Chapter 11
The measurement problem

The measurement problem of quantum mechanics was probably born in 1926:

> 'Thus Schrödinger's quantum mechanics gives a very definite answer to the question of the outcome of a collision; however, this does not involve any causal relationship. One obtains *no* answer to the question "what is the state after the collision," but only to the question "how probable is a specific outcome of the collision" (in which the quantum-mechanical law of [conservation of] energy must of course be satisfied). This raises the entire problem of determinism. From the standpoint of our quantum mechanics, there is no quantity that could causally establish the outcome of a collision in each individual case; however, so far we are not aware of any experimental clue to the effect that there are internal properties of atoms that enforce some particular outcome. Should we hope to discover such properties that determine individual outcomes later (perhaps phases of the internal atomic motions)? Or should we believe that the agreement between theory and experiment concerning our inability to give conditions for a causal course of events is some pre-established harmony that is based on the non-existence of such conditions? I myself tend to relinquish determinism in the atomic world. But this is [also] a philosophical question, for which physical arguments alone are not decisive.' (Born, 1926a, p. 866; translation by the author)

In other words, quantum mechanics stipulates that the state after some collision (or measurement) is $\psi = \sum_n c_n \psi_n$, whereas experiment demonstrates that in fact the final state is just one of the ψ_n, with (Born) probability $|c_n|^2$. Quantum mechanics, then, seems unable to account for single outcomes of experiments and has to satisfy physicists with merely probabilistic predictions. This, in a nutshell, is the measurement problem—although very substantial analysis is needed to flesh it out.

Giving up determinism was soon incorporated in the Copenhagen Interpretation of Bohr and Heisenberg (cf. the Introduction) and more broadly became part of what might be called "***orthodoxy***", which represents the apparent (but not actual) consensus among Bohr, Heisenberg, Pauli, Born, Jordan, Dirac, von Neumann, and many others, which they supposedly reached around 1930 after the formal completion of quantum mechanics. This "orthodoxy", which later gave rise to the unfortunate "shut up and calculate" attitude most physicists seem to have (especially towards the measurement problem), should be distinguished from the Copenhagen Interpretation. For example, von Neumann never endorsed the doctrine of classical concepts, which in the above attitude has been replaced by the different and far more superficial idea that it is the entire goal of physics to explain experiments.

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11.1 The rise of orthodoxy

Even within the strict Copenhagen Interpretation, there were sharp differences between Bohr and Heisenberg, beyond the one concerning classical concepts reviewed in the Introduction. However, it seems that they agreed about the following point made by Bohr in his Como lecture concerning measurement:

> 'According to the quantum theory, just the impossibility of neglecting the interaction with the agency of measurement means that every observation introduces a new uncontrollable element.' (Bohr, 1928, p. 584)

This placed measurement squarely outside quantum mechanics for the second time: the first time was in the insistence that the measurement device ("if it is to serve its purpose") had to be described classically (cf. the Introduction), and now we also learn that the interaction between the quantum object undergoing measurement and the apparatus in question is "uncontrollable", *despite the fact that Bohr and Heisenberg regarded quantum mechanics as a complete theory*: their argument was apparently that precisely the classical nature of the apparatus makes the interaction uncontrollable. This in turn justified the classical description of the device, in that registration of a measurement result ought to be "objective", so that reading it out by performing a measurement on the apparatus, so to speak, should not introduce any further disturbance and hence uncontrollability (or so the argument goes).

Consistent with Bohr's point, a more detailed conceptual analysis of the measurement process was given by Heisenberg (1958, pp. 46–47, 54–55), who consistently refers to the quantum state or wave-function as the "probability function":

> 'Therefore, the theoretical interpretation of an experiment requires three distinct steps:
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> 1. the translation of the initial experimental situation into a probability function;
> 2. the following up of this function in the course of time;
> 3. the statement of a new measurement to be made of the system, the result of which can then be calculated from the probability function.
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> (...) After [the] interaction [with the measuring device] has taken place, the probability function contains the objective element of tendency and the subjective element of incomplete knowledge, even if it has been a "pure case" before [i.e., it has become a mixture]. It is for this reason that the result of the observation cannot generally be predicted with certainty; what can be predicted is the probability of a certain result of the observation, and this statement about the probability can be checked by repeating the experiment many times. (...) The observation itself [i.e., the act of registration of the result by the mind of the observer] changes the probability function discontinuously; it selects of all possible events the actual one that has taken place. Since through the observation our knowledge of the system has changed discontinuously, its mathematical representation also has undergone the discontinuous change and we speak of a "quantum jump."

Here we find the typical Copenhagen view of measurement as a two-step process:

1. Measurement turns an initial pure state (of the measured object) into a mixture;
2. One term in this mixture is singled out (by Nature and thence by the observer).

Note that Heisenberg's last comment puts him squarely into the camp of what is now called "QBism" (i.e., ***Quantum Bayesianism***, see §11.2 below)!

Von Neumann (1932, §VI.1) gave a more formal (and highly influential) presentation of the (alleged) two stages of the measurement process:

> 'In the discussion so far we have treated the relation of quantum mechanics to the various causal and statistical methods of describing nature. In the course of this we found a peculiar dual nature of the quantum mechanical procedure which could not be satisfactorily explained. Namely, we found that on the one hand a state ϕ is transformed into the state ϕ' under the action of an energy operator H in the time interval $0 \leq \tau \leq t$:
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> $$\frac{\partial}{\partial \tau}\phi_\tau = -\frac{2\pi i}{h}\mathsf{H}\phi_\tau \;:\; 0 \leq \tau \leq t$$
>
> so if we write $\phi_0 = \phi$, $\phi_t = \phi'$ then $\phi' = e^{-\frac{2\pi i}{h}t\mathsf{H}}\phi$, which is purely causal. A mixture U is correspondingly transformed into
>
> $$\mathsf{U}' = e^{-\frac{2\pi i}{h}t\mathsf{H}}\mathsf{U}e^{+\frac{2\pi i}{h}t\mathsf{H}}$$
>
> Therefore, as a consequence of the causal change of ϕ into ϕ' the [pure] states $\mathsf{U} = \mathsf{P}_{[\phi]}$ [$=|\phi\rangle\langle\phi|$] go over into the [pure] states $\mathsf{U}' = \mathsf{P}_{[\phi']}$ (process **2** in V.1.). On the other hand, the state ϕ—which may measure a quantity with discrete spectrum, distinct eigenvalues and eigenfunctions $\phi_1, \phi_2, \ldots$—undergoes in a measurement a non-causal change in which each of the states ϕ_1, ϕ_2, ... can result, and in fact does result with the respective probabilities $|\langle\phi,\phi_1\rangle|^2$, $|\langle\phi,\phi_2\rangle|^2$, ... That is, the mixture
>
> $$\mathsf{U}' = \sum_{n=1}^{\infty} |\langle\phi,\phi_n\rangle|^2 \mathsf{P}_{[\phi']}$$
>
> obtains (...) (process **1** in V.1.). Since the [pure] states [i.e. $\mathsf{P}_{[\phi]}$] go over into mixtures, the process is not causal. The difference between these two processes $\mathsf{U} \mapsto \mathsf{U}'$ is a very fundamental one: aside from their different behaviors in regard to the principle of causality, they are also different in that the former is (thermodynamically) reversible, while the latter is not.' (pp. 417–418 in von Neumann (1955); translation: R.T. Beyer)

All this concerns merely the first stage of the measurement, in which a pure state is transformed into a mixed one. The second stage, in which a single outcome is obtained, is already alluded to above (though clouded by von Neumann's ensemble language), but is described (in prose) later on through what is now called a ***von Neumann chain***: one redefines system plus apparatus as the system, and couples it to a new apparatus, etc. This chain supposedly ends with the "ego" of the "individual" whose "intellectual inner life" is finally responsible for a single outcome.

It is very remarkable that von Neumann nowhere seems to use the central Copenhagen dogma that the apparatus be described classically (cf. the Introduction), especially since the mathematics of operator algebras he was inventing at almost exactly the same time is tailor-made for incorporating this dogma (which fact indeed forms the motivation for the present book). One clue for his lack of enthusiasm may come from the very end of his book (i.e., §VI.3), where he challenges 'an explanation often proposed to account for the statistical character of the process 1', namely the idea that (the non-unitary) process 1 might have its origin in an initial mixed state of the apparatus. Indeed, even if the apparatus as a quantum-mechanical system is in a pure state (as any system should be ontologically), its description as a classical system generally renders its state mixed—and the same conclusion may be drawn on epistemic grounds, arguing that the state of macroscopic or otherwise complicated systems cannot be known exactly. Many writings by the Copenhagen school, then, suggest that the alleged unanalyzable nature of the measurement and the randomness of its outcome should be attributed to the classical description of the apparatus and its ensuing mixed state, including our earlier quotation (cf. §8.4) from Heisenberg (1958) on the origin of probabilities in quantum mechanics:

> 'these uncertainties (...) are simply a consequence of the fact that we describe the experiment in terms of classical physics' (Heisenberg, 1958, p. 53)

To counter this argument, von Neumann argues that physics requires the (Born) probabilities for the various outcomes to depend only on the initial state ϕ of the quantum system undergoing measurement (as opposed to the state of the apparatus, be it classical or quantum), whereas any "process 2" (i.e. unitary) time evolution would merely push the coefficients w_n in the (alleged) mixed apparatus state into the role of probabilities for the possible outcomes. However, 'the w_n are characteristic of the observer alone (and therefore independent of ϕ)', and hence

> 'the non-causal nature of the process 1. is not produced by any incomplete knowledge of the state of the observer.' (von Neumann, 1955, p. 439).

