in the
expectation value (of a different observable) as a function of $\epsilon$. This violates the principle
of continuity.\(^1\)

However if one interprets the reduction of the wave packet as an approximation in a
specific way, it is predicted that this variation should be smoothed out.

Let $\mathcal{H}_1 = C \cdot |\uparrow\rangle \oplus C \cdot |\downarrow\rangle$ be a two dimensional Hilbert space which is the state space

\(^1\) This is the only violation of this principle in Quantum Mechanics. The so called
‘quantum jumps’ are not discontinuous, except in slang. For only functions can be contin-
uous or discontinuous. But it is precisely in the conventional interpretation of Quantum
Mechanics that the result of an experiment is not a function (of anything). The probability
with which a given result will be observed is a function of experimental conditions—but
this depends continuously on the physical parameters, except in the case to be discussed
in this paper.
of particle one. Let $\mathcal{H}_2$ be isomorphic to $\mathcal{H}_1$ and be the state space of a distinguishable particle, particle two. Let $\psi = |\uparrow\rangle \otimes |\uparrow\rangle + |\uparrow\rangle \otimes |\downarrow\rangle$. Let $Q$ be the (degenerate) observable “‘spin’ of the first particle,” taking eigenvalue 1 if the first particle has spin up and 0 if spin down. Now the eigenvalues of $Q$ each have a two-dimensional degeneracy. Let $Q_0 = |\uparrow\rangle \otimes |\uparrow\rangle = (1 + \epsilon)|\uparrow\rangle \otimes |\uparrow\rangle$ and $Q_0 = |\uparrow\rangle \otimes |\downarrow\rangle = (1 - \epsilon)|\uparrow\rangle \otimes |\downarrow\rangle$ (and be otherwise unperturbed). This perturbation removes the (relevant) degeneracy. We suppose the physical system is in state $\psi$. The results of experiments $Q_0$ pass continuously to those of $Q$ as $\epsilon \to 0$.

But the reduction of the wave packet does not. If $Q$ is measured, $\psi$ is unchanged. If $Q_0$ is measured, then there are equal chances that $\psi$ jumps to $|\uparrow\rangle \otimes |\uparrow\rangle$ or that it jumps to $|\uparrow\rangle \otimes |\downarrow\rangle$ but it never lands anywhere else in the span of those two vectors. Let $R$ be an observable which is zero on the zero-eigenspace of $Q$, and which has as eigenvectors $|\uparrow\rangle \otimes |\uparrow\rangle + |\uparrow\rangle \otimes |\downarrow\rangle$ and $|\uparrow\rangle \otimes |\uparrow\rangle - |\uparrow\rangle \otimes |\downarrow\rangle$, with eigenvalues one and zero, respectively.

If $Q$ has been measured, then the expectation value of $R$ is unity. But if $Q_0$ has been measured for any $\epsilon > 0$, the expectation value of $R$ is half of that. This is a violation of the principle of continuity which, indeed, is not an axiom of Quantum Mechanics, but
seems physically warranted in this example.

It is well-known\cite{Araki1960} that the performance of a measurement that discriminates with increasing precision between eigenvalues that approach more and more closely together requires increasing resources (laboratory size, etc.) It would seem therefore, that the passage from $Q_\epsilon$ to $Q$ is not physically continuous even though it is continuous in operator norm.

One might hypothesize that the degree of validity of the approximation of the reduction of the wave packet rule to the behaviour of the actual, finite size, physical apparatus, depends on three things which affect the convergence: the exact initial condition $\psi$, the magnitude of the apparatus, call it $n$, and the distance between neighbouring eigenvalues. Even if we neglect the first two of these, it is obvious that the third is variable as $\epsilon \to 0$ and so the variation of the expectation value will be smoothed out, as is easy to see.

\begin{footnotesize}

Benatti\textsuperscript{2} has worked out a derivation of the smoothing effect from the general assumptions of the Ghirardi–Rimini–Weber approach to quantum measurement, which involves replacing Schroedinger’s equation by a stochastic differential equation.

\end{footnotesize}

\begin{footnotesize}

\textsuperscript{1} Araki, H., and Yanase, M., Physical Review, vol. 120, p. 622 (1960).

