The Invalid Inference of Universality in Quantum Mechanics

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Abstract

The universality assumption ("U") that quantum wave states only evolve by linear or unitary dynamics has led to a variety of paradoxes in the foundations of physics. U is not directly supported by empirical evidence but is rather an inference from data obtained from microscopic systems. The inference of U conflicts with empirical observations of macroscopic systems, giving rise to the century-old measurement problem and subjecting the inference of U to a higher standard of proof, the burden of which lies with its proponents. This burden remains unmet because the intentional choice by scientists to perform interference experiments that only probe the microscopic realm disqualifies the resulting data from supporting an inference that wave states always evolve linearly in the macroscopic realm. Further, the nature of the physical world creates an asymptotic size limit above which interference experiments, and verification of U in the realm in which it causes the measurement problem, seem impossible for all practical purposes if nevertheless possible in principle. This apparent natural limit serves as evidence against an inference of U, providing a further hurdle to the proponent's currently unmet burden of proof. The measurement problem should never have arisen because the inference of U is entirely unfounded, logically and empirically.

Keywords: measurement problem; universality of quantum mechanics; unitary-only dynamics; Schrödinger's Cat; Wigner's Friend

I. INTRODUCTION

Just as logicians are bound by the laws of physics, physicists are bound by the rules of logic. Some of the most persistent problems and paradoxes in the foundations of physics, many of which have been unsuccessfully tackled by physicists and mathematicians for nearly a century, persist exactly *because* they aren't really physics or mathematics problems at all but rather problems of logic.

Consider the well-known problems of Schrödinger's Cat ("SC") (Schrödinger (1935)) and its conscious cousin, Wigner's Friend ("WF") (Wigner (1961)). If SC is characterized as, "A cat in state $|alive\rangle + |dead\rangle$[1] is simultaneously dead and alive" (e.g., Wang *et al.* (2016)), then SC is inherently impossible because *dead* and *alive* are mutually exclusive. Therefore, if SC is physically possible, at least in principle, then it is patently false that a cat in state $|alive\rangle + |dead\rangle$ is simultaneously dead and alive. Indeed, the characterization of any object in a quantum superposition of eigenstates of some observable as being "simultaneously in those eigenstates" is equally problematic.

While SC is not inherently paradoxical, it is more than a little odd. The generally accepted conclusion that a SC state is possible in principle[2] follows directly from the assumption that the mathematics of quantum mechanics (i.e., the linear or unitary evolution of a wave state) applies universally at all scales, including both electron and cat. This assumption leads to a variety of foundational issues in physics, perhaps most notably the century-old measurement problem ("MP") of quantum mechanics ("QM"). MP is actually a paradox, meaning that it comprises logically incompatible assumptions. Contradictions are necessarily false and cannot exist in nature. If the conjunction of statements A and B, for example, leads to a contradiction, then at least one of statements A and B is false. Often, MP has been characterized as the conjunction of three or more assumptions (e.g., Maudlin (1995) and Brukner (2017)); the logical incompatibility of these assumptions has been shown many times (e.g., Frauchiger and Renner (2018), Brukner (2018), and Bong *et al.* (2020)). A simpler characterization of MP, reducing it to two assumptions, has been provided by Gao (2019):

P1) The mental state of an observer supervenes on her wave function.
P2) The wave function always evolves in accord with a linear dynamical equation (such as the Schrödinger equation).

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[1] To simplify arguments, normalization constants in quantum superpositions will be neglected throughout this paper.

[2] No one argues that producing or measuring SC would be easy *in practice*, and several references (e.g., Mari *et al.* (2016), Skotiniotis *et al.* (2017), Fröwis *et al.* (2018), and Aaronson *et al.* (2020 Preprint)) offer explanations why interesting macroscopic quantum superpositions, like SC, would be particularly difficult to produce or measure. Having said that, not all commentators are forthright about this. Deutsch (1985), for example, in describing a modified WF experiment which, he claims, would distinguish a collapse theory of QM from the many worlds interpretation, hides the complexity and difficulty of such an experiment by representing them mathematically. He provides no details on how such an actual experiment might be performed and instead nonchalantly characterizes it as one "involving... fairly detailed adjustments inside an observer's brain and sense organs."

Assumption P2 is often characterized as the "universality" of QM, in that the mathematical rules of QM apply universally at all scales. Kastner (2020) correctly notes that "unitary-only evolution" is a better description than "universality" because, she argues, a complete theory of QM (if there is one) will necessarily apply universally but its wave functions may not evolve in a purely unitary, linear, or time-reversible fashion.[3] Whether meaning "universal" or "unitary-only," in this paper assumption P2 will generally be referred to as "U."

