Less Interpretation and More Decoherence in Quantum Gravity and Inflationary Cosmology

Crull

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Abstract

I argue that quantum decoherence—understood as a dynamical process entailed by the standard formalism alone—carries us beyond conceptual aspects of non-relativistic quantum mechanics deemed insurmountable by many contributors to the recent quantum gravity and cosmology literature. These aspects include various incarnations of the measurement problem and of the quantum-to-classical puzzle. Not only can such problems be largely bypassed or dissolved *without* default to a particular interpretation, but theoretical work in relativistic arenas stands to gain substantial physical and philosophical insight by incorporating decoherence phenomena.

1. Introduction: Quantum Theory's Bête Noire

The consensus view emerging from recent work in relativistic quantum theory—specifically, within both covariant and canonical approaches to quantum gravity and inflationary cosmology—is that deep puzzles in these fields cannot be resolved without first assuming a solution to the measurement problem inherited from quantum mechanics. Entwined with the measurement problem is a felt need to "close the explanatory circle" by providing a quantum theoretic explanation for classical phenomena, i.e., to tell a thoroughly-quantum story as to why macroscopic systems consistently appear stable with respect to certain degrees of freedom as opposed to others.

Consider [13] wherein we find the following statement:

> As regards the account of classical phenomena, the very formulation of non-relativistic quantum mechanics poses a problem that is known as the measurement problem. Relativistic quantum mechanics—that is, quantum field theory—faces this problem as well. Quantum gravity being the project of unifying quantum field theory with general relativity theory, it is not to be expected that quantum gravity will solve the measurement problem. Nonetheless, any approach to quantum gravity that is to be empirically adequate has to take a stance on the measurement problem, the question being how to account for measurement outcomes within a quantum theory, including a quantum theory of gravity. (p. 44)

Thus it is assumed that all approaches to quantizing gravity must first adopt a particular interpretation of quantum mechanics from which to proceed. Though the motivation for assuming a given interpretation is slightly different in [34], theirs is a similar method:

> An honest application of quantum mechanics to cosmology requires, by necessity, the use of an observer independent interpretation of the theory. That is, a version of the quantum formalism not fundamentally based on the notion of measurement or on that of an observer external to the studied system. The standard interpretation, then, is inadequate in this context because it relies too heavily either on measurement as a primitive term or on a division of the systems and process of the world into macroscopic and microscopic (or observer/observed, classical/quantum, irreversible/reversible, conscious/unconscious, etc...). (pp. 114-115)

Not only do Okon and Sudarsky believe interpretation is necessary, but they argue that three important issues plaguing relativistic theories are resolved given a collapse interpretation: the origin of the seeds of cosmic structure, the problem of time in quantum gravity, and the information loss paradox for black holes.

Okon and Sudarsky follow in the line of inquiry proposed a decade ago in [17], who provide the following explanation of their work (p. 276):

> We shall sketch how most, and perhaps all, of the conceptual problems of canon- ical quantum gravity vanish if we insists upon formulating our cosmological theories in such a matter that it is reasonably clear what they are about—if we insist, that is, upon ontological clarity—and, at the same time, avoid any reference to such vague notions as measurement, observer, and observables.

What these authors mean by "ontological clarity", it turns out, is to render things solely in terms of the de Broglie-Bohm theory. Thus we have yet another project in quantum gravity and cosmology which (implicitly or otherwise) assumes the measurement problem must be resolved before progress can be made.

But is the early incorporation of a particular interpretation really necessary? — Not just for addressing both new and inherited problems, but for making substantial progress in relativistic domains? I say no, and no again. The purpose of the following essay is to argue that quantum decoherence in and of itself addresses precisely those aspects of the measurement problem many believe require resolution before going onwards, and for resolving new issues within relativistic applications of the theory. Relatedly, decoherence is the dynamical process which provides the missing segment of the explanatory circle: nothing beyond the standard formalism is required for telling the story of the quantum-to-classical transition entirely quantum-mechanically. Granting these two claims, there is a way to get further into the physics of both cosmological and quantum gravitational theories without declaring adherence to one interpretation or another. Not only do I argue one can remain interpretation neutral for longer, but I also suggest that work in relativistic domains might benefit greatly by incorporating decoherence phenomena.

I begin with an investigation of the closely-related conceptual problems inherited from quantum mechanics, and what decoherence has to say about each—defining measurement, the measurement problem (in all its guises) and closing the explanatory circle. I then examine two different but representative responses to quantum theory's bête noire in relativistic theories: Esfeld and Vassallo's [13] response, in which it is argued that the measurement problem inevitably leads to a dilemma when applied to canonical quantum gravity, and the response of [34], which advocates the adoption of a particular interpretation (collapse) in order to resolve both long-standing issues and those emerging within cosmology and quantum gravity. In the case of responses like the first, I argue decoherence allows us to dissolve (most of) the beast and therefore make progress in the suggested theoretical arena; regarding responses of the latter type, I argue that decoherence alone can provide answers to the new riddles, and point out that such theoretical work has been done already. I close with a few suggestions for further work in keeping with this paper?s mantra: "more decoherence, less interpretation!"