Von Neumann's argument became the mother of all "insolubility theorems" for the measurement problem, some of which will be reviewed in §11.3 below.

Pauli (1933, §9) also includes some comments on measurement and the interpretation of quantum mechanics in general. These display a bizarre hybrid between the ideas of Bohr and von Neumann, somehow mediated by Heisenberg. Thus Pauli endorses (even starts with) some notion of Complementarity, but he relates this to the mathematical formalism rather than to the doctrine of classical concepts (which he nowhere invokes). Similarly, his treatment of measurement on the one hand follows the disturbance ideology of Bohr and Heisenberg (but without grounding this in the classical description of the apparatus), whilst technically he quotes and follows von Neumann, claiming that measurement leads to mixtures which subsequently reduce to one term through '*ein besonderer, naturgesetzlich nicht im Voraus determinierter Akt*' (i.e., special process that does not follow deterministic laws of nature). A rather more systematic review of early measurement theory was written by London & Bauer (1939), whose opening is highly promising and almost poetic:

> 'The majority of introductions to quantum mechanics follow a rather dogmatic path from the moment that they reach the statistical interpretation of the theory. In general they are content to show, by more or less intuitive considerations, how the actual measuring devices always introduce an element of indeterminism, as this interpretation demands. However, care is rarely taken to verify explicitly that the formalism of the theory, applied to that special process which constitutes the measurement, truly implies a transition of the system under study to a state of affairs less fully determined than before. A certain uneasiness arises. One does not see exactly with what right and up to what point one may, in spite of this loss of determinism, attribute to the system an appropriate state of its own. Physicists are to some extent sleepwalkers, who try to avoid such issues and try to concentrate on concrete problems. But it is exactly these questions of principle which nevertheless interest nonphysicists and all who wish to understand what modern physics says about the analysis of the act of observation itself.' (London & Bauer, 1939, pp. 218-219)

Yet the authors mainly repeat von Neumann's analysis (confirming its lofty status):

> 'The interaction with the apparatus does not put the object into a new pure state. Alone, it does not confer to the object a new wave function. On the contrary, it actually gives nothing but a statistical mixture: It leads to one mixture for the object and one mixture for the apparatus. For either system regarded individually there results uncertainty, incomplete knowledge. Yet nothing prevents our reducing this uncertainty by further observation.
>
> And this is our opportunity. So far we have only coupled one apparatus with one object. But a coupling even with a measuring device is not yet a measurement. A measurement is achieved only when the position of the pointer has been observed. It is precisely the increase of knowledge, acquired by the observation, that gives the observer the right to choose among the different components of the mixture predicted by the theory, to reject those which are not observed, and to attribute thenceforth to the object a new wave function, that of the pure case which he has found. We note the essential role played by the consciousness of the observer in this transition from the mixture to the pure state. Without his effective intervention, one would never obtain a new ψ function.' (*ibid.*, p. 251)

Accordingly, at the end of the golden era of quantum mechanics, the view of measurement as a two-stage process in which a pure state is first transformed into a mixture in a more or less scientific way, upon which unanalyzable and possibly mental phenomena bring about a single outcome, was firmly established, although—the point deserves to be repeated—in their formal treatments neither von Neumann nor London & Bauer incorporated the key claim Bohr and Heisenberg made about measurement, namely that the corresponding apparatus *must* be described classically.

Opponents of the Copenhagen Interpretation (the most prominent among whom were Einstein and Schrödinger) were well aware of this tension between formalism and ideology, which in the form of ***Schrödinger's Cat*** even reached immortality (!):

> 'One may also construct highly burlesque cases. A cat is confined in a box of steel together with the following hellish machine (which one should secure against a direct attack by the cat): A Geiger counter contains a tiny amount of radioactive material, *so* little that during one hour *possibly* one of its atoms decays, but equally likely also none does; if it does, then the counter is triggered and activates, via a relais, a little hammer which breaks a small container of hydrocyanic acid. Having left this system to itself for one hour, one will say that the cat is still alive *if* meanwhile no atom has decayed. The first decay of an atom would have poisoned her. The ψ-function of the entire system would express this in such a way that in it the living and the dead cat would be mixed or spread out on equal terms. What is typical about these cases is that an uncertainty which is originally limited to the atomic domain has been transformed into a coarse-grained uncertainty, which may then be *decided* by direct observation. This prevents us from regarding a "faded model" as an image of reality in such a naive way. As such [this model] contains nothing that is unclear or contradictory. There is a difference between a moved or poorly focused photograph and a record of clouds and fog banks.' (Schrödinger, 1935, p. 812; translation by the author)

The last sentence is particularly powerful, contrasting Schrödinger's (as well as Einstein's) view that physics should describe some sharply defined reality (of which quantum mechanics at best produces blurred pictures) with the Copenhagen view, according to which reality itself lacks focus (with quantum mechanics providing the best possible picture of it). This contrast confirms our idea that Schrödinger's Cat metaphor specifically draws attention to the problems that arise from the Copenhagen "duality postulate" that macroscopic systems (such as measurement devices and cats) admit both a classical and a quantum-mechanical description.

11.2 The rise of modernity: Swiss approach and Decoherence

Despite Schrödinger's Cat, the measurement problem was not an active field of research until Wigner (1963) rekindled interest in the topic. Even so, his paper mainly reiterated von Neumann's views—which already had been repeated by London and Bauer—including his omission of the doctrine of classical concepts. In particular, it continued to promulgate the suggestion that measurement is a two-step process for which the clarification of the first step (i.e. of turning a pure state into a mixture) would already be a major part of the solution of the measurement problem.

Wigner's paper inspired for example the "'Swiss" approach to the measurement problem, which was remarkable in being the first serious mathematical attempt to take into account the Bohr–Heisenberg dogma that the apparatus be described classically, whilst also paying tribute to von Neumann in insisting on mathematical rigour. Indeed, the Swiss approach relies on the formalism of operator algebras, which also marks a conceptual break with all earlier—and indeed most later—approaches in taking the observables rather than the states as a starting point. The aim of the Swiss approach is to show that relative to a suitable class of observables, the pure state

$$\rho = |\psi\rangle\langle\psi|, \;\; \psi = \sum c_n \psi_n,$$

coincides with the corresponding mixture without the off-diagonal terms, i.e.,

$$\rho' = \sum_n |c_n|^2 |\psi_n\rangle\langle\psi_n|.$$

Thus the ambition of this approach is limited, in that no attempt is made to explain (at least the appearance of) single outcomes, except by appealing to the ignorance interpretation of probability (in vain, see below). The alleged equivalence between pure states and mixtures can typically be achieved if the apparatus is infinite and the measurement time is infinite, too. The infinite character of the apparatus (here seen as an idealization of a macroscopic device, as is standard in quantum statistical mechanics), is no guarantee for its classicality, but it is certainly a step in the right direction (cf. Chapter 8). Thus two closely related problems must be overcome:

1. In its reliance on superselection sectors (technically, on disjoint states on a suitable algebra of observables of the apparatus, see Definition 8.18), the program only works in the limit of infinite apparatus and infinite measurement time. Indeed, any approximation ruins the equivalence between pure states and mixtures; and hence even this limited solution to the problem violates Earman's Principle.
2. In so far as the subsequent problem of obtaining single outcomes to measurement is recognized in the Swiss approach at all, it seems to be addressed by an appeal to the ignorance interpretation of probability. Despite the fact that the mathematical situation in this respect is better than in ordinary quantum mechanics (where the ignorance interpretation of the formal probability distribution given by the coefficients in a diagonal density operator is nonsensical, if only because the state space is not a simplex), there is still no valid argument for this move.