\textsuperscript{2} Personal communication

\end{footnotesize}
Finally, we remark that it seems to be a common mistake to think that because $\psi(x,t)$ is not continuous, that therefore the principle of continuity does not hold in Quantum Mechanics. Firstly, for fixed $t$, $\psi$ is not a function, it is an element of $L^2$ and its value at a point is not defined, and so is certainly not physical. Rather, $\int_{x_0}^{x_0+\epsilon} |\psi(x)|^2 dx$ is physical—but it is also a continuous function of $x_0$ and $\epsilon$. Secondly, for variable $t$, although it is true that $\psi$ has a jump discontinuity when a measurement is made, it is also true that precisely in the usual interpretation of Quantum Mechanics, because it is non-positivistic, $\psi$ is not regarded as objective and physical: only the results of calculations using $\psi$ in order to predict results of experiments are. And here, as remarked above, there are no discontinuities except in the case of a degenerate observable.

Even so, the fact that, at least in the $C^*$-algebra context, a measurement theory based on taking the thermodynamic limit of the interaction between a (fixed) microscopic system and a (variable) macroscopic measuring device has been constructed is encouraging. Classically it is of course well known that for any finite point at which we stop short of the thermodynamic limit, we have determinism. Only at the unphysical idealized thermodynamic limit do we obtain probabilistic behaviour. So it seems likely that the same thing is true for quantum systems. This would suggest, as a dependence on $n$, the same smoothing effect we have predicted (as a function of $\epsilon$).

It also suggests that the principle of continuity will hold at every finite point, and that the discontinuities usually taken to be part of the structure of theoretical physics are simply an unphysical artifact of the approximation procedure inherent in passing to the thermodynamic limit.
3. The Concrete Model of the Ming Effect

From now on, we distinguish between measurement apparatus and amplifying apparatus. The amplifying apparatus we study will be an explicitly given quantum system with \( n \) degrees of freedom, \( M_n \), modelled by a Hilbert Space \( \mathcal{H}_n \) and with a Hamiltonian \( H_n \). It approximates more and more to a measurement apparatus as \( n \to \infty \). The measurement apparatus is a thermodynamic limit of \( M_n \), denoted \( M_\infty \), and is a classical dynamical system. Its states are the equilibrium states of the thermodynamic limit, and are not described by wave functions, its state space is a symplectic manifold, not a Hilbert space, has no linear structure, superposition of states is a nonsensical undefined concept for it. Classical mixtures of its states are possible, as always in classical Statistical Mechanics. One can take the viewpoint that measurement apparatuses and processes are unphysical idealisations of the only processes that are physical, the amplifying processes. This is a valid logical interpretation of the measurement axioms (even, after some contortions, the reduction of the wave packet) and it does not involve any change in the operationalisation of the concept of measurement. In fact, it grounds in concrete calculations what used to be operationalised anyway without justification: the fact that an amplifying apparatus must be large before the measurement axioms are verified. A one-atom device does not perform a measurement \ldots or reduce the wave packet.

Let the state space of an incident particle be \( \mathbb{C}^2 \). This space has basis \( \{\psi_0, \psi_1\} \). For each \( n \), \( \mathcal{H}_n \) is the Hilbert space of wave functions describing the state space of an \( n \)-oscillator system which is an amplifying device. We let \( \mathcal{H}_n = \mathbb{C}^2 \otimes \mathbb{C}^2 \ldots \mathbb{C}^2 \).

In the presence of an incident particle in the state \( \psi_1 \), the amplifying apparatus will evolve in time under the influence of \( A_n \) (called “Ming,” since it leads to a bright and
clear phenomenon), a cyclic nearest-neighbour interaction which is meant to model the idea of stimulated emission or a domino effect. In the absence of a detectable particle, the dynamics on the amplifying device will be trivial. This means that 

$$\mathcal{H}_n^{com} = (C \cdot \psi_1 \otimes \mathcal{H}_n) \bigoplus (C \cdot \psi_o \otimes \mathcal{H}_n)$$

and we put 

$$H_n^{com} = I_2 \otimes A_n + I_2 \otimes I_2^n .$$

The intuition is that \( \psi_1 \) means the particle is in the state which the apparatus is designed to detect, but \( \psi_o \) means the particle is in a state which the apparatus is designed to ignore.