Because at least one of P1 and U is indeed false, a simultaneous belief in both is due to faulty reasoning. The measurement problem is not a problem with nature; it is a problem with us. It is either the case that P1 is the result of improper assumptions and/or reasoning, that U is the result of improper assumptions and/or reasoning, or both. Conventional wisdom seems to demand that anyone who attacks U is obligated to provide the supplemental explanation and mathematical modification necessary to account for nonlinearity – i.e., U is presumed true until proven false.[4] Several researchers have admirably attempted to assume that burden, most notably the "GRW" spontaneous collapse of Ghirardi, Rimini, and Weber (1986) and gravitational collapse of Penrose (1996).

My goal in this paper is to attack U on a variety of grounds without assuming any obligation to account for or explain nonlinearity. A fascinating example of this atypical approach is that of D'Ariano (2020), who argues that "unitarity of the realisation of quantum transformations is a spurious postulate [that is] inessential [and] not falsifiable." The author further concludes that the black hole information paradox and Schrödinger's Cat conundrum, among others, disappear as logical paradoxes and that interpretations of QM that assume U (including many worlds, relational, and transactional interpretations) cannot be falsified and "should not be taken too seriously."

Specifically, I will argue in this paper that U is actually an invalid inference that is not supported by the existing evidence and should never have been accepted. The high and currently unmet burden of proving the validity of the inference of U remains with its proponents. Consequently, the measurement problem should never have arisen because the inference of U is scientifically unfounded.

II. THE INVALID INFERENCE OF U

A. What's the Problem?

The measurement problem is inextricably related to SC and WF and might be colloquially phrased as, "If a cat can exist as a superposition over macroscopically distinct eigenstates $|dead\rangle$ and $|alive\rangle$, then why do we always see either a dead or a live cat?" Or: "If quantum mechanics

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[3] She argues that because U is "not an established fact" and "provides no way to get single outcomes out of any measurement," we should take seriously the possibility of non-unitary collapse.

[4] I cannot find in the academic literature a justification for this belief despite its apparent pervasiveness among physicists. The typical reasoning, which implicitly places the burden of proof on the skeptics of U, is given by Brukner (2018): "There is nothing in quantum theory making it applicable to three atoms and inapplicable to 10^{23}."

applies universally at all scales (or wave functions always evolve linearly, or all physical interactions are reversible), then why do we never observe quantum superpositions?" Or even better: "Why don't we see quantum superpositions?"

MP is actually a paradox and any solution to it requires showing that at least one of its assumptions is incorrect. All formulations of the measurement problem depend on the assumption of U; MP would be solved if it were shown, for example, that QM wave states did not evolve linearly in systems large enough to effect measurement. After 100 years, why has this not yet been shown? Perhaps this is the wrong question. The first questions we might ask are: were we justified in assuming U in the first place? Has it been empirically demonstrated?

U is itself an *inference* because a conclusion that a wave state always evolves linearly, based on a limited number of data points, requires inductive reasoning. The question is not whether logical inferences based on inductive reasoning can be made in science – despite philosophical arguments to the contrary (e.g., Popper and Miller (1983)), I, and certainly those who endorse U, believe that valid scientific inferences can be made. One can never prove that a scientific hypothesis or law always holds; rather, it can only be adequately verified to permit such an inference, subject always to the possibility of experimental falsification.

What constitutes "adequate verification" in science is akin to the legal issue, in court cases, of evidentiary standard or burden of proof. For example, legal cases in the U.S. may be subject to proof under various standards, such as "preponderance of evidence," "clear and convincing evidence," or "beyond a reasonable doubt," depending in part on whether the case is civil or criminal. Just as a plaintiff assumes the burden of proving her claim to the required standard, the scientist who asserts the validity of an inference assumes the relevant burden of proof; those who assert the inference of U therefore assume the burden of showing its validity. In the following analysis, I aim to address the extent to which available evidence meets, or actually increases, that burden.

U is a very special kind of inference, one that I will argue in the following sections is invalid. First, U has been tested and verified only in microscopic regimes. No experiment has shown it to apply to macroscopic regimes and there is no direct experimental evidence for the applicability of linear QM dynamics to macroscopic systems. Second, the lack of such evidence is not for lack of trying. Rather, there seems to be a kind of natural asymptotic limit to the size of a system to which we are able to gather such evidence. Third, U gives rise to the measurement problem – that is, it conflicts with what seems to be good empirical evidence that linear and time-reversible QM dynamics do *not* apply in most[5] cases. These together, as I will argue, render U an invalid inference. Further, even if one disagrees with this argument, the burden of proof remains, as it always has, with the proponents of U, not the skeptics.