To explain the last point, we quote Leggett (though somewhat out of context):

> 'Now, following Schrödinger, let us consider a thought experiment in which the quantum-mechanical description of the final state, as obtained by appropriate solution of the time dependent Schrdinger equation, contains simultaneously nonzero probability amplitudes for two or more states of the universe that are, by some reasonable criterion, macroscopically distinct (in Schrödingers example, this would be "cat alive" and "cat dead"). Of course, just about everyone, including me, would accept that because of, inter alia, the effects of decoherence, it is likely to be impossible, at least for the foreseeable future, to experimentally demonstrate the interference of such states. (On the other hand, as the late John Bell was fond of pointing out, the foreseeable future is not a very well-defined concept. In fact, as late as 1999, not a few people were confidently arguing that because of the inevitable effects of decoherence, the projected experiments to demonstrate interference at the level of flux qubits would never work. In this case, the foreseeable future lasted approximately one year. As Bell used to emphasize, the answers to fundamental interpretive questions should not depend on the accident of what is or is not currently technologically feasible.) But the crucial point is that the formalism of quantum mechanics itself has changed not one whit between the microscopic and macroscopic levels. Are we then entitled to embrace, at the macrolevel, an interpretation that was forbidden at the microlevel, simply because the evidence against it is no longer available? I would argue very strongly that we are not, and would therefore draw the conclusion: also at the macrolevel, when the quantum-mechanical description assigns simultaneously nonzero [probabilities] to two or more macroscopically distinct possibilities, then it is not the case that each system of the relevant ensemble realizes either one possibility or the other.' (Leggett, in Schlosshauer, 2011, p. 155)

This argument of Leggett's (which is a special case of Earman's Principle) was originally targeted at decoherence, but it also applies *verbatim* to the Swiss approach (which is closely related to decoherence, as both heavily rely on limits and superselection rules—which are absolute in the former and dynamically induced in the latter). In an even earlier hunch of Earman's Principle, Bell— this time aiming directly at the Swiss approach—in fact made a related point about its reliance on the $t \to \infty$ limit (in that even at extremely large but finite time the state remains pure).

Jumping to the modern era, a striking point of continuity with the 1920s and 1930s is the idea that the measurement procedure (and hence the measurement problem) consists of two stages; only the terminology and the scope have changed:

> 'There are two distinct measurement problems in quantum mechanics: what Pitowsky has called a "big" measurement problem and a "small" measurement problem. The "big" measurement problem is the problem of explaining how measurements can have definite outcomes, given the unitary dynamics of the theory: it is the problem of explaining how individual measurement outcomes come about dynamically. The "small" measurement problem is the problem of accounting for our familiar experience of a classical, or Boolean, macroworld, given the non-Boolean character of the underlying quantum event space: it is the problem of explaining the dynamical emergence of an effectively classical probability.' (Bub, in Schlosshauer, 2011, pp. 145–146)

Clearly, the "small" measurement problem is modern parlance for the problem how to turn a superposition into a mixture, upon which the "big" problem—if it is noticed at all—still concerns the old issue of selecting *one* term from this mixture.

Furthermore, the measurement problem seems to have acquired increased scope and importance, as exemplified by the following quotations:

> 'One of the most ancient philosophical questions (Heidegger thought is was *the* question) is this: why is there something rather than nothing? In terms of events rather than substances, the question would be: how come anything happens at all? That question is the measurement problem.' (Fine, in Schlosshauer, 2011, p. 146)

> 'The measurement problem has been called "the reality problem" by Philip Pearle. This is a better name for it. We perceive objects in the world as being in definite states. A door is either open or shut, a given ball either is in a given box or it is not. The wave function, however, can have superpositions of these things, suggesting that the door can be simultaneously open and shut at the same time, and that the ball can be both in the box and not in the box at the same time. The reality problem is that there is a discrepancy between the version of reality we perceive, and the version presented to us by the most obvious interpretation of the wave function.' (Hardy, in Schlosshauer, 2011, p. 153)

> 'Fundamentally, the measurement problem is the problem of connecting probability with truth in the quantum world, that is to say, it is the problem of how to relate quantum probabilities to the objective occurrence and non-occurrence of events. The problem arises because there appears to be a difficulty in reconciling the objectivity of a particular measurement outcome with the entangled state at the end of a measurement.' (Bub, *ibid.*, p. 145)

More technically, the measurement problem has come to be seen as a special case of the problem of explaining at least the *appearance* of the classical world from quantum theory. If the measurement problem is seen from the Copenhagen perspective this is eminently reasonable, as both problems involve the dual description of either the apparatus or the world around us as both classical and quantum (and its possible failure). In this context, an alleged solution to the "small" problem, such as Decoherence, is often also seen as this explanation (as if there were no issue about the derivation of the laws of classical physics, including the dynamical ones).

A propos, another characteristic feature of the modern era is undoubtedly the dominance of ***Decoherence*** (if only over the Swiss approach), for example:

> 'I think the whole discussion about whether measurements in quantum mechanics are indeed problematic somewhat misses the point. Measurement interactions are only one of many examples of quantum interactions that lead to superpositions of macroscopically distinct states. Nature has been producing macroscopic superpositions for millions of years, well before any quantum physicist cared to artificially engineer such a situation. The key concept here is decoherence. Environmental interactions tend to produce superpositions of classically distinct states. This raises the issue of how one could describe a classical regime in quantum mechanics, quite irrespective of the existence of measuring apparatuses. (...)
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> If decoherence and its applications had been developed early in the history of quantum theory, then the idea that measurements play a special role in the theory might not have risen to such prominence, and the foundations of quantum mechanics would have focused instead on the problem of how to derive a classical regime within the theory.'
> (Bacciagaluppi, in Schlosshauer, 2011, p. 143)

Mathematically, decoherence boils down to the idea of adding one more link to the von Neumann chain (see §11.1) beyond $S+A$ (i.e. the system and the apparatus). Conceptually, however, there is a fundamental conceptual as well as technical difference between Decoherence and older approaches that took such a step: whereas previously (e.g., in the hands of von Neumann, London & Bauer, and Wigner) the chain *converged towards the observer*, in Decoherence it *diverges away from the observer*. Namely, the third and final link is now taken to be the ***environment***.

This notion is often taken in a fairly literal sense in agreement with the intuitive meaning of the word, but it may also (we would even say: preferably) refer to internal degrees of freedom of the apparatus, as in the Spehner–Haake model in §11.4. Either way, the "environment" is usually treated as an infinite system (necessitating a limit like $N \to \infty$), which (in simple models where the pointer has discrete spectrum) has the consequence that the post-measurement state $\sum_n c_n \psi_n \otimes \phi_n \otimes \chi_n$ (in which the χ_n are mutually orthogonal) is only reached not only in the limit $N \to \infty$ of infinitely many degrees of freedom but also in the limit $t \to \infty$ of infinite time. In that case, the restriction of the above state to $S+A$ (i.e. the trace of the corresponding density operator over the degrees of freedom of the environment) is mixed, which means that the quantum-mechanical interference between the states $\psi_n \otimes \phi_n$ for different values of n has become "delocalized" to the environment, and accordingly is deemed irrelevant if the latter is not observed (i.e. omitted from the description).

Unfortunately, in so far as it claims to provide a solution to the measurement problem, Decoherence is an unmitigated disaster:

1. Decoherence actually *aggravates* the measurement problem: where previously this problem was believed to be man-made and relevant only to rather unusual laboratory situations, it has now become clear that "measurement" of a quantum system *by the environment* (instead of by an experimental physicist) happens everywhere and all the time: hence it remains even more miraculous than before that there is a single outcome after each such measurement.
2. Even the need for *one* of the two limits $N \to \infty$ or $t \to \infty$ makes Decoherence vulnerable to Earman's Principle; see Bell's and Leggett's critiques above.
3. Like the Swiss approach, Decoherence suffers from the difficulty that even if it were able to reach its goal of reducing pure states to mixtures (about which ability one may have doubts), there is no sound follow-up step to solve the next problem of selecting one term from the mixture produced in the previous step. The ignorance interpretation seems blocked by Leggett's argument quoted above (i.e. his continuity argument to the effect that Decoherence just removes the *evidence* for a given Schrödinger's cat state to be a superposition, elsewhere charging those claiming that Decoherence solves the measurement problem of committing the logical fallacy that removal of the evidence for a crime would undo the crime).

Thus Decoherence is parasitic on some interpretation of quantum mechanics that solves the measurement problem, which in turn is typically strengthened by it. In this context, the most popular of these has been the Everett (i.e., Many-Worlds) Interpretation, which, after decades of obscurity or even derision, suddenly started to be greeted with a flourish of trumpets in the wake of the popularity of Decoherence. However, even if such extravagant interpretations are coherent, these should in our opinion be a very last resort, acceptable only if truly everything else has failed.

On the positive side, Decoherence has led to the important idea of ***einselection*** (for *environment-induced superselection*), where a pure state ψ of some system (possibly plus apparatus) is "einselected" if it remains pure after coupling to the environment and subsequent restriction. The hope (or rather program), then, is to show that classical states are classical precisely because they are robust in this way.