2. Applying Decoherence to the Measurements, Interpretations and the Explanatory Circle

I define decoherence in keeping with the vast experimental literature being generated through applications of canonical models of decoherence: it is a dynamical process whereby a system's phase relations in particular bases become decohered or randomized by commuting with external (environmental) degrees of freedom. The cause of decoherence processes is entanglement with external degrees of freedom (weakly defined; one can even let a single electron's spin be the "system" and its translational degrees of freedom the "environment" and observe decoherence). Decoherence of a system will suppress to extraordinary degree interference terms in the decohered basis, such that further interactions will practically always "see" the system in an eigenstate of the basis or bases most affected by decoherence (that is, with respect to system degrees of freedom that commute most rapidly and efficiently with environmental degrees of freedom).

Note that entanglement can arise apart from interaction. Thus, entanglement is necessary and sufficient for decoherence irregardless of whether or not an interaction ("measurement event", if you will) has taken place. Since any given system is entangled with at least one other system (if not initially, then immediately thereafter), decoherence will ensue. Thus one may assume that a vast majority of systems are already decohered or undergoing decoherence in various bases.

2.1. Measurements

Most will agree that for a measuring entity to count as such requires only that some information about the system be gained by the entity, and perhaps also that this information be in principle able to be gathered at a later time. If the sole necessary condition is that information be transferred from system to environment, and if entanglement enables such a transfer of information even without interaction, then the knowledge that entanglement begets decoherence between environmental degrees of freedom and a system is sufficient for understanding how this process enables the environment to effectively measure that system in interaction-less cases. Even were one to demand weak interaction with external degrees of freedom in order for a measurement to occur, one must concede that uncontrollable measurements of this sort are taking place constantly via scattering of stray particles in a manner sufficient to induce decoherence. No reference to an observer, a measuring device or even a measurement is therefore necessary when decoherence processes are involved.

2.2. The "Measurement Problem" Problem

The frequency with which a variety of issues are conflated with the measurement problem is itself a problem. Take, for example, just such a confusion from the opening comments in [20]—a foundational paper on decoherence, written by physicists, who are nonetheless driven by questions of a philosophical nature. They begin the paper as follows:

> The relation between classical and quantum mechanics is at the heart of the interpretation problem of quantum theory. Outcomes of measurements are usually expressed in classical terms at a certain level of description: the pointer position is assumed to be definite like the position of a classical point mass in space. On the other hand, the general applicability of quantum theory—that is, essentially, the superposition principle—is important for many phenomena of macroscopic objects, for example, in solid state physics. However, if applied rigorously, this principle would lead to possible states never observed in nature, like superpositions of macroscopic objects in very different positions or of other "macroscopically different" states. One may also wonder why microscopic objects are usually found in energy eigenstates, whereas macroscopic objects occur in time-dependent states. (Ibid., p. 223)

Within this brief paragraph one can see traces of the following issues: the quantum- to-classical transition, the problem of why macroscopic superpositions are never observed, why certain eigenstates seem to dominate at different scales, and the assumption that measurement outcomes are definite. Furthermore, it has been (and continues to be) claimed that decoherence solves the measurement problem.[1] But *which* measurement problem? While a study of decoherence may have something to say regarding *certain* questions grouped under the measurement problem rubric, the theory does not, solve all the questions sometimes attributed to it. Max Schlosshauer [37, pp. 49-50] has devised a catalogue of the different problems sometimes called "the

[1] A representative sample of this sort of claim can be found in [3,4], in which Anderson argues that decoherence eliminates the need to invoke a collapse of the wave function. See also the response to Anderson by Adler [1]; more glimpses of the question of decoherence solving the measurement problem can be found in [2,7,40,42].

measurement problem"; I borrow his breakdown of the problem into three distinct questions, arguing for each that decoherence theory either obviates the question or cannot be expected to answer it.[2]

2.2.1. The Problem of the Preferred Basis

The superposition principle, which lies at the heart of quantum theory, has the logical consequence of allowing a vast number of basis choices in which to carry out measurements of a given quantum system. The question then arises: why do we consistently observe systems in only a small—and consistent!—subset of all possible bases? This subset of bases corresponds to classical observables in an overwhelming majority of cases (i.e., to diagonalized bases). For example, one usually observes larger systems in position bases but not superpositions of position bases. Given the statistical improbability of always observing bases that are classical, why should such preferences for them appear in nature?

Zurek [44] and Schlosshauer [37], among others, argue that decoherence theory provides something by way of an explanation to the puzzle of why certain bases (and bases that typically correspond to classical variables) appear to be favored. Zurek explains that decoherence brings about a process he has named "environment-induced superselection"—often abbreviated "einselection" in the literature—and it is this latter process that accounts for the prevalence of certain bases in nature. Einselection refers to a dynamical process that arises as a system continues to interact with environments: during interaction, the space of possible superpositions is narrowed to a subset consisting of those states of the system dynamically *robust* with respect to the environment's effects. In other words, strongly favored bases are those in which the quantum system is resilient to the quantum-coupling influence of the environment, either because the respective degrees of freedom do not commute or because they only weakly interact.