Regarding the first point – that U has been verified only in the microscopic realm – there are certainly those who choose to tamper with the colloquial meanings of the words "microscopic" and "macroscopic," or attempt to redefine "mesoscopic" as "nearly macroscopic," to bolster their

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[5] It is important to note how strong a claim U is: even a single counterexample is adequate to produce the apparent contradiction of MP and call U into question.

case.[6] Some may include as "macroscopic quantum superpositions" atoms in superposition over position eigenstates separated by a meter, or "macroscopic" (though barely visible) objects in superposition over microscopically-separated position eigenstates (e.g., O'Connell *et al.* (2010)). I regard these characterizations as sophistry, particularly when such examples are used as empirical evidence that the creation and measurement of interesting macroscopic superpositions, such as SC and WF, are mere technological hurdles. Nevertheless, I will assume that any physicist acting in good faith will readily admit that there is currently no direct empirical evidence that linear wave state evolution applies to a cat, a human, or even a virus. That lack of experimental evidence need not interfere with making a valid inference – which by its nature is an assertion that is not directly supported by evidence – unless that inference conflicts with other observations. Consider these statements:

A1) Newton's law of gravity applies universally.
A2) The observed perihelion of Mercury is in conflict with Newton's law of gravity.

Statement A1 had been accepted as a valid inference for a very long time. The conjunction of these two statements, however, is a contradiction, implying that at least one of them is false. A contradiction sheds new doubt on each statement and increases the evidence necessary to verify each unless and until one of the assumptions is shown false. Despite enormous quantities of data supporting an inference of A1, conflicting evidence ultimately led Einstein to reject A1 and formulate General Relativity.

The measurement problem is such a paradox; it is the conjunction of two or more statements that leads to a contradiction. If there were no paradox, one might have inferred U based only on the limited experimental data showing interference effects from electrons, molecules, etc. However, U is in direct logical conflict with other statements the veracity of which we seem to have a great deal of evidence. Therefore, to justify the inference of U, we need more than a reason: we need a *good* reason. However, given that the paradox arose essentially simultaneously with quantum theory, leading Erwin Schrödinger (1935) to propose his hypothetical cat as an intuitive argument against U, the burden of proof has always lain with those who assert U. Have they met their burden? Do we have good evidence to support the inference of U?

B. Fighting Fire with Fire

Many (perhaps most) physicists have never questioned the assumption of U, and once asked whether we have good evidence to support the inference of U may regard the question itself as nonsense. "Of course the wave function always evolves linearly – just look at the equations!" Indeed, standard QM provides no mathematical formalism to explain, predict, or account for breaks in linearity. Some collapse theories do posit such breaches, but no experiment has yet

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[6] Fröwis *et al.* (2018) thoroughly discuss the meaning of "macroscopic" but generalize that "*macroscopic* is a synonym for large" and "macroscopic quantumness means quantum coherence between *macroscopically distinct* states."

confirmed any of them or distinguished them from other interpretations of QM. It is thus tempting, when evaluating U, to glance at the equations of QM and note that they do, indeed, evolve linearly. But the question isn't whether the equations evolve linearly, but whether the physical world *always* obeys those equations, and the answer to that question does not appear within the QM formalism itself.[7]

Modern physicists, who rely heavily on mathematics to proceed, typically demand rigorous mathematical treatment in addressing and solving physics problems. Ordinarily, such demands are appropriate. However, MP arises directly as a *result* of the mathematics of QM, in which the Schrödinger equation evolves in a unitary and reversible fashion. Because MP is itself a product of the mathematics of QM, its solution is inherently inaccessible via the symbolic representations and manipulations that produced it. If the math itself is internally consistent – and I have no reason to believe otherwise – then you cannot use the math of QM as evidence that the math of QM is always correct.[8] You can, however, use empirical evidence to support such an inference. Do we have such evidence?

C. Empirical Data Do Not Support an Inference of U

Whether empirical data are sufficient to validate a particular scientific inference is a question about which reasonable scientists may differ. However, we can all agree that a *single* datum in the abstract is inadequate to infer that a particular hypothesis *always* holds. I will argue in this section that whether additional data provide further evidence to validate that inference depends on the source of those data and the choice of experiments that produced them.

Consider, for example, a scientist at home who uses a barometer to measure the atmospheric pressure in his kitchen. From this single datum – say, 1 atm – he infers that the pressure is 1 atm not just in his home, but everywhere in the universe. Obviously, no reasonable scientist would agree that this datum is adequate to infer such a universal statement. In response to this objection, the scientist then takes another measurement, this time in his dining room, and offers this second datum as further evidence to support his inference. However, this additional datum, resulting from an experiment inside his home, provides no further evidence beyond the first datum to validate an inference about the atmospheric pressure *beyond* his home. He can continue to take more measurements and collect more data, but to the extent that he chooses his home in which to take those measurements, the data do nothing to bolster the original invalid inference.