Finally, it may be appropriate to close this historical introduction to the measurement problem by mentioning another modern approach, namely outright *denial*:

> 'I remember giving a talk at a meeting at the London School of Economics seven or so years ago. In the audience was an Oxford philosophy professor, and I suppose he didn't much like my brash cowboy dismissal of a good bit of his life's work. When the question session came around, he took me to task with the most proper and polite scorn I had ever heard (I guess that's what they do). "Excuse me. You seem to have made an important point in your talk, and I want to make sure that I have not misunderstood anything. Are you saying that you have solved the measurement problem? This problem that has plagued quantum mechanics for seventy-five years? The message of your talk is that, using quantum information theory, you have finally solved it?" (Funny the way the words could be put together as a question, but have no intended usage but as a statement.) I don't know that I did anything but turn the screw on him a bit further, but I remember my answer. "No, not me; I havent done anything. What I am saying is that a "measurement problem" never existed in the first place. (...)
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> The "measurement problem" is purely an artefact of a wrong-headed view of what quantum states and/or quantum probabilities ought to be. (...) quantum states are not real things from a Quantum Bayesian view (...) but a personal judgment, a quantified degree of belief. A quantum state is a set of numbers an agent uses to guide the gambles he might take on the consequences of his potential interactions with a quantum system. It has no more substantiality than that. Aren't epistemic states real things? Well ... yes, in a way. They are as real as the people who hold them. But no one would consider a person to be a property of the quantum system he happens to be contemplating. And one shouldn't think of a quantum state in that way either—one shouldnt think of it as a property of the quantum system to which it is assigned. Take the source of the paradox away, we say, and the paradox itself will go away.' (Fuchs, in Schlosshauer, 2011, pp. 146–147)

These words have been quoted at some length, because the view that "physics is information" and its alleged corollary that all foundational problems are solved by Bayesian reasoning (perhaps with a quantum flavour) is becoming increasingly popular. Physicist are now seen as punters (or, in academic parlance, "agents") who in smoky offices bet on the outcomes of experiments, and hence use (quantum) Dutch Book arguments to justify some sort of strictly epistemic (quantum) probability calculus. However, the ideology of "***QBism***" thus expressed appears to have adopted precisely the weakest ingredients of the Copenhagen Interpretation—viz. the idea that the wave-function is just a catalogue of the probabilities for possible outcomes of measurements whose details are supposedly beyond our grasp, cf. the Introduction—at the expense of its one strong component, namely the doctrine of classical concepts. Although there may have been pragmatic reasons for this attitude in the 1920s, (mathematical) physics has moved forward since then, enabling much more detailed analysis and hence justifying considerably greater ambition in understanding the measurement process than Bohr and Heisenberg *cum suis* had.

In any case, the fact that one competent author regards the measurement problem as the key to reality whilst another flatly denies even its very existence should give pause for thought. As in the Bohr–Einstein debate, different perspectives on reality and on the task of physics seem to play a role here, culminating in contrasting views of quantum-mechanical states: the more "reality" one attributes to states, the more serious the measurement problem is. Or, contrapositively, the more operationalist one's attitude, the further the problem disappears behind the horizon.

11.3 Insolubility theorems

Since in §11.4 we will "propose the impossible", namely miraculously solving the measurement problem within unitary quantum mechanics, it is helpful to review the arguments why this is generally felt to be impossible. Such arguments take the form of so-called ***insolubility theorems***. As already mentioned, such theorems ultimately go back to von Neumann: especially those that prove the impossibility of explaining his process 1 (i.e. the transition from a pure state to a mixture) from process 2 (unitary time evolution according to the Schrödinger equation). Another kind of insolubility theorem shows that single outcomes are impossible from process 2.

It might be argued that both kinds of theorem add little to the basic mathematical intuition behind the measurement problem, which is as follows (it goes without saying that we disagree with this traditional description of measurement, see below). Let $s \in B(H_S)$ be the observable being measured (where H_S is some Hilbert space associated to a quantum object S undergoing measurement) and let $a \in B(H_A)$ be a "pointer observable" correlated to S (where H_A is a second Hilbert space). In particular, the measurement apparatus A is described quantum mechanically. For the moment we assume both Hilbert spaces to be finite-dimensional and both operators to be non-degenerate, even having the same spectrum $\{\lambda_1,\ldots,\lambda_n\}$; this of course implies that $\dim(H_S) = \dim(H_A) = n$. Thus H_S has a basis $(\upsilon_i^{(s)})$ of eigenvectors of s and likewise H_A has a basis $(\upsilon_i^{(a)})$ of eigenvectors of a, with $s\upsilon_i^{(s)} = \lambda_i \upsilon_i^{(s)}$ and $a\upsilon_i^{(a)} = \lambda_i \upsilon_i^{(a)}$ ($i = 1,\ldots,n$). The (erroneous) argument, then, is as follows:

1. Measurement should establish a correlation between values of s of S and values of a of A, which with the above labeling implies that for each i the initial system state $\upsilon_i^{(s)}$ should push the pointer from some initial state $\psi_0^{(A)}$ into a final (post-measurement) state $\upsilon_i^{(a)}$. Hence the dynamics, described by some unitary operator $u \in B(H_S \otimes H_A)$, should be such that
$$u(\upsilon_i^{(s)} \otimes \psi_0) = \upsilon_i^{(s)} \otimes \upsilon_i^{(a)} \equiv \varphi_i. \tag{11.1}$$
2. If the initial system state is $\psi_0^{(S)} = \sum_i c_i \upsilon_i^{(s)}$ (with $\sum_i |c_i|^2 = 1$), then, by linearity of u, the final state is $\varphi = \sum_i c_i \varphi_i$. But if A is sufficiently macroscopic this conflicts with observation, which always shows one of the terms in the sum. In other words, in theory, a—more precisely, $1_{H_S} \otimes a$—has no value in this state, whereas in practice it does, since in the real world measurements do have outcomes.
3. Hence the final state should be the *mixed* density operator $\sum_i |c_i|^2 |\varphi_i\rangle\langle\varphi_i|$ (rather than the *pure* one $|\varphi\rangle\langle\varphi|$), whose ignorance interpretation (allegedly) yields one of the states φ_i with probability $|c_i|^2$. But it is impossible to transform the initial pure state $|\varphi_0\rangle\langle\varphi_0|$ into the above mixture by any unitary operator, let alone by the u defined by (11.1), which by construction yields
$$u|\psi_0^{(S)}\rangle\langle\psi_0^{(S)}|u^* = |\varphi\rangle\langle\varphi| \neq \sum_i |c_i|^2 |\varphi_i\rangle\langle\varphi_i|. \tag{11.2}$$

As we already discussed, for some authors the measurement problem is the clash between nos. 1 and 3 (this is the "small" problem), whereas for others it is the conflict between nos. 1 and 2 (i.e. the "big" one). Either way, the goal of insolubility theorems is to show that the problem is not a consequence of idealizations in primitive arguments like the one just given, but remains even under very general assumptions. In particular, both the purity of the initial system as well as apparatus states (and hence of their tensor product), and the exact system-apparatus correlation assumed (including the premise of point spectra and finite-dimensional Hilbert spaces), can be considerably relaxed. To illustrate the kind of discussion, we present one example of an insolubility proof along the former lines and one along the latter. These proofs even remain valid if the notion of an observable itself is relaxed, too, namely from a self-adjoint operator to a POVM (see (2.178)), but we will not discuss this utmost generality (if only because it would not circumvent our critique below). It should be noted that insolubility theorems tacitly assume that the mathematical objects in the quantum-mechanical formalism describe all there is physically.

In the first direction, we have Theorem 11.2 below, which we may summarize as the ***problem of statistics***: there is a contradiction between the following postulates:

1. *System and apparatus are both described quantum-mechanically.*
2. *The wave-function of the system is complete.*
3. *The wave-function always evolves linearly (e.g., by the Schrödinger equation).*
4. *Measurements with identical initial wave-functions may have different outcomes, and the probability of each possible outcome is given by the Born rule.*

Here the second and third postulates may be consequences of the first, but even so it is useful to list them separately, since denying or circumventing nos. 1, 2, and 3 is typically done in completely different ways (see the end of this section).

Formally, let $s = s^* \in B(H_S)$ be an arbitrary self-adjoint operator on an arbitrary (separable) Hilbert space H_S, with associated spectral projections $e^{(s)}_{\Delta} \in \mathscr{P}(H_S)$, $\Delta \subset \sigma(s)$, and likewise $a \in B(H_A)$. It is convenient (and entails no genuine loss of generality) to still assume that $\sigma(s) = \sigma(a)$. Recall that the Born measure $\mu^{(s)}_{\rho_S}$ on the spectrum $\sigma(s)$ induced by some density operator $\rho_S \in \mathscr{D}(H_S)$ is given by

$$\mu^{(s)}_{\rho_S}(\Delta) = \mathrm{Tr}\left(\rho_S e^{(s)}_{\Delta}\right) = \omega_S\left(e^{(s)}_{\Delta}\right) = \mu^{(s)}_{\omega_S}(\Delta), \tag{11.3}$$

cf. (4.9), where ω_S is the state associated to ρ_S by (2.33), and no notational confusion between $\mu^{(s)}_{\rho_S}$ and $\mu^{(s)}_{\omega_S}$ should arise (they are the same thing). Likewise for a.