A provocative example of decoherence dynamically selecting a preferred basis involves a study done in [12]. The authors consider the evolution of a spin quantum system in a spin bath (an environment at thermal equilibrium) and demonstrate that under limiting conditions regarding system-environment

[2]Though I follow Schlosshauer's analysis of the measurement problem, my arguments for what decoherence has to say about each one are not necessarily his.

interactions certain polarization components of the system stabilize very rapidly.

The master equation for the sort of model considered by Cucchietti et al. contains a polarization vector with components for each of the three possible spin axes. It is then simply a theoretical exercise of taking the equation as input and mapping its evolution through time, observing which component of the polarization vector stabilizes in various cases. The component that is most robust under environmental interaction will determine into which eigenstates the system may fall upon measurement. They considered two limiting cases: the first interaction they modeled involved assuming weak intrinsic dynamics of the system, and the second interaction involved the limit of strong intrinsic system dynamics. Strong intrinsic dynamics might be generated in a quantum system by that system's polarization degrees of freedom becoming entangled with its own translational degrees of freedom. When the system has strong dynamics internally, the system self-Hamiltonian dominates the total Hamiltonian (which also includes an environmental self-Hamiltonian and an interaction Hamiltonian) and one expects little or no environmental effect. In the limiting case of weak system dynamics, the interaction Hamiltonian will govern the evolution of the total Hamiltonian, and one expects to see the system responding keenly to the influence of the environment.

This is in fact what Cucchietti et al. [12] found. In modeling the weak limiting case, the system evolved such that the x and y components of the polarization vector decayed extraordinarily rapidly to approximately zero and maintained values approaching that limit. The z component, however, remained comparably stable at a value substantially greater than the other axes. This means that in the case of weak intrinsic dynamics, the effect of the system-environment interaction was such that two of the three possible spin bases were quickly and effectively suppressed while the z basis—corresponding to possible measurements of the quantum system in either a spin up or spin down state—was largely impervious to environmental effects.

In the case of strong intrinsic dynamics, the system?s evolution results in the steady decay of oscillating values for the y and z polarization bases but a stable value for the x component. This leads to the dynamical emergence of energy as the most robust basis, and indeed measurements on the system in this case yield (apparent) energy eigenstates.

Thus decoherence gives us a dynamical story explaining why certain bases seem to be preferred in different situations. No spooky, biased world consciousness is at work: it is only the dynamics of entanglement manifest according to the nature of the given system?environment interaction.

2.2.2. The Problem of the Non-Observability of Interference

To illustrate how decoherence explains away this problem, consider the paradox of optical isomers. The two primary configurations for optical isomers are symmetric with respect to parity. However, certain molecules like sugar and ammonia fail to exhibit this symmetry due to the complicated relationship among the elements composing them, and are thus referred to as chiral molecules, and are identified in terms of left-handed states or right-handed states.

Applying the superposition principle to this case, we note that the proper spatial state description for a chiral molecule should include left-handedness plus right-handedness, with certain phase amplitudes attributed to each component. This is similar to double-slit experiments where the position of the particle is described by a superposition of the trajectory through the first slit (with some phase amplitude) plus the trajectory through the second slit (with some phase amplitude). Since chemists know their quantum mechanics, they expect to find optical isomers occupying a super- position of left- and right-handedness a vast majority of the time. And this makes sense: the probability that the phase amplitudes of both handed states are non-zero (and thus give rise to a measurable superposition) is far greater than the probability that one state has phase amplitude 1 and the other 0.

Enter the "paradox": while the ammonia molecule is often observed in a superposition of chiral states (as we expect) and rarely measured in a definite handed state, the structurally similar sugar molecule has only ever been measured in one or the other of the definite handed states, and never a superposition thereof. Why does nature act according to our expectations regarding the ammonia molecule's spatial position but contrary to our expectations in the case of the sugar molecule? In [20] the authors tackled the paradox of optical isomers from the vantage point of system-environment interactions. They considered a parity eigenstate of these molecules in interaction with a single unpolarized photon (i.e., one with trivial intrinsic dynamics). Joos and Zeh applied their new Hamiltonian—with a component describing the molecule qua system, the photon qua measuring device, external photons as the environment and an interaction component—and found that in the case of the sugar molecule, the photon-plus-molecule system became strongly correlated to environmental photons. This entanglement destabilized and prodigiously damped the phase amplitudes existing among the sugar molecule's handed states, which is to say that the superposition decohered leaving the handed states stable under environmental influence. Hence our rarely (if ever) finding sugar in a left-plus-right superposition state.[3]

Joos and Zeh calculated rudimentary values for the rate of decoherence of the chiral states and found that it would happen on a timescale many orders of magnitude faster than the measurement process itself. In other words, before a measurement event indicating which state the sugar molecule was in could even occur, decoherence had already destabilized to prodigious degree the relationship between various components of the superposition, and rendered the left- the right-handed components effectively discrete. So this type of molecule, handed eigenstates remain robust under environmental influence while the phase relations between them are suppressed to near-nothingness.