Of course, the scientist may have other reasons to validate inferences. For instance, if he knows that the air in his home is roughly in equilibrium with the air in a nearby neighbor's home

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[7] Unless you count the "projection postulate," which some might argue is *prima facie* evidence that the QM equations do not always evolve linearly.

[8] Some go further and assert that the beauty, symmetry, and/or simplicity of linear dynamical evolution are evidence for their universal applicability. I disagree: aesthetic arguments are not empirical evidence for a scientific hypothesis. (See, e.g., Hossenfelder (2018).)

(which is at the same altitude), then he might validly infer, based on the single datum from his kitchen, that the atmospheric pressure in his neighbor's home is 1 atm. However, in the absence of other reasons to validate the inference, if the scientist's first measurement in his home is inadequate to infer a universal claim, then subsequent measurements in his home are equally inadequate. If the first datum did not meet the scientist's burden of proof, whatever that burden happens to be, then neither do those subsequent measurements.

That said, let's consider what kind of empirical evidence might support a scientific inference. Imagine two concentric circles, the inner circle designated Region A and the outer circle, which encloses Region A, designated Region B. Several experiments are done and all resulting data points support Hypothesis X. Eventually, after many more experiments, it is noted that no data have ever falsified X, and the statement "X applies universally" becomes generally accepted in the scientific community. At some point, a skeptic points out that all data points are enclosed in Region A and asks for evidence that Hypothesis X can be validly inferred to apply in Region B. Imagine the ensuing hypothetical discussion between the skeptic and proponent of X:

> **Proponent:** "Every single experiment we have done confirms X. We cannot possibly do every experiment, so if we're ever justified in making a scientific inference, then at some point we have to accept that enough experiments have been done and the inference is valid. If you're still skeptical, we'll do another experiment – and look! The outcome, once again, supports X."
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> **Skeptic:** "Yes, but you have specifically chosen experiments that probe Region A. You assert that the data in Region A support an inference that X applies in Region B, but you intentionally choose experiments that can only produce data in Region A. The fact that you have chosen experiments that exclude outcomes in Region B, specifically on the *basis* that they exclude outcomes in Region B, invalidates your data as supporting your inference. You can do a million more experiments, and perhaps none of them will falsify X. But as long as you choose experiments that only probe Region A, those experiments will fail to provide support for an inference that X applies in Region B."
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> **Proponent:** "It is true that we have only probed Region A. But that is because it is very technologically challenging to do an experiment that probes Region B, and we do not currently have the funding. But it is possible in principle to probe Region B, so we will someday be able to do so, and we have no reason to believe that X will not apply to Region B."
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> **Skeptic:** "That is not the question. The question is this: right now, based on the existing empirical data, do you have reason to believe that X *does* apply to Region B? The burden of proof rests with the proponent of the inference."

Of course, in the context of MP, Region A is the microscopic realm and Region B is the macroscopic realm. I will argue in this section that because the only available data come from experiments that are chosen specifically by experimenters on the basis of probing microscopic systems, these data inherently do not and cannot support an inference that QM wave states evolve linearly in macroscopic systems.

The rules of QM allow us to make probabilistic predictions on the outcomes of measurements that differ from the expectations of classical probability. In a very real sense, this is both how QM was discovered[9] as well as what makes an event quantum mechanical. Wave functions of objects can always be written as a superposition of terms (corresponding to distinct eigenstates) having complex probability amplitudes, and interference between those terms in an experiment can alter outcome probabilities from what might be expected classically. A demonstration of quantum effects, then, depends on an interference experiment – i.e., an experiment that demonstrates interference effects in the form of altered probability distributions. In that sense, quantum mechanics is fundamentally about making probabilistic predictions that depend on whether interference effects from terms in a coherent superposition are relevant.[10] Like the proponent in the hypothetical conversation above, proponents of U often claim that no violation of the linearity of QM has ever been observed, or that no experiment has ever shown that the mathematics of QM is not universally applicable. (See, e.g., Deutsch (1985).) How good is this evidence as an assertion to support the inference of U? Consider the following claim, perhaps supported by the vast majority of physics:

"The mathematics of QM applies to every object subjected to a double-slit interference experiment, no matter how massive, because no experiment has ever demonstrated a violation."