Definition 11.1. *1. Let H be a Hilbert space and let $b \in B(H)_{\mathrm{sa}}$. Two (normal) states ω, ω' on $B(H)$ are called* **b-distinguishable** *if $\mu^{(b)}_{\omega} \neq \mu^{(b)}_{\omega'}$; in other words, there is some $\Delta \subset \sigma(b)$ such that $\mu^{(b)}_{\omega}(\Delta) \neq \mu^{(b)}_{\omega'}(\Delta)$. Similarly for $\rho, \rho' \in \mathscr{D}(H)$.*
2. In the situation described before (11.3)*, a pair (ρ_A, u), where ρ_A is a density operator on $B(H_A)$ and u is a unitary operator on $H_S \otimes H_A$, is a* **measurement scheme** *for s if s-distinguishability of two density operators ρ_S, ρ'_S on H_S implies $1_{H_S} \otimes a$ -distinguishability of the two states $u(\rho_S \otimes \rho_A)u^*$ and $u(\rho'_S \otimes \rho_A)u^*$.*

3. *A measurement scheme* (ρ_A, u) *for* s **preserves probabilities** *if for any density operator* $\rho_S \in \mathscr{D}(H_S)$ *the probability measure on* $\sigma(a) = \sigma(1_{H_S} \otimes a)$ *induced by* $u(\rho_S \otimes \rho_A)u^*$ *equals the Born measure* $\mu^{(s)}_{\rho_S}$ *on* $\sigma(s) = \sigma(a)$ *induced by* ρ_S*.*
4. *A density operator* $\rho \in \mathscr{D}(H_S \otimes H_A)$ **objectifies** *the pointer observable* a *relative to some countable partition* $\sigma(a) = \bigsqcup_i \Delta_i$ *of its spectrum if* $\rho = \sum_i p_i e_{\upsilon_i}$*, where each unit vector* $\upsilon_i \in H_S \otimes H_A$ *is an eigenvector of* $1_{H_S} \otimes e^{(a)}_{\Delta_i}$ *(*$p_i \geq 0$*,* $\sum_i p_i = 1$*).*

For example, in case of a discrete spectrumf or simplicity, if $\lambda_1 \neq \lambda_2$ in $\sigma(b)$, then any two unit eigenvectors $\upsilon_i^{(b)}$ $(i = 1,2)$ give rise to b-distinguishable vector states $\rho_i = |\upsilon_i^{(b)}\rangle\langle\upsilon_i^{(b)}|$. If $\psi = c_1\upsilon_1^{(b)} + c_2\upsilon_2^{(b)}$ with $|c_1|^2 + |c_2|^2 = 1$ and $c_1 \neq 0,1$, then also the trio (ρ_1, ρ_2, e_ψ) is pairwise b-distinguishable. If, the other hand, $\lambda \in \sigma(b)$ is degenerate, then e_ψ and $e_{\psi'}$ fail to b-distinguishable whenever $\psi, \psi' \in H_\lambda$.

Clause 2 of Definition 11.1—which incorporates a vast number of at least theoretical scenario's—is a considerable weakening of the scheme (11.1), while clause 3 sharpens the second, implying that measurement transfers all Born probabilities for the object to the apparatus, probabilistically making the latter a mirror image of the former. Clause 4 firstly takes care of continuous spectra; if $\sigma(a)$ is discrete, one may simply partition it by its points (a partition of $\sigma(a)$ is sometimes called a ***reading scale***). The "objectification" terminology is questionable (if not outright misleading), as it is motivated by the ignorance interpretation of mixtures (see below), but we follow the literature in using it. In what follows, we exclude the trivial cases where $\sigma(s)$ consist of a single point, and/or $\sigma(a)$ is partitioned by itself.

Theorem 11.2. *For any nontrivial object observable* s *and partitioning of* $\sigma(a)$*, there exists no measurement scheme* (ρ_A, u) *for* s *whose final state* $u(\rho_S \otimes \rho_A)u^*$ *objectifies* a *for any initial system state* ρ_S *(let alone one that preserves probabilities).*

Proof. Since we will not use this theorem (except for pointing out that it attacks a straw man), we just prove it in the special case where $\sigma(a)$ is discrete and partitioned by its points, and also the spectral decomposition $\rho_A = \sum_n p_n e_n$ of the initial apparatus state is unique, cf. (B.490). For any unit vector in $\upsilon^{(s)} \in H_S$ we then have

$$u(e_{\upsilon^{(s)}} \otimes \rho_A)u^* = \sum_n p_n u(e_{\upsilon^{(s)}} \otimes e_n)u^*. \tag{11.4}$$

Take $\lambda_1 \neq \lambda_2$ in $\sigma(s)$, with associated eigenvectors $\upsilon_1^{(s)}$ and $\upsilon_2^{(s)}$. If $e_n = |\alpha_n\rangle\langle\alpha_n|$, for unit vectors $\alpha_n \in H_A$, then objectification of a requires that each of the vectors

$$u(\upsilon_1^{(s)} \otimes \alpha_n), \;\; u(\upsilon_2^{(s)} \otimes \alpha_n), \;\; u((c_1\upsilon_1^{(s)} + c_2)\upsilon_2^{(s)} \otimes \alpha_n),$$

with $|c_1|^2 + |c_2|^2 = 1$ and $c_1 \neq 0,1$, must be an eigenvector of $1_{H_S} \otimes a$. This is only possible if the first two vectors (and hence the third) lie in the same eigenspace for $1_{H_S} \otimes a$, but in that case condition no. 2 in Definition 11.1 is violated, since the three given initial system states are pairwise s-distinguishable whereas the corresponding outcomes states just listed evidently fail to be $1_{H_S} \otimes a$-distinguishable. □

Insolubility theorems of the second kind describe the ***problem of outcomes***, according to which clauses 1., 2., and 3. of the problem of statistics also contradict:

4'. Measurements have determinate outcomes.

Technical statements to this effect are even more straightforward than those formalizing the problem of statistics. We keep H_S and $s \in B(H_S)$ as they were, but this time, H_A may refer to the rest of the Universe outside the quantum object described by H_S (which includes the pointer, of course). Here is the key assumption.

Definition 11.3. *Let $s \in B(H_S)_{\mathrm{sa}}$ be an object observable with partition $\sigma(s) = \bigsqcup_{i \in I} \Delta_i$ of its spectrum (if $\sigma(s) = \{\lambda_1, \ldots\}$ is discrete, one may take $\Delta_i = \{\lambda_i\}$), and let H_A be a second Hilbert space. A* **sound measurement scheme** *consists of:*

- *A collection $(S_i)_{i \in I}$ of* **outcome spaces***, i.e. subsets of the (normal) state space,*
$$S_i \subset S_n(H_S \otimes H_A) \cong \mathscr{D}(H_S \otimes H_A), \tag{11.5}$$
for which there is $0 \leq \eta < 1/2$ such that for $i \neq j$, one has
$$2\sqrt{1-\eta} \leq \|\omega_i - \omega_j\| \leq 2 \;\; (\omega_i \in S_i, \omega_j \in S_j). \tag{11.6}$$
- *A pair (ρ_A, u), where ρ_A is a density operator on $B(H_A)$ and u is a unitary on $H_S \otimes H_A$, such that for each $i \in I$ and each unit vector $\upsilon_i^{(s)} \in H_{\Delta_i}$ (i.e., $e_{\Delta_i} \upsilon_i^{(s)} = \upsilon_i^{(s)}$), the state $u(e_{\upsilon_i^{(s)}} \otimes \rho_A)u^*$ (i.e. the outcome of the measurement) lies in S_i.*

In (11.6) the first bound (which for small η is $\approx (2-\eta) \leq \cdots$) is the key one, as the last one ≤ 2 is always satisfied and has been included for clarity. In particular,
$$\|\omega_i - \omega_j\| > \sqrt{2}. \tag{11.7}$$

Note that (11.6) implies that the S_i must be disjoint, since assuming $\omega \in S_i$ gives $\|\omega - \omega_j\| \geq 2\sqrt{1-\eta}$ for all $\omega_j \in S_j$, whereas $\omega \in S_j$ allows one to take $\omega_j = \omega$ in this inequality, leading to the contradiction $0 \geq 2\sqrt{1-\eta}$. Note that in terms of density operators we have
$$\|\omega_i - \omega_j\| = \|\rho_i - \rho_j\|_1, \tag{11.8}$$
where $\omega_i(a) = \mathrm{Tr}(\rho_i a)$, cf. (B.481) and Theorem B.146. If ω_i and ω_j are pure, induced by unit vectors ψ_i and ψ_j in $H_S \otimes H_A$, then by (C.637), eq. (11.6) comes down to
$$0 \leq |\langle \psi_i, \psi_j \rangle|^2 \leq \eta. \tag{11.9}$$

For example, in the von Neumann measurement scheme (11.1), the subspace S_i just consist of the vector state defined by $\upsilon_i^{(s)} \otimes \upsilon_i^{(a)}$, hence (11.6) holds with $\eta = 0$.