It will simplify things if we assume \( n \) is prime. (The general case can be reduced to this by perturbation.) Let \( q = \frac{2^n - 2}{n} \) (which is an integer by Fermat’s little theorem).

Let \( i \) be any integer between 0 and \( 2^n - 1 \). There are \( n \) binary digits \( d_i \) with \( i = \sum_{0}^{n-1} 2^i d_i \) and uniquely so. If \( \{ |1\rangle, |0\rangle \} \) is a basis for \( \mathcal{H}_1 \), then \( |i\rangle_n = \otimes_{0}^{n-1} |d_i\rangle \) form a basis of \( \mathcal{H}_n \). The intuition is that the \( i^{th} \) oscillator is in an excited state \( |1\rangle \) if \( d_i = 1 \) and is in the ground state \( |0\rangle \) if \( d_i = 0 \). We also write \( |i\rangle_n = |d_0 d_1 \ldots d_{n-1}\rangle \).

We wish to find \( \text{Ming} \) such that in one unit of time the \( d_i \) are cycled as follows:

$$e^{\frac{2\pi i}{n} A_n} |i\rangle_n = |d_{n-1} d_0 d_1 \ldots d_{n-2}\rangle.$$

Choose a set of representatives \( b_i \) such that every integer \( k \) from 1 to \( 2^n - 1 \) can be written uniquely as \( b_i 2^m \mod (2^n - 1) \mathbb{Z} \) for some \( 1 \leq i \leq q \) and \( 0 \leq m \leq n - 1 \), that is, 

\( k = b_i 2^m + j(2^n - 1) \) for some \( j \) but \( i \) is unique. (Since \( 2^n - 1 \) and \( 2^m \) are relatively prime, no matter how \( k \) and \( m \) are fixed, there exist unique \( b_i \) and \( j \) satisfying this.)

Then each \( |b_i\rangle \) represents an orbit under the action of \( e^{\frac{2\pi i}{n} A_n} \). Re-order the basis as follows: let \( v_o = |b_1\rangle, v_2 = |b_1 2\rangle, v_2 = |b_1 2^2\rangle, \ldots, v_{n-1} = |b_1 2^{n-1}\rangle, v_n = |b_2\rangle, v_{n+1} = |b_2 2\rangle, \ldots, v_{2n-1} = |b_2 2^{n-1}\rangle, v_{2n} = |b_3\rangle, \ldots, \) up to \( v_{(q-1)n} = |b_q\rangle, v_{(q-1)n+1} = |b_q 2\rangle, \ldots, v_{(q-1)n+n-1} = |b_q 2^{n-1}\rangle \), but \( (q - 1)n + n - 1 = 2^n - 3 \), so we have \( 2^n - 2 \) basis vectors accounted for. Let \( v_{2^n-2} = |0\rangle \) and \( v_{2^n-1} = |2^n - 1\rangle \).

Let \( V_1 \) be the space spanned by \( \{v_o, \ldots, v_{n-1}\} \), let \( V_2 \) be the space spanned by \( \{v_n, \ldots, v_{2n-1}\} \), etc., up to \( V_q \). Let \( V_o \) be the space spanned by \( \{v_{2^n-2}, v_{2^n-1}\} \). The
Ming Hamiltonian operator $A_n$ on $\mathcal{H}_n$ is a direct sum of its restrictions to the $V_i$. Its restriction to $V_0$ is to be the zero operator. Each $V_i$ is isomorphic to $V_1$ and we give the matrix of each restriction of $A_n$ with respect to the given bases.

Solving

$$A_n = \frac{\hbar}{2\pi} \log \begin{pmatrix} 0 & 0 & \cdots & 0 & 1 \\ 1 & 0 & \cdots & 0 & 0 \\ 0 & 1 & \ddots & \vdots & \vdots \\ \vdots & \ddots & \ddots & 0 & \vdots \\ 0 & \cdots & 0 & 1 & 0 \end{pmatrix},$$

we obtain a cyclic skew-hermitian matrix, whose $i,j$ entry, $-\frac{\hbar}{n} \sum_{k=0}^{n-1} ke^{2\pi i k(i-j)}$, is approximately (if $n$ is large compared to $i-j$) $\frac{\hbar}{2\pi}$ unless $i=j$ in which case $\frac{\hbar}{2}$.