Decoherence also satisfactorily explains the behavior of the ammonia molecule:

[3]Granted, it would be hard to say just what a superposition like that would look like: would the molecule be smeared out between the two positions? or flash back and forth between them? or phase in and out of existence? The question might then arise: how do we know these superposed states are still part of the system's description if we cannot really measure them, and do not even know what it would look like if we could? In response, recall that interference terms are the physical manifestation of interference among different states of a superposition, i.e., phase relations among component states. There have been a number of significant experiments done in recent years in which physicists were able to observe interference phenomena in systems even larger than sugar and ammonia molecules. That is to say, experimenters have been able to stave off decoherence long enough in highly-engineered situations that they could observe interference—which can only be explained as arising from superposed states—in systems that are enormous compared to the sorts of things typically described using quantum mechanics. In fact, the 2012 Nobel Prize in Physics was jointly awarded to Haroche and Wineland, whose respective labs have made extensive progress in measuring the quantum behavior of systems at the microscopic and mesoscopic scale (cf. [8,31,33,35] for details). Physicists have now observed superpositions in systems as large as Rydberg atoms and 'Bucky balls'—C_{60} and C_{70} fullerenes. One wonders whether our engineering might ever become so clever that we could send an elephant into a double-slit apparatus and get a pachydermatous interference pattern out. It is, in principle, possible.

the dynamics of *its* spatial states under environmental interaction are such that chiral superpositions do not become correlated with the photon environment. As such, no decoherence occurs among phase relations in the position basis. Superpositions of handed spatial states in the case of ammonia, then, remain stable and are what we observe. Joos and Zeh conclude from this that decoherence processes can account for the chemists? paradox: it is nothing above and beyond the quantum dynamics of a *total* system—that is, system plus environment—that explain measurement results for both of these optical isomers. Nature is not fickle—it does not favor pure chiral states in the case of one molecule but superposed chiral states in the case of the other. Instead, it is the specific way in which a specific molecule entangles to a specific environment that results in the increased or decreased "observability" of specific states.

A separate question involves the effects of these supposedly ubiquitous superpositions, namely, interference phenomena. If superpositions ought to be the usual measurement outcomes, then interference effects ought to be much more prevalent in nature, yet they are observed much less frequently than one would expect. It took some years for physicists to realize that the interference terms missing from observation were not necessarily absent but perhaps merely hidden. However, this puts a different puzzle on the table: *why* do interference phenomena become hidden so quickly? It cannot be due to the dissipation of the system's energy into the environment (or vice versa), for decoherence theory provides us with the means to calculate a rate of decoherence (given some basic assumptions about the behavior of the system and the environment) and it is extremely fast—orders of magnitude faster than the rate of dissipation in uncontrolled environments.

The usual textbook explanation for the non-observability of interference in matter, as reported in [37, pp. 55-56], goes something like this: consider the analogy of a classical light wave and the interference that results from its passing through a suitable diffraction apparatus—e.g, the traditional double-slit arrangement. In this case, students are told that the distance between the slits must be comparable to the de Broglie wavelength of the light in order for interference phenomena to appear, owing to the resolution power of a double-slit apparatus so constructed. It follows that in the case of matter waves, interference phenomena are practically hidden from us because we lack the technological ability to manufacture a double-slit apparatus with adequate resolution at that scale.

There is no denying that this technological limitation prevents our observation of interference between matter in many laboratory setups. However, this does not explain instances of clever engineering wherein experimenters have been able to observe interference effects at a scale well within the operationally defined scopic domain. Such is the case, for example, in experiments performed with massive C_{70} molecules (cf. [10]) and the even more massive fluorinated fullerene molecules ($C_{60}F_{48}$) studied by the same research group [18].[4]

Yet there must be some other explanation for the pedestrian fact that in usual, uncontrolled situations, interference is not observed as often as one should expect. The further explanation is once again provided by decoherence. The phase relations of the system's post-entanglement state in the emergent einselected basis become suppressed with great rapidity upon interaction with the environment's mutually-commuting degrees of freedom. Such mutually-commuting quantities in the environment can then be considered to continually measure or observe that quantity within the system. Put simply, we do not observe interference because without utmost precision and care, we are always dealing with a system whose prior interaction with a huge number of external degrees of freedom effectively hides (but note well, *does not destroy*) interference terms.

2.2.3. The Problem of Outcomes

This problem is the one most frequently taken to be *the* measurement problem in the literature, and is perhaps the question most directly related to explaining the quantum- to-classical transition. Schlosshauer [37, p. 57] separates the problem of outcomes into two questions: the generic problem of why we get definite outcomes from measurements on quantum systems with probabilistic state distributions, and the specific question of why we get the *particular* outcome we do. Decoherence obviates the first question but cannot answer the second; consequently, it is only the specific problem of outcomes one should consider as-yet unresolved.