Theorem 11.4. *For any nontrivial object observable s and partitioning of $\sigma(s)$, any sound measurement scheme $((S_i), \eta, \rho_A, u)$ admits initial states $\upsilon \in H_S$ such that $u(e_\upsilon \otimes \rho_A)u^*$ (i.e. the post-measurement state) does not lie in any outcome space S_i.*

Proof. Let $\upsilon = (\upsilon_i + \upsilon_j)/\sqrt{2}$, where $i \neq j$ and for the moment υ_i and υ_j are merely orthonormal vectors in H_S. For each $i = 1, 2$ we then compute:

$$\begin{aligned} \|u(e_\upsilon \otimes \rho_A)u^* - u(e_{\upsilon_i} \otimes \rho_A)u^*\|_1^{(H_S \otimes H_A)} &= \|e_\upsilon \otimes \rho_A - e_{\upsilon_i} \otimes \rho_A\|_1^{(H_S \otimes H_A)} \\ &= \|e_\upsilon - e_{\upsilon_i}\|_1^{(H_S)} \\ &= \|\omega_\upsilon - \omega_{\upsilon_i}\| \\ &= 2\sqrt{1 - |\langle \upsilon, \upsilon_i \rangle|^2} \\ &= \sqrt{2}, \end{aligned} \tag{11.10}$$

where $\|\cdot\|_1^{(H)}$ denotes the trace norm relative to H. Now take $\upsilon_i = \upsilon_i^{(s)}$ as in Definition 11.3. Since $\omega_i \equiv u(e_{\upsilon_i^{(s)}} \otimes \rho_A)u^* \in S_i$ by definition of a sound measurement, it follows from (11.7) and (11.10) that $\omega \equiv u(e_\upsilon \otimes \rho_A)u^*$ cannot lie in any subspace S_k, since that would require $\|\omega - \omega_l\| > \sqrt{2}$ for all $l \neq k$, whereas (11.10) shows that this inequality fails for at least two values of l, viz. $l = i$ and $l = j \neq i$. □

In order to circumvent Theorems 11.2 and 11.4, one should deny at least one of their explicit premises. Moreover, we note that postulate no. 3 (i.e. linearity of time-evolution) is always implicitly used in the form of the following counterfactual:

> *If* ψ_n *were* the initial state, then *for each n* it *would* evolve (linearly) according to the Schrödinger equation with *given* Hamiltonian h. *If* the initial state *were* $\sum_n c_n \psi_n$, also then it *would* evolve according to the *same* Hamiltonian h.

This counterfactual should be added as a tacit assumption to all insolubility proofs (and also to informal statements of the measurement problem). As such, it may reasonably be denied (see §11.4), and such a denial puts assumption no. 4 in the *problem of statistics* in perspective, namely by denying the possibility that identical initial states can always be prepared in such a way that they evolve through exactly the same Hamiltonian. This leaves room for the following denials of some premise:

¬ 1. The apparatus is not described quantum-mechanically;
¬ 2. The wave-function of the system is not complete;
¬ 3. The wave-function does not always evolve by the Schrödinger equation;
¬ 4. Identical initial wave-functions always yield identical outcomes;
¬ 4'. Measurements do not have determinate outcomes.

Current programs for solving the measurement problem neatly fall into this scheme:

¬ 1. Copenhagen Interpretation and Swiss Approach;
¬ 2. Hidden-variable theories, most prominently Bohmian mechanics;
¬ 3. Dynamical collapse theories (such as GRW);
¬ 4. Instability approaches, e.g., the Flea on Schrödinger's Cat (which keeps 3);
¬ 4'. Many-Worlds Interpretation, i.e., Everettian quantum mechanics.

Leaving most of these to the literature, we now turn to the instability approach (¬4).

11.4 The Flea on Schrödinger's Cat

The conclusion of this lengthy historical and technical introduction is that there are (at least) two different formulations of the measurement problem, whose insolubility is expressed by Theorems 11.2 and 11.4, respectively (leaving apart lavish opportunities for disagreement about the precise formulation of the underlying assumptions, and not even speaking about the outright dismissal of the whole issue as a *Scheinproblem*). Thus the problem in question is evidently of a different kind from say the famous open conjectures in mathematics (like the Riemann hypothesis), where it is clear what the theorem is that needs to be proved. Nonetheless, despite its undeniable philosophical aspects, we see the measurement problem as a genuine physics problem concerned with the discrepancy between (quantum) theory and experiment, to be addressed by mathematical, physical, and philosophical analysis.

Well aware that different people typically draw different lessons from history, we will now, in the interest of motivating our approach to follow, draw our own (necessarily subjective) conclusions from the history of the measurement problem.

1. Though grounded in genius and tradition (Heisenberg, von Neumann, Wigner), the two-step way of looking at the measurement process (i.e. in terms of firstly a reduction of the wave-function by some non-unitary "process 1" and secondly a registration of a single outcome), with ensuing separation of the measurement problem into a "small" and a "big" problem, is fruitless and should be abandoned. It has no basis whatsoever in experimental physics (where the alleged mixed post-measurement states are conspicuously absent), it reflects obsolete ensemble thinking, and it is unsound also theoretically, as shown both by the first kind of insolubility results (à la von Neumann and Theorem 11.2), as well as by the failure of programs addressing just the "small" problem (like the Swiss approach and Decoherence). These approaches are unable to deal with the "big" problem (except perhaps through desperate remedies like Many Worlds) and hence, even if they work, they deliver Pyrrhic victories at best. The problem of obtaining single outcomes should be solved directly, before it is too late. Since such a solution would leave nothing to interfere, the "small" problem automatically disappears. This does not mean that it is sufficient to obtain definite outcomes alone; among all remaining challenges, deriving the Born rule stands out in particular.
2. Too much formal analysis has been done on the measurement problem (including the insolubility theorems just reviewed) without taking the special nature of measurement devices into account; alas, this negligence has its roots in the work of von Neumann. These devices are typically treated as ordinary quantum systems, as a consequence of which the notion of an "outcome" has to be defined within quantum mechanics and hence has to be identified e.g. with an eigenstate of some operator describing the apparatus (as in Theorem 11.2) or with some subspace of the quantum-mechanical state space (as in Theorem 11.4). Such identifications are purely formal and have little basis in experimental physics: as long as one defines outcomes of measurements within quantum mechanics, there is no measurement problem (but at worst some unease concerning value indefiniteness)!

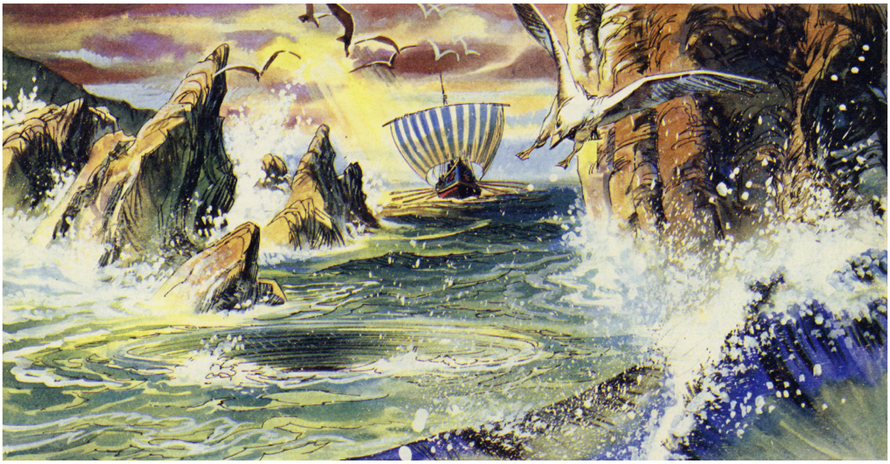

Fig. 11.1 *The waves crashed between the towering cliff of Scylla and the jagged rocks of Charybdis*. Colour litograph by Gino D'Antonio. Reprinted with permission from Look and Learn Ltd.

On the other hand, both the Copenhagen Interpretation and the Swiss approach seem to have gone too far in the opposite direction: the former because it simply assumed (without providing any justification) that measurements have outcomes as soon as the apparatus is described classically, the latter in treating apparatuses as strictly infinite, and hence falling victim to Earman's Principle. The right approach, then, must be to define measurement as in the Copenhagen Interpretation, i.e. using a classical description of the apparatus whilst realizing it is ontologically a quantum system, and thusly navigate between Scylla (who treats measurement devices as arbitrary *quantum* systems) and Charybdis (who is too enthusiastic in taking infinite limits and hence in using a *classical* description).

3. Some kind of reality has to be attributed to the state of the system (though this reality cannot be "absolute", as in classical physics). In the algebraic approach to quantum theory adopted throughout the present book, the starting point is provided by the observables, relative to which states are defined. Since the doctrine of classical concepts drives us to switch between quantum-mechanical and classical descriptions, the reality of the quantum state is therefore *perspectival*. However, their perspectival nature does not make states less real; they say everything there is to say (at least by quantum theory) about some given level of description (which may be said to be chosen by the observer, and hence is intersubjective).

Thus the measurement problem arises in the way Schrödinger (rather than von Neumann) described it, although a precise framework has to be added to his poetry.