Indeed, double-slit interference experiments ("DSIE") over the past century have been successfully performed on larger and larger (though still microscopic) objects, such as a C_{60} molecule. (See, e.g., Arndt *et al.* (1999).) However, to evaluate the extent to which this evidence supports an inference of U, it is necessary to consider how DSIEs are set up and performed. Nature – thanks to the Heisenberg Uncertainty Principle – creates superpositions ubiquitously. Quantum uncertainty, loosely defined for a massive object as $\Delta x(m\Delta v) \geq \hbar/2$, guarantees dispersion of quantum wave packets, thus increasing the size of location superpositions over time. However, interactions with fields, photons, and other particles ever-present in the universe constantly

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[9] The characterization of light as discrete particle-like objects, thanks to Planck's use of $p = \hbar k$ to avoid the Ultraviolet Catastrophe and Einstein's explanation of the photoelectric effect, showed that classical probability is inapplicable to predicting the detection outcome of individual particles in a double-slit interference experiment.

[10] Like all probability rules, a statistically significant ensemble is necessary to obtain useful information. A measurement on any object will always yield a result that is consistent with that object's not having been in a superposition; only by measuring many identically prepared objects may the presence of a superposition appear in the form of an interference pattern.

"measure" the locations of objects and thus decohere[11] these superpositions. (See, e.g., Tegmark (1993) and Joos *et al.* (2013).) This decoherence, which I'll discuss in greater detail in Section II(D), explains both why we don't observe superpositions in our normal macroscopic world and also why visible interference patterns from quantum superpositions of non-photon objects[12] are so difficult to create.

For instance, let's consider the nontrivial process, first performed by Davisson and Germer in 1927, of producing an electron in superposition state $(|A\rangle + |B\rangle)$, where $|A\rangle$ is the wave state corresponding to the electron traversing slit A in a double-slit plate while $|B\rangle$ is the wave state corresponding to the electron traversing adjacent slit B. Electrons, one at a time, are passed through (and localized by) an initial collimating slit; quantum uncertainty results in dispersion of each electron's wave state at a rate inversely proportional to the width of the collimating slit. If the process is designed so that adequate time elapses before the electron's wave state reaches the double-slit plate, and without an intervening decoherence event with another object, the electron's wave will be approximately spatially coherent over a width wider than that spanned by both slits. If the electron then traverses the double-slit plate, its wave function becomes the superposition $(|A\rangle + |B\rangle)$. Because such a superposition does not correspond to its traversing slit A or traversing slit B, it carries no "which-path" information about which slit the electron traversed. If each electron is then detected at a sensor located sufficiently downstream from the double-slit plate, again without an intervening decoherence event, the spatial probability distribution of that electron's detection will be calculable consistent with quantum mechanical interference effects. This lack of which-path information implies that the electron's superposition coherence was maintained and thus the rules of quantum mechanics (not classical probability) would apply to probability distribution calculations.[13]

Because the dispersion of an object's wave function is directly proportional to Planck's constant and inversely proportional to its mass, the ability to demonstrate the wave-like behavior of electrons is in large part thanks to the electron's extremely small mass.[14] The same method of producing superpositions – waiting for quantum dispersion to work its magic – has been used to produce double-slit interference effects of objects as large as a couple thousand atoms. (See, e.g., Eibenberger *et al.* (2013) and Fein *et al.* (2019).) However, the more massive the object, the slower the spread of its wave state and the more time is available for an event to decohere any possible

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[11] The theory underlying decoherence is not incompatible with the assumption of U; in fact, many (if not most) of the proponents of decoherence specifically endorse U. Rather, decoherence is often used to explain why it is so difficult to measure macroscopic objects in (coherent) superpositions.

[12] Interference effects of photons are actually quite easy to observe in part because photons do not self-interact and thus are not decohered by other radiation. Prior to the invention of lasers, a dense source of coherent photons, which confirmed light's wave-like behavior, came directly from the sun.

[13] Indeed, the existence of which-path information – that is, a correlating fact about the passage of the electron through one slit or the other – is incompatible with existence of a superposition at the double-slit plane. (See, e.g., Knight (2020).)

[14] We might alternatively say that the de Broglie wavelength of an electron can be made sufficiently large in a laboratory so as to reveal its wave nature.

superposition. Are there other methods, besides quantum dispersion, to prepare an object for a DSIE? I don't know. However, every successful DSIE to date has indeed depended on quantum dispersion of the object's wave packet, and it is this evidence, not the hypothetical possibility of other experiments, that is available to support (or not) an inference of U.