[4] For more experiments in the mesoscopic regime, see [5,6] or the numerous experiments discussing the production of stable mesoscopic superpositions, or so-called "Schrödinger kittens."

- **The *generic* problem:** why do we get definite measurement outcomes?

  Because decoherence—which is effectively universal and exceedingly rapid in most uncontrolled cases—does not destroy but only suppresses interference terms, what we observe to be a definite outcome is only apparently so. The nonlocal aspects of the system still exist, but they are damped to such an extent that their consequences would be unobservable in any realistic timeframe.[5] Thus the answer to the question, "Why do we get definite outcomes for quantum measurements?" is simply that we *don't* get definite outcomes, except in the smallest percentage of situations. What we usually get is a distinctly quantum (nonlocal) state of affairs that is empirically indistinguishable from a definite outcome or eigenstate in a preferred basis.[6]

- **The *specific* problem:** why do we get the particular outcome we do?

  While the generic question asks why we usually measure eigenstates in a preferred basis—a definite point on the screen instead of a fuzzy splotch, say—the specific question asks why we measure a *given* eigenstate instead of (possibly equiprobable) others in the robust basis—e.g., an apparently definite point on the left side of the screen instead of the right side, or an alive cat instead of a dead one.

  Though decoherence obviates the general question, I agree with the assessment of Schlosshauer and others that the question of specific outcomes is not, and perhaps cannot, be answered by decoherence. Thus, this problem alone remains outstanding of all those routinely categorized as "the measurement problem." Decoherence explains why it is that measurement outcomes assume the appearance of eigenstates despite the far greater statistical likelihood of measuring superposed states. But once the process of decoherence has dampened interference

[5] Although for certain interactions there is a time of recurrence, or a time after which nonlocal aspects of the system will re-achieve their initial values, this length of time is so great in most situations involving uncontrolled decoherence as to be ignored.

[6] By "empirically indistinguishable" I mean there exists no executable test or event or measurement, etc., that would likely ever—even if repeated over the span of several lifetimes of the universe—yield results differing from what we usually expect in this regard, e.g., a macroscopic system occupying a superposition of position eigenstates.

> terms between components of possible superposed states, the component states themselves remain robust possible states. How it is that one among these comes to be measured remains an open question.
>
> It is my suspicion that at the root of this desire to render quantum mechanics complete is a deep discomfort with accepting that quantum mechanics reveals a truly indeterministic world—a world that does not contain a causal (or any other) story about the "choice" of one approximately non-superposed state over other approximately non-superposed yet equiprobable states. If physics has taught us that the world is indeterminate, then an answer to the specific problem of outcomes might well lie outside the scope of what is accessible or demonstrable.

In sum, when the measurement problem is broken down into separate questions as above, we see that decoherence has something to say about three of the four: the problem of preferred basis is solved by considering the dynamical consequences of system-environment interactions, the problem of lack of observable interference is explained by the suppression of interference terms resulting from system-environment interactions, and the general problem of outcomes is seen to be ill-posed, in that we are rarely obtaining definite results but results that have the appearance of definiteness due to decoherence. Decoherence itself cannot answer the question of specific outcomes. I have suggested that to hope for this sort of answer from physics may be a result of our misunderstanding or hesitating to accept thorough-going indeterminacy in our world.

2.3. Interpretations

By incorporating decoherence as one should, within the standard formalism of quantum mechanics, one can make substantial progress eliminating the black beast; interpretations of quantum mechanics need only be invoked if one requires a physical explanation for the problem of specific outcomes. There is a cost-benefit analysis to be carried out here, as no single interpretation currently on offer is free of significant philosophical baggage. Everett interpretations typically exceed the commitments necessary for standard quantum mechanics by positing extra ontology like branching worlds or minds to explain why *this* outcome; if one does not regard such interpretations as satisfactorily answering the specific problem of outcomes, then in light of the interpretation-free explanations provided by decoherence and the standard formalism, it seems one pays a rather high price for little gain.

Historically, Bohmian mechanics and collapse interpretations were developed precisely in order to explain the appearance of classical phenomena (definite outcomes being among these). Now that we largely understand such puzzles in virtue of entanglement and decoherence, it would seem that the primary motivation for adopting a Bohmian or collapse interpretation has been pulled out from under one. This historical point aside, if one focuses on the specific problem of outcomes as the source of the anxiety, then the same is true of these interpretations as of many Everettian explanations: they supplement the standard formalism in order to get answers. In the case of Everett, this supplement is often philosophical but sometimes carries inescapable physical ramifications; in the case of Bohm and collapse interpretations, one must certainly admit additional physics alongside one?s philosophy, e.g., the quantum potential or a non-unitary collapse mechanism. But if important theoretical work can be done without going these extra steps in any given direction, don't we stand to gain from the sustained generality of such an approach?