A framework that is precise both conceptually and mathematically is offered by ***asymptotic emergence***, which we already encountered in our discussion of SSB in the previous chapter (see especially its preamble). To repeat the main points, we speak of asymptotic emergence if the following three conditions are all satisfied:

1. A “higher-level theory” H (which in the context of the measurement problem is either classical mechanics or classical thermodynamics, depending on the measurement setup) is a limiting case of some “lower-level theory” L (viz. quantum mechanics, including quantum statistical mechanics of a finite system).
2. Theory H is well defined and understood by itself (typically predating L).
3. Theory H has “emergent” features that cannot be explained by L, e.g. because L does not have any property inducing those feature(s) in the limit pertinent to H.

The root of the measurement problem (and hence the relevance of asymptotic emergence), then, lies in Bohr’s requirement that the outcomes of measurements on systems defined within L be recorded in (at the least the language of) H, so that, crucially, *measurement according to* L *is a notion external to* L (if only partly), in particular involving the relationship between L and H. None of the insolubility proofs of the measurement problem take this into account (although due to Butterfield’s Principle these proofs remain relevant in a secondary way). The typical feature of H that would be emergent in the above sense if the measurement problem were unresolved is that every physical system subject to the theory H is ontologically in a pure state; in Schrödinger’s words quoted in §11.1: in H, sharply focused photographs of states are always possible (and hence any uncertainty or chance is due to ignorance, as in classical physics). Now, whatever the ontological nature of states in L, the states they induce in H should be real in the above sense, i.e., pure. But this is precisely what does *not* seem to be the case in typical measurement situations (e.g., Schrödinger’s Cat), where the post-measurement state on L induces a *mixed* state on H. Just as in the case of SSB, this violates Butterfield’s Principle, which in the case at hand states that since H is an idealization of L, any physical effect in H must be foreshadowed in L: *as* L *approaches* H, sharp measurement outcomes (defined as pure states in H) must arise from at least approximate single measurement outcomes (i.e. “singly-peaked wave-functions”) in *the relevant asymptotic regime of* L (since only these wave-functions gives rise to pure classical states on H).

As noted before in the setting of SSB: violating Butterfield’s Principle means violating Earman’s Principle, which in turn leads to a violation of the link between theory and reality. It is worth spelling this out for the measurement problem:

- Reality is described by quantum mechanics (even in the Copenhagen Interpretation, classical mechanics is an idealization of quantum mechanics);
- Real phenomena—in this case, sharp measurement outcomes— are correctly described by classical mechanics *although this is an idealization*;
- Quantum mechanics (allegedly) cannot possibly induce these phenomena in its limit towards classical mechanics *although it is the theory that should apply*;
- Hence quantum mechanics contradicts reality. Classical mechanics does not contradict the reality of sharp measurement outcomes, but it is not the appropriate theory to explain them; this explanation should come from quantum mechanics.

It may now seem that invoking Butterfield’s Principle has reduced the measurement problem to the usual one(s) described in the preceding sections. But look at the small print: in the Copenhagen Interpretation, single measurement outcomes only appear in some limiting “classical” regime of quantum mechanics.

"Deep inside" quantum mechanics, there is no need at all for the typical superposition $\sum_n c_n \psi_n$ to collapse into one of the states ψ_n (unless one conflates the physical measurement problem with the philosophical problem of value indefiniteness). The external and asymptotic nature of measurement outcomes *causes* the measurement problem, but, as we shall see, at the same time it provides the key for its *solution*, since the collapse mechanism we propose is only effective asymptotically (so that it operates where it should and does not act where it should not). More precisely, by taking into account perturbations of the Hamiltonian that are tiny and ineffective in the quantum regime, but become hugely destabilizing in the classical regime (even before the actual limit), the wave-function of the apparatus will collapse.

Summarizing the preceding discussion, "our" measurement problem states that:

- *Certain* pure *post-measurement states of an (ontologically quantum-mechanical!) apparatus coupled to a microscopic quantum object induce* mixed *states on the apparatus (and on the composite)* **once the apparatus is described classically**.

This is a precise version of Schrödinger's Cat problem (rather than von Neumann's purely quantum-mechanical measurement problem), making it clear that at heart the problem does not lie with the (dis)appearance of interference terms (which is a red herring) but with the inability of quantum mechanics to predict single outcomes.

We now show by means of a simple example what it means to describe an ontologically quantum-mechanical apparatus classically, and outline the scenario we envisage for the solution of the measurement problem on the basis of this example. The ***Spehner–Haake model*** of the apparatus described below is too simple to be realistic, but nonetheless it may serve its purpose (as Bohr would say). The model involves a double-well potential like (10.11), modified however by a little basin in the middle, as shown below (including ground states for one large and one small value of $\hbar$). Also here, SSB will play a crucial role, so please recall §10.1.

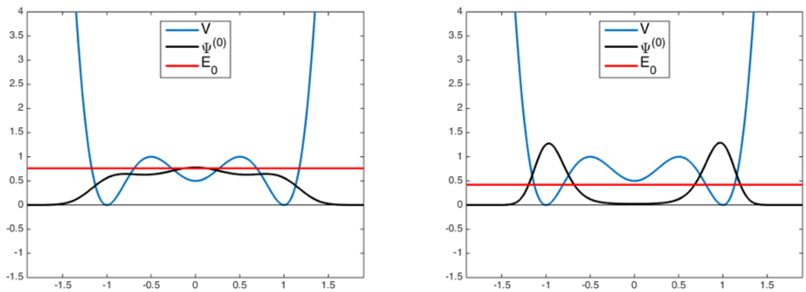

Fig. 11.2 Double-well potential with basin; ground state $\psi^{(0)}_{\hbar=0.5}$ and $\psi^{(0)}_{\hbar=0.01}$.

Consider $N' \equiv N+1$ non-interacting particles, each with mass m, moving on the real line under the influence of a one-particle potential V (note that although the zero'th particle with be handled lightly differently from the others, it is not the pointer!). In terms of the canonical coordinates $(\mathbf{p}', \mathbf{q}') = (p_0, \ldots, p_N, q_0, \ldots, q_N) \in \mathbb{R}^{2N'}$ on the phase space $X = T^*\mathbb{R}^{N'}$ the classical Hamiltonian is

$$h(\mathbf{p}', \mathbf{q}') = \sum_{n'=0}^{N} \left(\frac{p_{n'}^2}{2m} + V(q_{n'}) \right). \tag{11.11}$$

Now perform a canonical transformation to center of mass and relative coordinates

$$P = \sum_{n'=0}^{N} p_{n'} \qquad\qquad Q = \frac{1}{N'} \sum_{n'=0}^{N} q_{n'}; \tag{11.12}$$

$$\pi_n = \sqrt{N'} p_n - \frac{1}{\sqrt{N'}} \sum_{n'=0}^{N} p_{n'} \qquad \rho_n = \frac{1}{\sqrt{N'}} (q_n - q_0) \;\; (n = 1, \ldots, N); \tag{11.13}$$

the center of mass (P, Q) will be the pointer. The inverse transformation is given by

$$p_0 = \frac{P}{N'} - \frac{1}{\sqrt{N'}} \sum_{n=1}^{N} \pi_n; \tag{11.14}$$

$$p_n = \frac{P}{N'} + \frac{1}{\sqrt{N'}} \pi_n; \tag{11.15}$$

$$q_0 = Q - \frac{1}{\sqrt{N'}} \sum_{n=1}^{N} \rho_n; \tag{11.16}$$

$$q_n = Q + \sqrt{N'} \rho_n - \frac{1}{\sqrt{N'}} \sum_{k=1}^{N} \rho_k. \tag{11.17}$$

Granted that $\{p_{n'}, q_{k'}\} = \delta_{n'k'}$, $\{p_{n'}, p_{k'}\} = 0$, and $\{q_{n'}, q_{k'}\} = 0$, we then duly have $\{P, Q\} = 1$ and $\{\pi_n, \rho_k\} = \delta_{nk}$, with all other elementary Poisson brackets vanishing.