As usual in classical statistical mechanics, the observables are all abelian, and are given by measurable functions on the phase space, hence $f_n$ is an observable if $f_n : \mathcal{H}_n^{\text{com}} \to \mathbb{R}$ is measurable and $f(c\psi) = f(\psi)$ for $c \in \mathbb{C}^\times$. In order to avoid confusion with the orthodox primitive concept of observable, modelled by a linear operator, we will not refer to $f_n$ as an observable and will not use the term ‘observable’ in our system at all. These measurable functions are dynamical variables, as usual in Hamiltonian dynamics.

The intuitive picture is that this device is getting more and more classical as $n$ goes to infinity. So the energy levels get closer and closer, approaching a continuum, the oscillators get closer and closer which is why the interaction, at a constant speed, travels from an oscillator to its neighbour in less and less time. If we adjusted by rescaling the dynamics to accomplish this, the entries of $H_n$ would diverge with $n$. We rescale $\hbar$ instead, so that it decreases as $\frac{1}{n}$, so that we have finite length and fixed density and fixed total energy. This is typical of rescaling procedures in classical statistical mechanics. It is physically meaningful because the thermodynamic limit is never physically real, it is only one of the $\mathcal{H}_n$ which is physically real: $n$ is not a physical variable, it is a parameter. Passing to the
limit is only a mathematical convenience to obtain simple approximations for the physical truth about $\mathcal{H}_{1.1 \times 10^{23}}$. Since $h$ is truly small, this yields valid approximations.

4. Classical Statistical Mechanics

The presentation of classical Statistical Mechanics in undergraduate, and even most graduate, textbooks is logically incoherent and is inadequate for the purposes of answering Bell’s criticisms of the statistical mechanical approach to the problem of Quantum Measurement. A logically coherent and adequate foundation for the subject was outlined, with many special cases done explicitly and compared to experimental results, by Darwin and Fowler in the 20’s. This approach was also taken up by Kolmogoroff, Khintchine, and Wiener, in scattered papers on the ergodic theorem. Khintchine attempted a somewhat controversial generalisation of Darwin and Fowler’s many examples, but we will not appeal to the controversial part of Khintchine’s explanation, but only the unusually perceptive and acute formulation he gives of the foundations of classical Statistical Mechanics as a program. In its full generality, it remains today as one of the challenges to pure mathematics to establish its scope of validity as a theorem, we do not need this.

The notion of thermodynamic limit employed by these mathematical physicists does not seem to fit into the framework of topology. These methods were acutely, if disparagingly, characterised by R. Minlos, “For a long time the thermodynamic limit was understood and used too formally: the mean values of some local variables and some relations between them used to be calculated in a finite ensemble and then, in the formulas obtained, the limit passage was carried out.” But this is much the same as to say that a method of double duality was employed . . .

The methods of Darwin–Fowler and Ford–Kac–Mazur are well supported experimen-
tally. The results of the quantum measurement axioms are well supported experimentally. It adds to the likelihood of the correctness of our analysis that the statistical methods we will use in the next section are brain-damaged analogues of well-supported methods and lead to well-known results.

It is also the case that these methods have been largely supplanted by $C^*$-algebra methods and the definition of thermodynamic limit as infinite system (not as a limiting process) of Ruelle and others. But these latter methods are less flexible, so far. So far, they have only been used to study infinite volume limits. But these are not the limits appropriate to Brownian motion. Besides this, if we take Bohr seriously, we want an ‘infinitely rigid’ limit, or ‘increasingly classical in nature’ sort of limit, and this seems to involve smaller and smaller quantum units of action becoming located closer and closer to each other, nothing to do with infinite volume. Doubtless the classical limit of quantum systems we will construct in the next section could be fit into some kind of algebraic framework, but because of the paradox of degeneracy, it seems that the strong operator topology is un-physical, so it seems pointless to do so.