What does this evidence tell us? To perform a DSIE on an object to demonstrate interference effects in a laboratory, the object is specifically chosen so that quantum dispersion is adequate, and decoherence can be sufficiently prevented, to make the object spatially coherent over a distance exceeding the slits. Because both of these requirements become increasingly difficult with an increase in the object's size, the objects of DSIE experiments are chosen by experimenters specifically *because they are small*. This choice is relevant to the extent to which data, which exist only from experiments that probe microscopic systems, can be used to infer that experiments that probe macroscopic systems would have comparable results. If microscopic objects are intentionally chosen for DSIEs because their size makes (practically) possible their preparation into states that are adequately spatially coherent to allow the subsequent demonstration of interference effects, then the outcomes of those DSIEs cannot provide *new* evidence for an inference that a DSIE on a macroscopic object would have the same outcome (or is even possible to perform).

My goal is not to call into question the usefulness of interference experiments in demonstrating the applicability of unitary QM to objects in those experiments.[15] My goal is to point out that if DSIEs are only performed on objects that are already adequately spatially coherent over a macroscopically resolvable distance, and if experimenters specifically choose objects that are microscopic (because, for example, issues of quantum dispersion and decoherence make other choices impractical), then there is a logical circularity in the claim that wave states always evolve linearly, even in macroscopic systems, on the basis that nonlinear dynamics have never been shown. In other words, if a proponent asserts that the observation that "No DSIE has ever demonstrated a violation of linearity" supports the applicability of the data, which only exist in the microscopic realm, to the macroscopic realm, then that data can't have resulted from experiments that are chosen to test *only* the microscopic realm.

But that is exactly the case. The reason for the choice, in this regard, is irrelevant. I offered above a reason why DSIEs, which so far have depended on quantum dispersion of objects, become more difficult as the objects get larger. Quantum interference effects may be shown in experiments other than DSIEs, but those experiments have so far still probed only microscopic realms. Scientists may offer any explanation they like – an alternative experiment is too complicated or costly, it's not yet technologically viable, whatever – but if an actually-performed experiment is

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[15] For example, if QM is indeed nonlinear through a physical collapse mechanism like that proposed by GRW (1986) or Penrose (1996), then such a collapse might be confirmed by first preparing a system in an appropriate superposition (which should, if properly measured, demonstrate interference effects), and then failing to observe interference effects. The ability to demonstrate, for example, a C_{60} molecule exhibiting interference effects puts a lower limit on the scale (mass, time period, etc.) to which a physical collapse mechanism would act.

chosen *because* it probes the microscopic realm, then the results from that experiment cannot support an inference to the macroscopic realm.

It may well be true that no interference experiment has shown nonlinearity, but if all such experiments performed so far only probe the microscopic realm and – more importantly – if these experiments are quite literally chosen *because* they only probe the microscopic realm, then the fact that no interference experiment has ever shown a violation of linearity is simply not evidence to support the inference that QM is *universally* linear.

D. Empirical Data Oppose an Inference of U

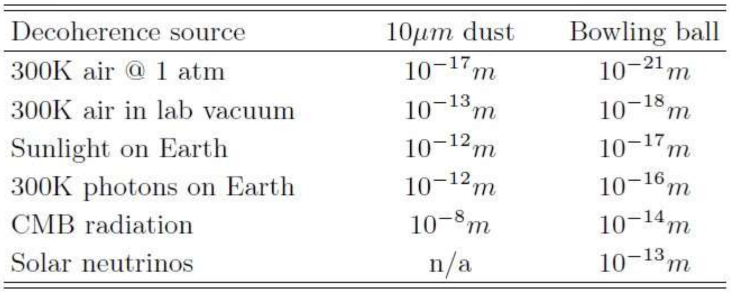
In Section II(C), I argued that because all available data to support U come from experiments specifically chosen on the basis of *not* probing the macroscopic realm, these data are inadequate to validly infer U. This observation is not evidence *against* the inference of U, which could be made for different reasons (although I am not aware of other reasons than those discussed in prior sections), but rather that the empirical data cannot meet the proponent's burden of justifying that inference. In this section, I will show that the experimenter's choice is limited by the nature of the physical world in such a way that any attempts to probe systems beyond the microscopic realm seem to abut against an asymptotic limit, beyond which it is impossible to probe for all practical purposes ("FAPP"). I will argue that this FAPP limit is actually *prima facie* evidence against an inference of U.