In terms of the new coordinates, the classical Hamiltonian (11.11) reads

$$h(P, Q, \pi, \rho) = h_A(P, Q) + h_{AE}(Q, \rho) + h_E(\pi), \tag{11.18}$$

where $\pi = (\pi_1, \ldots, \pi_N)$, $\rho = (\rho_1, \ldots, \rho_N)$, and the three partial Hamiltonians are

$$h_A(P, Q) = \frac{P^2}{2M} + N' V(Q); \tag{11.19}$$

$$h_E(\pi) = \frac{1}{2M} \left(\sum_{n=1}^{N} \pi_n^2 + \left(\sum_{n=1}^{N} \pi_n \right)^2 \right); \tag{11.20}$$

$$h_{AE}(Q, \rho) = \sum_{k=1}^{\infty} \frac{1}{k!} f_k(\rho) V^{(k)}(Q), \tag{11.21}$$

where $M = Nm$ is the total mass of the system, for simplicity we assumed V to be analytic (it will even be taken to be polynomial), and we abbreviated

$$f_k(\rho) = \left(-\frac{1}{\sqrt{N'}}\sum_{l=1}^{N}\rho_l\right)^k + \sum_{n=1}^{N}\left(\sqrt{N'}\rho_n - \frac{1}{\sqrt{N'}}\sum_{l=1}^{N}\rho_l\right)^k. \tag{11.22}$$

Note that $f_1(\rho) = 0$, so that to lowest order (i.e. $k = 2$) we have

$$h_{AE}(Q,\rho) = \left(\tfrac{1}{2}N\sum_{n=1}^{N}\rho_n^2 - \sum_{k\neq l}^{N}\rho_k\rho_l\right)V''(Q) + \cdots \tag{11.23}$$

We pass to the corresponding quantum-mechanical Hamiltonians in the usual way, and couple a two-level quantum system to the apparatus through the Hamiltonian

$$h_{SA} = \mu\cdot\sigma_3 \otimes P, \tag{11.24}$$

where the object observable $s = \sigma_3$, acting on $H_S = \mathbb{C}^2$, is to be measured. The idea is that h_A is the Hamiltonian of a pointer that registers outcomes by localization on the real line, h_E is the (free) Hamiltonian of the "environment", realized as the internal degrees of the freedom of the total apparatus that are not used in recording the outcome of the measurement, and h_{AE} describes the pointer-environment interaction. The classical description of the apparatus then involves two approximations:

- Ignoring all degrees of freedom except those of A, which classically are (P,Q);
- Taking the classical limit of h_A, here realized as $N \to \infty$ (*in lieu of* $\hbar \to 0$).

The measurement of s is now expected to unfold according to the following scenario:

1. The apparatus is initially in a metastable state (this is a very common assumption), whose wave-function is e.g. a Gaussian centered at the origin.
2. If the object state is "spin up", i.e., $\psi_S = (1,0)$, then it kicks the pointer to the right, where it comes to a standstill at the bottom of the double well. If spin is down, likewise to the left. If $\psi_S = (1,1)/\sqrt{2}$, the pointer moves to a superposition of these, which is close to the ground state of V displayed in Figure 11.2.
3. In the last case, the Flea mechanism of §10.2 comes into play: tiny asymmetric perturbations irrelevant for small N localize the ground state as $N \to \infty$.
4. Mere localization of the ground state of the perturbed (apparatus) Hamiltonian in the classical regime is not enough: there should be a *dynamical* transition from the ground state of the original (unperturbed) Hamiltonian (which has become metastable upon perturbation) to the ground state of the perturbed one. This dynamical mechanism in question should also recover the Born rule.

Thus the classical description of the apparatus is at the same time the root of the measurement problem and the key to its solution: it creates the problem because at first sight a Schrödinger Cat state has the wrong classical limit (namely a mixture), but it also solves it, because precisely in the classical limit Cat states are destabilized even by the tiniest (asymmetric) perturbations and collapse to the "right" states.

The "flea" perturbation might itself be a genuine random process, perhaps ultimately of quantum origin. In that case, the measurement merely amplifies the randomness that was already inherent in the flea by transferring it to the apparatus.

Alternatively, the flea might be fundamentally deterministic (though it may nonetheless be modeled stochastically for pragmatic reasons). In principle, this would open the door to a restoration of determinism: for the flea now transfers its *determinism* (rather than its *randomness*) to the apparatus. The mistaken impression that quantum theory implies the irreducible randomness of nature then arises because although measurement outcomes are determined, they are unpredictable "for all practical purposes", even in a way that (because of the exponential sensitivity to the flea in $1/\hbar$ or N) dwarfs the unpredictability of classical chaotic systems.

Either way, the flea perturbation would naturally be different at each different run of an experiment under otherwise identical initial conditions, which motivates our critique of the counterfactual discussed after the proof of Theorem 11.4.

The location of the flea plays a similar role to the position variable in Bohmian mechanics, i.e., it is essentially a hidden variable. Recall the notions of *Outcome Independence* (OI) and *Parameter Independence* (PI), reviewed in §6.5. Briefly, the conjunction of OI and PI is equivalent to Bell's locality condition, and if the latter is satisfied, then the Bell inequalities hold. Since these are violated by quantum mechanics, any hidden variable theory compatible with quantum mechanics must violate OI or PI. Deterministic hidden variable theories necessarily satisfy OI, in which case Bell's Theorem or the Free Will Theorem shows that they must violate PI in order to be compatible with quantum mechanics. A violation of PI leads to possible superluminal signaling only if the hidden variable z can be controlled. If the wave-function ψ is regarded as the hidden variable, then quantum theory itself satisfies PI but violates OI (since ψ *can* be prepared, the other way round would be disastrous). *Qua* deterministic hidden variable theory, Bohmian mechanics satisfies OI, and hence it violates PI; for the GRW collpase theory it is the other way round.

The fate of the flea therefore depends on the nature of the perturbation: if it is deterministic, the theory behaves like Bohmian mechanics in this respect and hence violates PI, whereas stochastic perturbations typically violate OI (and possibly also PI). Either way, no conflict with the said theorems arises. Moreover, in the Colbeck–Renner Theorem, assumption **CP** fails for the flea scenario—assuming, in view of its limitation to finite-dimensional Hilbert spaces, the theorem is applicable at all!

Besides such issues, others remain to be resolved, of which we just mention two:

1. Collapse of the wave-function has become a tunneling process, whose static effects are exponentially enhanced as $N \to \infty$ (or $\hbar \to 0$, as in §10.2). However, tunneling times *increase* in the same way, so that the environment is needed not only to provide the perturbation, but also to speed up the dynamics of collapse.
2. The flea not only destabilizes the Schrödinger Cat state (as desired), but also destabilizes the intended outcome states (like those in S_i, cf. Theorem 11.4). Also here the environment should play a decisive role in (re)stabilizing the latter but not the former, possibly through the mechanism of *einselection*, cf. §11.2.

Notes

§11.1. **The rise of orthodoxy**

The literature on the measurement problem is vast. Apart from the annotated reprint volume Wheeler & Zurek (1983), relatively recent surveys of and books include Bell (1990b), Maudlin (1995), Busch, Lahti, & Mittelstaedt (1996), Bassi & Ghirardi (2003), Mittelstaedt (2004), Wallace (2012), Allahverdyan, Balian, & Nieuwenhuizen (2013), and Busch, Lahti, Pellonpää, & Ylinen, (2016). In modal interpretations of quantum mechanics, the measurement problem is (dubiously) conflated with the far milder problem of value indefiniteness, see e.g. Bub (1997).

§11.2. **The rise of modernity: Swiss approach and Decoherence**

The Swiss approach to the measurement problem was initiated by Jauch (1964), to be continued by e.g. Hepp (1972), Emch & Whitten-Wolfe (1976), and recently also by Hepp's former student Fröhlich; see e.g. Fröhlich & Schubnel (2013) and Blanchard, Fröhlich & Schubnel (2016). In addition, see Landsman (1991, 1995)—now seen as naive—, Breuer, Amann & Landsman (1993), and Sewell (2005).

Key early papers on decoherence were Zeh (1970), Zurek (1981), and Joos & Zeh (1985), and standard reviews are Zurek (2003), Joos et al (2003), and Schlosshauer (2007). Penetrating critiques include Janssen (2008) and Tanona (2013). See also Camilleri (2009a) and Freire (2009) for some history.

A defence of QBism may be found in Caves, Fuchs, & Schack (2002b).

§11.3. **Insolubility theorems**

Insolubility theorems of the first kind kind go back to von Neumann (1932) and, in his wake, Wigner (1963) and Fine (1970). Theorem 11.2 is (in even more general form) due to Busch & Shimony (1996); with slightly different assumptions, the special case proved in the main text is due to Brown (1986). The monographs by Busch, Lahti, & Mittelstaedt (1996) and Mittelstaedt (2004) contain detailed discussions of theorems of this kind. See also Bacciagaluppi (2014).

The formulation of the problem of statistics and the problem of outcomes is taken from Maudlin (1995). Theorem 11.4 is due to Bassi & Ghirardi (2003), although here it is presented in a form inspired by Grübl (2003).

For Bohmian mechanics see e.g. Goldstein (2013) and Bricmont (2016). A recent review of the GRW program and related dynamical collapse theories is Bassi et al (2013). Nowadays, the *locus classicus* for Many Worlds is Wallace (2012).

The time-evolution counterfactual discussed in the main text was inspired by the problem of free will, see the quotation of Dennett at the beginning of §6.3.

S11.4. **The Flea on Schrödinger's Cat**

The approach to the measurement problem discussed here has its roots in Landsman & Reuvers (2013) and Landsman (2013), whose model at the time only involved the apparatus. This was criticized in van Heugten & Wolters (2016), many of whose points may be addressed by turning to the Spehner–Haake model, introduced by Spehner & Haake (2008). The ABN-model of Allahverdyan, Balian, & Nieuwenhuizen (2013) gives a similar picture; for a comparison see Spehner (2009).