The Gibbs program is to derive the probabilities from a deterministic dynamics via a Hamiltonian heat bath. When applied to the question of Brownian motion, it takes the following form: for every integer $n$ we consider a Hamiltonian system of one Brownian mote and $n$ surrounding bath particles. As $n$ increases, we may wish to let the mote remain at a fixed mass but let the other particles decrease in mass proportionately, keeping the total mass fixed. Let each such system be labelled $M_n$, it is a symplectic manifold as a phase space and possesses a deterministic flow on it given by the dynamics: given an initial
condition $v_o \in M_n$, the system will be in state $v_o(u)$ after a period of time equal to $u$.

They study a particular dynamical variable $f_n$ on each system: the momentum of the Brownian mote. Or, rather, its auto-correlation function,

$$g_n(t) = \lim_{T \to \infty} \frac{1}{T} \int_0^T f_n(v_o(u))f_n(v_o(u+t))du.$$

Or, rather, what would be equivalent if the system were ergodic, a phase average of $f_n(v_o(u))f_n(v_o(u+t))$, instead of a time-average. That is, following Gibbs's original vision, they impose a Maxwell–Boltzmann probability distribution on the space $M_n$, supposing it to be a heat bath in equilibrium at a positive temperature. (This has been superseded in following papers.) Now, a deterministic flow taken together with a probability distribution on the space of initial conditions yields a stochastic process, call it $P_n$. This yields, in turn, a stochastic process on the momentum of the Brownian mote alone. Hence for each integer $n$, they obtain a Gaussian stochastic process with auto-correlation function $g_n(t)$. They pass to the limit as $n \to \infty$, which requires a cut-off and re-scaling procedure, obtaining a function $g_\infty(t)$ which they interpret as the auto-correlation function of a Gaussian stochastic process, even though they have as yet no phase space or probability space for the process to live on, much less the process itself. They show that this limit function has no memory effects, even though the finite systems $M_n$ each satisfy Poincaré recurrence. In fact, it corresponds to the Wiener process. They have thus derived the Wiener process from placing a mote in a Hamiltonian heat bath at a positive temperature.

Let the stochastic process on each $M_n$ as above be $P_n$. Let the Wiener process which arose in the limit be $P_\infty$. If we regard their procedure as being somehow that the sequence of $P_n$ has, as a limit, $P_\infty$, then we have a notion of limit that does not seem to fit into the
framework of topology very well. It is very flexible.

Their paper has been very influential and further work has shown that the thermodynamic limit is largely independent of the particular probability distribution imposed on the bath, and robust with respect to the particular dynamics introduced on the $M_n$. This is only to be expected, since thermodynamic limits are robust to the underlying dynamics, because of the central limit theorem. In the next section, we will show that a stochastic process arises even without a probability distribution being imposed on the bath.

In summary, then. The procedure of Ford–Kac–Mazur was a kind of double duality. Given a sequence of finite classical systems, $M_n^{com}$, they did not attempt to find the state space of the limit object from the state spaces of the sequence. Instead, they, before passing to the limit, passed to the consideration of dual objects, (dynamical variables: of course the auto-correlation functions are a species of dynamical variables). Passing to the limit they obtained a candidate for a dynamical variable, which they regarded as the dual object for an unknown dynamical system, to be sought. We will carry this out in our setting (but using time averages for convenience).

5. The Statistical Analysis of Amplification

Should we study quantum, non-commutative observables, i.e., linear operators, on each amplifying apparatus and then pass to the limit? For the reasons discussed in the paradox of degeneracy, the strong operator topology on observables, which in our model is the only relevant topology, cannot be foundational or of direct physical significance. It probably arises as the result of a limiting procedure, so that the discontinuity in the paradox can be seen as a non-physical artifact of invalidly interchanging limits in a double
Therefore we should study something else on each $\mathcal{H}_n$ and derive the observable as a limit as $n \to \infty$. Amazingly, the ordinary abelian dynamical variables suffice for this purpose, as if the space of rays in $\mathcal{H}_n$ were to be treated as a phase space exactly like in classical Hamiltonian Mechanics. Because only one measurement can be made at a time, that measurement commutes with itself, so abelian-ness is not a restriction. The non-commutativity of the quantum observables will arise out of the fact that the different measurement apparatuses required to measure them force each sequence of abelian dynamical variables to live on totally disjoint spaces, so that their commuting with each other is a meaningless question. If one and the same amplifying apparatus can measure two quantum observables, then they must commute. But this was already pointed out by Bohr and Heisenberg.