2.4. Closing the (Non-vicious) Explanatory Circle

I reiterate what has been said above: decoherence qua a physical process resulting from entanglement among various degrees of freedom brings with it no supplement to the physics, which has been known in full for nearly a century now. It introduces no new physical principles, nor is it an interpretation. Instead, decoherence presents a fascinating lens through which we might understand with greater precision the dynamics giving rise to quantum phenomena, enabling us to study the strange effects of entanglement from a broader perspective.

Earlier papers written on decoherence often include arguments for the universality of decoherence in a stunning majority of realistic cases, and calculate the extraordinary rapidity of decoherence compared to other physical processes. These results have led some to claim that decoherence provides the key for understanding why there exist in the pantheon of everyday objects things that can be labeled classical objects despite to a fundamentally quantum world.

This particular mismatch between what is observed and what is known and expected regarding quantum behavior and phenomena is commonly referred to as the problem of the quantum-to-classical transition. In his 2012 entry on decoherence for the *Stanford Encyclopedia of Philosophy*, Bacciagaluppi introduces the idea of coming full circle as follows:

> The question of explaining the classicality of the everyday world becomes the question of whether one can *derive* from within quantum mechanics the conditions necessary to *discover and practise* quantum mechanics itself. (Sect. 3.3, emphases original)

Decoherence accomplishes precisely this: it was in virtue of analogies with classical mechanics that we were able to develop quantum mechanics, and now we find that it is in virtue of quantum processes that objects appear classical to begin with. More to the point in the essay that follows: it is in virtue of processes *entirely described within the standard formulation of quantum mechanics* that phenomena in all regimes—micro, meso and macro—can be given quantum-mechanical explanations.

To put it another way: by pursuing the consequences of quantum principles, we learn that it is a quantum feature of the world—namely, the universality of entanglement and the decoherence processes resulting therefrom—that gives rise to states of affairs that are empirically indistinguishable from "classical" states of affairs.

The related concept of closing the *epistemic* circle is from [38], and it is meant to describe the fascinating historical fact that it was only by implementing classical concepts and the correspondence principle between classical mechanics and quantum mechanics that the latter was articulated, yet now we find that it is in virtue of quantum mechanics that the full truth underlying classical mechanics (i.e., that it is only apparent and that this appearance is contingent upon quantum processes) comes to light. Decoherence is the crucial link needed to close this loop, and it seems to do so beautifully.

There are some who argue that closing Shimony's epistemic loop through decoherence is viciously circular. In particular, a few authors have taken refuge in the correspondence principle, demanding that this principle (whose intended function was primarily heuristic) nevertheless remain *the* point on which all questions about the classical-to-quantum transition are settled.

Related to these convictions are notions still expressed in philosophy and physics (though becoming less frequent) that puzzles like nature's apparent preference for classical observables must be explained in terms of selection or super-selection principles, which are primitive notions.

To begin with, consider the just warning on this point given in [9, p. 354] (emphasis original):

> Being a pragmatic precondition for is not tantamount to *Being* full stop. Having been used as an indispensible [sic] starting point of an epistemic process is not equivalent to having more ontological weight than the end produce of this very epistemic process. One should realize that choosing a starting point has no ontological implication at all.

The historical fact of the matter is that being creatures of a particular size and with particular faculties of perception and interpretation and so forth, we came to know our world in a classical way before we understood it in a quantum-mechanical way. Naturally, the first class of variables chosen to be observables were those suitably describing the familiar, macroscopic world. But now that we have available to us a deeper under- standing of the microphysics, we must recognize that the choice of such observables was contingent upon available methods of measurement. Such concepts as were successfully used for millennia to describe objects at certain scales are, we now know, concepts whose very appearance is owing to ubiquitous quantum interactions. There is simply no need to puzzle any longer about the strange fact that nature seems to present itself more easily in certain ways over others: macroscopic objects commute with their environments largely in terms of position, and thus environments quickly begin to ?monitor? such systems in that basis—the system becomes decohered in position and any further interaction with the system will find it to be in an apparent eigenstate of that same basis. Similar accounts succeed for microscopic systems: electrons commute most readily with typical environments (like atomic nuclei or electromagnetic field modes) in the energy basis, and thus when measured electrons apparently occupy energy eigenstates. Bohr's atomic model of 1913 was considered successful even after the advent of the new quantum theory precisely because measurements always yielded energy eigenstates: the electron was decohered by some environment in the energy basis and phase relations among components of the superposition were suppressed beyond recall.

Let us return for a moment to the question of observables, and on what grounds certain operators have come to be known as observables. The same story applies here: why certain operators lend themselves more readily to physical interpretation over others is a contingent fact based on the nature of environmental interactions, and not due to some hidden law of nature that selects or prefers such operators. As discussed above, the stability of a particular basis with respect to uncontrollable environmental interaction is determined by the dynamics of the specific interaction(s), which leads to the approximate diagonalization of the einselected basis (another way to describe the effects of decoherence). Because certain bases (e.g., macroscopically superposed bases) are extremely unstable under evolution in even weak environments, the probability of observing a system in such bases is infinitesimal.