If the reason that interference experiments are chosen to probe only the microscopic realm is merely that of convenience, or insufficient grant funding, or technological limitation, then my argument might be limited only to the conclusion that current empirical data do not support an inference of U, in which case the proponent's burden of proof remains but does not necessarily *increase*. However, if it turns out that interference experiments are chosen to probe only the microscopic realm because there is something about the physical world, directly related to the size of systems, that makes it impossible (at least FAPP) to probe larger systems, then this would serve as empirical evidence *against* U, in which case the proponent's unmet burden of proving the inference of U is far greater. I will now argue that the rate of quantum dispersion, which decreases with increasing size, coupled with decoherence mechanisms, which increase with increasing size, conspire to force the experimenter to choose interference experiments that only probe the microscopic. That is, limitations inherent in nature, not the scientist's limitations vis-à-vis funding or ingenuity, are what differentiate "microscopic" from "macroscopic" and prevent the scientist, for all practical purposes, from performing an experiment that would confirm interference effects in the macroscopic realm.

Let me elaborate. If an experimenter can rely on quantum dispersion to put a molecule in adequate spatial coherence to measure interference effects, why can't he do that for a dust particle or a cat even if, as pointed out in Section II(C), it might be very challenging? Consider the difficulty in performing a DSIE on a dust particle. Let's assume it is a 50μm-diameter sphere with a density of 1000 kg/m^3 and has just been localized by an impact with a green photon ($\lambda \approx 500$nm).

How long will it take for its location "fuzziness" to exceed its own diameter (which would be the absolute minimum spatial coherence allowing for passage through a double-slit plate)? Letting $\Delta v = \hbar/2m\Delta x \approx 10^{-18}$ m/s, it would take around $5\mathrm{x}10^{13}$ seconds (about 1.5 million years) for the location uncertainty to reach a spread of 50µm.[16] In other words, if we sent a dust particle into deep space, its location relative to other objects in the universe is so well defined due to its correlations to those objects that it would take over a million years for the universe to "forget" where the dust particle is to a resolution allowing for the execution of a DSIE.[17] In this case, information in the universe would still exist to localize the dust particle to a resolution of around 50µm, but not less. Unfortunately, this rough calculation depends on a huge assumption: that new correlation information isn't created in that very long window of time. In reality, the universe is full of particles and photons that constantly bathe (and thus localize) objects.

Thus there is a trade-off in the delocalization caused by natural quantum dispersion and localizing "measurements" caused by interactions with the plethora of stuff whizzing through space. This trade-off is heavily dependent on the size of the object; a tiny object (like an electron) disperses quickly due to its low mass and experiences a low interaction rate with other objects, allowing an electron to more easily demonstrate interference effects. On the other hand, a larger object disperses more slowly while also suffering a much higher interaction rate with other objects. These observations can be quantified in terms of coherence lengths: for a particular decoherence source acting on a particular object, what is the largest fuzziness we might expect in the object's center of mass? And, if we're hoping to do a DSIE, does this fuzziness exceed the object's diameter?



Decoherence source	$10\mu m$ dust	Bowling ball
300K air @ 1 atm	$10^{-17}m$	$10^{-21}m$
300K air in lab vacuum	$10^{-13}m$	$10^{-18}m$
Sunlight on Earth	$10^{-12}m$	$10^{-17}m$
300K photons on Earth	$10^{-12}m$	$10^{-16}m$
CMB radiation	$10^{-8}m$	$10^{-14}m$
Solar neutrinos	n/a	$10^{-13}m$

Table I. Some values of coherence lengths for a 10µm dust particle and a bowling ball caused by various decoherence sources, given by Tegmark (1993).

Tegmark (1993) calculates coherence lengths (roughly "the largest distance from the diagonal where the spatial density matrix has non-negligible components") for a 10µm dust particle and a bowling ball caused by various decoherence sources, as shown in Table I. Even in deep space, cosmic microwave background ("CMB") radiation alone will localize the dust particle to a dimension many orders of magnitude smaller than its diameter, thus ruling out any possibility

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[16] Tegmark (1993) notes that macroscopic systems tend to be in "nearly minimum uncertainty states."

[17] This estimate completely neglects the additional time necessary to subsequently measure an interference pattern.

for that object to become adequately delocalized (and thus adequately spatially coherent) relative to the universe to perform an interference experiment. The prospects are far worse for a bowling ball-sized cat – it simply cannot physically happen.

In other words, as at least a practical matter, the physical world is such that there is a size limit to the extent that quantum dispersion can be relied upon to perform a DSIE. Having said that, no one seriously argues (as far as I know) that SC or WF could be produced, even in principle, through natural quantum dispersion. Rather, the typical argument is that SC/WF could be produced through amplification of a quantum event via a von Neumann (2018) measurement chain.[18] Crucially, however, the purported linear amplification of a quantum superposition *assumes* universal linearity of QM, which means that it cannot be logically relied upon to contradict an argument (as in this paper) challenging that assumption. Further, there is no empirical evidence that quantum amplification ever has produced a measurable macroscopic quantum superposition.[19] In other words, without assuming that quantum amplification can accomplish what quantum dispersion cannot – i.e., ignoring the logical circularity of assuming the very conclusion that I am questioning – one must concede that existing empirical evidence does not support the assertion that a DSIE can in principle be performed on macroscopic objects like a cat.