Macroscopy and pointer variables

In order to do thermodynamics we consider sequences of observables which are abelian dynamical variables, $\{f_n\}_{n=1}^\infty$. Each $f_n$ should have the same “physical meaning” relative to $\mathcal{H}_n$. It is unclear how to formalise this in complete generality. If each $f_n$ is “energy” we are doing the same thing as Khintchine, Darwin–Fowler. Ditto if each $f_n$ is a phase average of a measure of the fluctuation of energy of a single component. Ford–Kac–Mazur consider the example where each $f_n$ is the momentum auto-correlation of one component. (It is an important open problem, which is very difficult, to find the largest range of validity of this procedure, which they do in concrete examples, and give an abstract definition which will cover such a range.) But we have to consider a sequence $\{f_n\}$ which captures the notion of “visible to the naked eye” or, “macroscopic.”

At any rate, we formally define such a sequence of $f_n$ to be macroscopic if whenever
the sequence of norm one vectors \( v_i \in \mathbb{C}^2, i > 0 \), satisfies

\[
\lim_{n \to \infty} f_{n+n_o}(v_o \otimes v_{n_o+1} \otimes v_{n_o+2} \otimes \cdots \otimes v_{n_a+n})
\]

exists for some \( n_o \) and some \( v_o \in \mathcal{H}_{n_o}^{\text{com}} \), then it exists and is independent of the choice of \( n_o \) and \( v_o \).

Many macroscopic observables, such as temperature, are not relevant. Only macroscopic variables relevantly coupled to the incoming particle are pointer variables.

The concept of a pointer variable is one that had resisted precise definition. The intuitive picture has always been that of a measurement apparatus which, after amplifying its response to the incoming microscopic system, makes a macroscopic needle point to a number on a dial in a naked-eye fashion. If the measurement apparatus is classical this is easy to define, but then it is not so easy to link it to a quantum mechanical Hamiltonian, which is required if one is to build the apparatus out of the bricks that are available.

Wigner and others define a pointer variable as some sort of quantum observable which is “coarse” in the naive sense of not varying much over a large subspace of a Hilbert space. This famously fails to convert quantum superpositions into classical probabilities.

The theory of the Ming Effect uses a novel type of pointer variable: an amplifying apparatus is one of the negative temperature systems such as \( \mathcal{H}_n \). A measurement apparatus is the thermo-dynamic limit of quantum negative temperature amplifiers, and is a classical dynamical system \( \Omega_\infty \). A pointer variable for \( \Omega_\infty \) is a macroscopic observable \( \{f_n\} \) for the sequence \( \{\mathcal{H}_n\} \) as above, such that the expectation of \( f_n \) is coupled to the initial state of the microscopic system \( \mathcal{H}_0 \) and a fixed class of states of \( \mathcal{H}_n \) (regarded as a state of readiness to detect). The main result of this note is that pointer variables exist that satisfy further the probabilistic laws of quantum measurement. (It seems that the
condition of being a pointer variable puts strong constraints on the $f_n$.) We now define the family $f_n$, which in the limit, becomes the pointer position of the measuring apparatus. There is a basis of $\mathcal{H}_n$ consisting of separable vectors of the form $\psi_\epsilon \otimes |i>$ where $\epsilon$ is 0 or 1. Let $C$ be the set of basis vectors such that all but a negligible number of the $d_i$ for $i < n/2$ are 1 and all but a negligible number of the others are 0. (This is the device being ‘cocked’ and ready to detect. It is very far from being a stable state, in the limit.) By negligible, we mean that as a proportion of $n$, it goes to zero as $n$ increases. For $w_n$ any norm one state of the combined system, let $c_i$ be the Fourier coefficients of $w_n$ with respect to the cocked basis vectors, i.e., those in $C$. Define $f_n(w_n) = 1 - \sum_i |c_i|^2$.