Hermitian operators are linked to classical variables in quantum theory because their self-adjointness guarantees eigenvalues corresponding to easily interpretable, apparently distinguishable quantities. By associating this class of operators with historically and contingently preferred variables like position, momentum, energy and time we adopted a way of doing quantum mechanics that determined which variables could be interpreted as physical or real or classical, and which could not. To illustrate the contingency of Hermitian operators as linked to classical variable values, note the existence of research programs in quantum mechanics that employ non-Hermitian operators—that is, programs who take as observables certain quantities that do not correspond to the usual variables. Though the experimental nature of this physics is (unsurprisingly) enormously difficult, the existence of such programs demonstrates that entire regimes of quantum phenomena and systems have remained unexplored simply because they were deemed unphysical long ago.

Now that we understand that the choice of operators is in a deep physical sense arbitrary and only limited by obstacles of engineering, entirely new arenas of investigation are open to us. Examples of work with non-Hermitian operators include [19,29,36,39,43]. More poignant for the theories at issue in this paper is an article written recently by Kiefer (whose research we will encounter again in later sections) and Schell [27] in which they provide justification for the use of triads as canonical variables in loop quantum cosmology. In previous work describing such approaches to quantizing gravity, the association between mathematically convenient variables (like triads) and appropriate, measurable, physical variables (like the three-metric) were either ignored or assumed. Kiefer and Schell use decoherence to justify this link between the formalism and the physical facts by pointing out that triads—which play a role in quantum cosmology analogous to that of the three-dimensional metric in geometrodynamics—are chiral creatures able to assume two different orientations, and yet are supposed to give rise to only one (*the* physically realized, if you will) metric (ibid., p. 1).

Mimicking the technique used to resolve the very same paradox of optical isomers mentioned in our treatment of the measurement problem, Kiefer and Schell show that given typical models of loop quantum gravity, fermonic fields are sensitive to triad orientation and are therefore suitable environments for initiating decoherence in triads with respect to that basis. Assuming (trivially) that triads are able to interact or otherwise become entangled with some femionic field, they will deco- here into effective eigenstates of chirality at a rate much greater than the calculated time of purity-recurrence in that basis (cf. [27, p. 8]). Thus the theoretical overdetermination of the 3-metric by triad orientation can be resolved by arguing as follows: triads will quickly become decohered and so effectively interact as either left- handed or right-handed and not a superposition thereof; this effectively pure orientation, no matter which it is, will result in a determinate, physical orientation of the universe. Furthermore, we expect that this orientation of the metric will be globally stable due to the strength and frequency of continual fermonic interactions with triad variables.

Thus it would seem nature is largely impervious to whether we choose Hermitian or non-Hermitian operators to act on quantum systems, how we divide particular subspaces into subsystems, or how we bundle together different degrees of freedom. While it is clear that we must divide the world somehow in order to analyze it, what becomes even more clear through decoherence is the utter disregard the universe has for our choices in this respect. There are no truly “classical” observables, nor ?classical? quantities. There are no mysterious selection rules to be discovered explaining why objects “prefer” certain bases in different energy and size regimes. There is no mystery about the appearance of definite outcomes. For explaining all this quantum dynamics alone are sufficient.

3. The "Insurmountability" Response: Esfeld and Vassallo

Esfeld and Vassallo [13] begin with a presumably non-contentious claim: if quantum gravity is supposed to be the most fundamental physical theory, it must resolve certain issues within non-relativistic quantum theory. In particular, quantum gravity should explain the appearance of classical phenomena like measurement outcomes.

The authors focus on canonical approaches to quantum gravity instead of covariant approaches. In their overview, Esfeld and Vassallo (hereafter "EV") introduce two deep problems which emerge when quantizing systems previously subjected to Dirac constraints. In Sect. 5 below I will suggest that decoherence has something to say about these new problems; here I focus on the paper?s central argument, to wit—the problems inherited from non-relativistic quantum theory force one to adopt some interpretation, but no matter which choice one makes on this front, one will face significant troubles (of different sorts, depending on which interpretation was chosen) when attempting to understand canonical quantum gravity in terms of the adopted interpretation.

EV introduce two issues which "are inherited from the classical regime" (p. 43). The first arises from quantum gravity's infamous problem of time: if we choose as observables for canonical quantum gravity those quantities with (weakly) vanishing Poisson brackets under the appropriate constraints, and if we let our operators be quantities producing states annihilated by those constraints, then we are left with a class of quantities which do not evolve. This problem—how to baptize the appropriate quantities as 'observables' and 'operators' in our new quantum theory—is considered by EV to be directly related to the measurement problem. In light of the fourfold disambiguation of the problem stated above, we might connect EV's observable-naming issue to the problem of preferred bases. Understood as such, one can anticipate my interpretation-free response to the authors.

The second problem has to do with defining measurement consistently within the canonical approach to quantum gravity. One can easily surmise my response to this problem as well. But it will be instructive to first consider in detail how EV set up the measurement problem.