Consequently, a scientist's choice in designing interference experiments is not merely on the basis of convenience – rather, the physical world is such that interference experiments inherently become *increasingly difficult at an increasing rate* as the size of an object increases. There seems to be an asymptotic limit[20] on the size of an object on which we can, for all practical purposes, perform a DSIE. The fact that nature herself imposes this limit is – or should be – telling us something fundamental about whether QM wave states always evolve linearly. More to the point, this FAPP distinction between the microscopic and macroscopic realms serves as evidence against an inference of U.

Importantly, I am *not* asserting that the above analysis shows that performing an interference experiment on a cat (or anything else) is impossible in principle. Rather, it shows that

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[18] Even if quantum dispersion of a cat's wave packet could allow a DSIE, it is unclear why a cat state $|cat>_A$ corresponding to the cat passing through hole A would substantively differ, other than its position, from cat state $|cat>_B$ corresponding to the cat passing through hole B. If the correlations among objects (such as atoms) within the cat do not depend on the cat's position in hole A or B, then there is no interesting difference (in my opinion) between these two cat states, as it is really the same cat both from the cat's and an observer's perspective. What makes the SC and WF problems interesting is the amplification of a quantum state into an entangled macroscopic superposition in which one term includes a state of a cat, such as |dead>, while another includes a *magnificently different* state of the cat, such as |alive>.

[19] A related objection is whether it is possible to adequately isolate or shield a macroscopic object from decoherence sources long enough for dispersion to work its magic. The answer is no, for reasons, including logical circularity, that exceed the scope of this paper. But, like the hypothetical proposed fix of amplification, there is no actual evidence that shielding or isolation ever has produced a measurable macroscopic superposition.

[20] I don't mean this in a rigorous mathematical sense. Rather there is some rough object size below which we are able, as a practical matter, to show interference effects of the object and above which we simply cannot. We might loosely call this size the "Heisenberg cut."

a fundamental feature of our physical world is that our efforts to demonstrate interference effects in DSIEs for larger systems have quickly diminishing returns; the harder we try to increase the size of an object to which QM is verifiably linear, the more slowly that size increases. There is at least some physical size (perhaps within an order of magnitude or two of the dust particle in Table I) above which no conceivable experiment, no matter how technologically advanced, could demonstrate interference effects. The fact that such a size exists to physically distinguish the "macroscopic" from the "microscopic," which as a practical matter *forces* us to choose interference experiments that probe only the microscopic regime, is strong empirical evidence against an inference of U. In other words, the existence of a FAPP limitation on the size of objects subject to interference experiments, even if there is no in-principle limitation, is itself evidence against an inference of U.

III. CONCLUSION

The burden of showing the validity of *any* scientific inference always lies with those who assert the inference. Scientists may reasonably disagree about what standard might ordinarily be required, but that burden certainly increases if, as in this case, the inference is logically incompatible with one or more assertions (such as statement P1) for which we ostensibly have a great deal of evidence. In other words, the fact that the century-old measurement problem continues to be taken seriously is good reason to subject all of its logically inconsistent assumptions, including the inference of U, to a higher standard of proof.

Despite this heightened burden, I argued in Section II(C) that the empirical data, all of which derive from experiments that are chosen specifically to probe microscopic objects, do not meet *any* burden of showing that comparable experiments could be performed on macroscopic objects or that such experiments would verify U. I further argued in Section II(D) that the existence of a FAPP size limit, derived from the fundamental nature of the physical world, to which interference experiments can be done is itself evidence against an inference of U. Quite simply, the assumption of U is not scientifically justified as a valid inference because relevant empirical evidence supports only ¬U. Not only have proponents failed to meet their burden to prove an inference of U, the existing data actually increase that burden.

I cannot offer or conceive of any rational scientific or philosophical basis on which to accept such an inference. For these reasons alone, from a scientific standpoint, MP should be outright dismissed – not because it has been solved, but because it should never have arisen as a problem in the first place. MP, a contradiction, depends on the truth of at least two logically incompatible statements, one of which is U. The burden of proof falls on proponents of U to support the validity of an inference of U, but I have shown above that the best empirical evidence is inadequate to support, and in fact opposes, an inference of U. That burden remains with the proponents, not the skeptics; however, in light of the arguments above, that unmet burden is exceptional and may prove simply insurmountable.

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