Now we are interested in phenomena in the limit as $n$ approaches $\infty$, yet one cannot directly compare the arguments of $f_n$ with those of $f_{n+1}$. Instead of comparing individual values of these functions, one compares time or phase averages of the various $f_n$ as $n$ varies, in keeping with the procedures of classical Statistical Mechanics. (Phase averages would be taken over the submanifold of accessible states, it is more convenient for us to deal with time averages. Time averages have been made the basis for von Plato’s theory[25] of the meaning of probability statements.) For any abelian dynamical variable $f$, define $<f> = \lim_{T \to \infty} \frac{1}{T} \int_0^T f(e^{\frac{2\pi i}{T}H_n} \cdot v)dt$ possibly depending on $v$.

Why time averages?

The Gibbs program usually starts here by postulating a canonical distribution of some kind and considering phase averages. Ford–Kac–Mazur do the same. Work of Mazur and Kim following on the above has shown that in the thermodynamic limit, the result is largely independent of the initial probability distribution postulated. The heat bath need not even be in equilibrium. It is widely felt, therefore, that something deeper is going on
here. Wiener always used time averages to define the auto-correlation function, since for ergodic stationary stochastic processes these agree with phase averages. If the underlying deterministic dynamics were ergodic, the time averages would be almost independent of $v$ and equal to the phase averages. Khintchine has shown that for a certain class of abelian dynamical variables, even if the dynamics is not ergodic, the phase averages in the limit will be equal to the limit of the time averages for those particular dynamical variables. Since we are passing to the thermodynamic limit anyway, the results of Mazur, Kim, and others support Khintchine’s insight, although the exact range of validity of this principle remains unknown.

It is standard practice in theoretical physics to prefer time averages to phase averages for probabilities, and so if we take as our goal, to derive the probabilities of quantum mechanics from the first three axioms, we need to calculate time averages. The standard textbook of Landau–Lifschitz for example says that probabilities are infinite time averages, and the probability that a system will be found in the region $M$ of phase space is, by definition,

$$\lim_{T \to \infty} \frac{1}{T} \int_0^T \chi_M(v(t))dt,$$

where $\chi_M$ is the characteristic function of the measurable set $M$. (This can be criticised from a foundational point of view, but it should only be replaced by a definition that yields the same answers because the standard procedure is well-supported experimentally . . . ) and therefore we study the thermodynamic limits of time averages.

The reason Wiener always studied time-averages was because, as he and, after him, Gelfand, pointed out, the idea of measurement in classical Statistical Mechanics is mod-
elled by a time average, since the characteristic relaxation time of the measuring device, which is macroscopic compared to the individual degrees of freedom, is almost infinite by comparison with the time scale of the underlying deterministic dynamics. von Plato traces this idea to Einstein, and von Plato’s ergodic theory of the meaning of probability makes the probabilistic expectation of the value of $f$ equal by definition to the time average of $f$.

There is no use trying to devise a probability distribution for the states of the amplifying apparatus as a whole because we control quite a few parameters. In such a situation it is standard[17], p. 50f., procedure to introduce the submanifold of “accessible states.” We know that the amplifying apparatus is in a particular sort of state, a state of readiness to detect. Only a small subspace, $C$, of the total phase space is accessible to the apparatus, then. It is easy to see that our results are completely independent of which vector in $C$ we start at. The question of what probability distribution on the states of the amplifier could be derived by analogy to Khintchine’s derivation of the canonical distribution, but carried out for negative temperatures instead of equilibrium, is therefore irrelevant to the question at hand, although of sufficient interest in its own right. It would, however, remove us from the only published coherent theory of the meaning of probability assertions that is closely tied to the frequency theory, von Plato’s ergodic theory of probability.

Let the incident particle be in the state described by any (normalised) wave function in $\mathbb{C}^2$. Let it be $v_0 = a_0\psi_0 + a_1\psi_1$. The amplifier is in the state $|111\ldots 000\rangle$ in $C$.

Consider $f_\infty = \lim_{n \to \infty} < f_n >$. Consider a typical trajectory in the manifold of accessible states inside of $\mathcal{H}_n^{com}$ (i.e., states which the dynamical system can reach starting from a state in $C$). It is elementary to calculate $\lim_{n \to \infty} < f_n >$, it is $|a_1|^2$. (Nothing is
altered if we suppose the amplifier is in a mixed state given by some probability distribution supported on the span of $C$.)

Why define this $f_n$?

Up until now it has been thought impossible for a pointer variable to exist for any model of this type that will agree with the probabilistic axioms. We are about to succeed in showing that this is not true, it is possible. How unique is it, and what other macroscopic variables will *not* verify those axioms is a question which is too large to answer yet. To be a macroscopic variable at all is a strong constraint, it means the limit of the sequence

$$g(v_o, v_1, v_2, \ldots) = \lim_{n \to \infty} f_n(v_o \otimes v_1 \otimes v_2 \cdots \otimes v_n)$$

is a tail event in the theory of probability, and there seems to be some kind of zero-one law which governs these. But tail events are highly non-unique, and even if we add the constraint of being a pointer variable in the sense that the macroscopic variable must be correlated with the microscopic particle’s incoming sharp states, it is still non-unique. We could, for example, take an indicator function on the manifold $C$ and get a pointer variable that would disobey the probability axioms.

There are intuitive reasons for selecting the sequence of $f_n$ which we explicitly constructed. The first one, which rules out the indicator function, is that the phase functions in Classical Statistical Mechanics with physical significance are supported on sets of positive measure, not zero or negligible measure. The second one is that in the debate, which exists, about whether the wave function is physical or not, one notices that although the wave function may be rather difficult to measure completely with only one measurement, at least one can get a physical grip on the $|c_i|^2$'s, they can be measured by repeating exper-
iments since they are, after all, operationalised as frequencies. So a physically significant phase function could be constructed out of those amplitudes. Thirdly, the classical phase functions which have thermodynamic significance are sum functions, sums of identical functions of each degree of freedom, so it is natural to study something like the $f_n$.

Continuing

We will next find a classical dynamical system $\Omega_\infty$ which has a mixed state $X$, which depends on $v_0$, and a classical dynamical variable $F$ whose expectation values match these limits.

We search for $\Omega_\infty$, $F$, and $X_{v_0}$ as above, satisfying

$$\int_{\Omega_\infty} FdX_{v_0} = \lim_{n \to \infty} <f_n>.$$  

Let the state of the (classical limit) measurement apparatus where the pointer position points to cocked (and hence, an absence of detection) be the point $P_0$. Let the Ming state where the excited states of the apparatus are proceeding from out of its initial cocked state, and travelling steadily towards the right, be $P_1$. Then $\Omega_\infty = \{P_0, P_1\}$. The dynamical variables on this space are generated by the characteristic functions of the two points, $\chi_{P_0}, \chi_{P_1}$. Let $F$ be $\chi_{P_1}$. It is the pointer position which registers detection. The mixed state of $\Omega_\infty = \{P_0, P_1\}$ which gives the right answer when the incident particle is in state $v_0$ is the probability distribution which gives $P_0$ the weight $|a_0|^2$, and $P_1$ the weight $|a_1|^2$. This is precisely what it means to say the the measuring apparatus will register the presence of the particle with probability $|a_1|^2$, and fail to register with probability $|a_0|^2$.

As the discussion of foundational matters in Khintchine[17] makes clear, thermodynamic limits do not really exist, and they need not obey the laws of physics. They are
merely convenient devices for organising our thoughts about calculating clever approximations to answering questions about finite systems with a large but fixed number of degrees of freedom. Since many approximation techniques for large \( n \) are asymptotic expressions which diverge, it is merely a technical convenience to re-normalise or re-scale the question with \( n \) to introduce convergence. There was no physical significance to the divergence since \( n \) is not in fact a variable but a parameter. We renormalised Planck’s constant \( h \) to be equal to zero in the limit, by letting it be proportional to \( \frac{1}{n} \).

Of course there are many properties of \( H_{1.1 \times 10^{23}} \) which can not be well-studied by neglecting \( h \). But we have just proved that \( < f_n > \) is not one of them. Thus, although there are many topics in the physics of amplifying apparatus which would be poorly served by taking the limit as \( h \to 0 \), the pointer variable is not one of them. Bohr had this idea, in words. He said that the pointer position was classically describable, meaning that we should be able to study it as part of the physical description of the apparatus which does not vary appreciably if we neglect Planck’s constant. The method introduced by this note is the only way yet known to make this precise. It, by coincidence, agrees perfectly with the method of Khintchine and others following the Gibbs program.

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