Recall the quote from EV in Sect. 1: the authors do not require the measurement problem of non-relativistic quantum mechanics be solved by quantum gravity, rather that "any approach to quantum gravity that is to be empirically adequate has to take a stance on the measurement problem, the question being how to account for measurement outcomes within a quantum theory..." (Ibid, p. 44). It is unclear what the substantial difference is between solving the measurement problem and merely "taking a stance" on it. Regardless, it should come as no shock that I, too, say the measurement problem needn?t be solved by those working with quantum theories—but of course I say the only stance required is strict adherence to the standard formalism of quantum mechanics.

The authors follow [30] by framing the measurement problem in terms of three inconsistent propositions which come out of quantum mechanics: that the wave function is a complete description of a system ("A1"), that the wave function always evolves in accordance with a linear dynamical equation—the Schrödinger equation ("1B"), and that measurements always (or at least usually) have determinate outcomes ("1C"). Different solutions to the measurement problem are arrived at by adjusting or jettisoning one or more of these propositions in a way that renders the trio consistent [13, p. 44].

But there are problems with this way of framing things. For a start, 1B is false if, as it implies, it considers the evolution of a wave function for a single system. Unitary evolution (and honest-to-goodness obedience of the Schrödinger equation) is destroyed locally when the system of interest interacts and/or becomes entangled to another system, as it will inevitably do. Unitarity is of course preserved at the scale of the new composite system, but not so with respect to the initial system. When one is interested in tracking the dynamical evolution of the initial system independently of any environmental influence the mathematical description quickly becomes non-unitary and (in most cases) frighteningly complicated. There exist mathematical methods for approximately following the evolution of a subsystem—the method of reduced density matrices or the method of Feynman path integrals; these "tricks" allow physicists to bypass the complicated state of affairs continually created by entanglement and describe the measurement statistics of a system effectively independently of any environment.

In H. Dieter Zeh's seminal paper on decoherence, he establishes the pervasiveness of entanglement by noting that the total wave function for a pair of systems will only in rare cases be found with both systems in definite states. He writes [41, p. 73]: "Any sufficiently effective interaction will induce correlations"; since macroscopic systems can effectively interact even at astronomical distances, "the only 'closed system' is the universe as a whole." Thus if one assumes the unitary evolution of the Schrödinger equation to describe sufficiently any microdynamics, one must accept the consequences of that universality: the ubiquity of entanglement, leading to the ubiquity of decoherence. But one must take care when claiming that wave functions evolve linearly—it is in virtue of decoherence with the environment that a given wave function can be successfully treated as locally linear despite inevitable interactions which destroy such linearity.

Another point regarding Maudlin?s statement of the problem is that 1C makes an crucial, unjustified ontological assumption by claiming that measurements "always (or at least usually) have determinate outcomes" (as quoted in [13, p. 44]). All we can really say is that measurements *appear* definite. As testimony to this point one might cite the burgeoning experimental literature confirming measurement of indeterminate outcomes (typically manifest as interference phenomena) for mirco- and mesoscopic systems; some of this work was mentioned in the previous section (also cf. [11]). Maudlin?s 1C therefore should state that measurements *appear* to have definite outcomes in a majority of cases, and in response one either considers the appearance of definiteness the question to be answered, or one considers the appearance of definite- ness to be indicative of a deeper, ontological state of affairs. Stated this way, only the latter reading of 1C necessitates extra-physical interpretation, for if one takes decoherence seriously, this problem—the general problem of outcomes—can be explained away using wholly quantum processes.

EV note that Maudlin's version of the measurement problem doesn't depend on any particular account of measurement. I see this as a virtue: interpretations of quantum mechanics and of the measurement problem need not depend on anything beyond maximally weak definitions of measurement and similarly weak definitions of observation (i.e., as merely one system providing information about another system; there is absolutely no need to call upon consciousness or agents with volition in such con- texts). However, at this point in their analysis EV introduce a variation of Maudlin?s proposition

1C, called 1C*, explicitly addressing the problem of the quantum-to- classical transition without any reference to measurement events or devices [13, p. 45]:

> 1C* The macroscopic systems with which we are familiar—such as, e.g. tables, trees, cats, people, and the like—always (or at least usually) have determinate positions in space, and these systems are composed of microscopic quantum systems.

They continue:

> Consequently, quantum systems, whatever they are, must at least sometimes have positions that are determinate enough so that they can compose macroscopic systems that have determinate positions.

We are well-equipped at this point to see just how decoherence renders this proposition unnecessary and, indeed, ill-posed.[7] Once again: because Hamiltonians describing such macroscopic systems and their environments mutually commute with respect to position, it is as eigenstates within this basis such systems appear. Of course the standard formalism demands that superposed positions still contribute to the total state description; it's just that such highly non-classical states are very effectively hidden.

Without appeal to the insights from decoherence, naturally EV demand resolution to their stated problems. In their Sect. 3 they introduce two "conservative" solutions—so named because such interpretations choose to drop Maudlin?s propositions 1A or 1B in order to preserve 1C/1C*. These are the folks committed to there really being a definite outcome and not just the appearance of one, and thus must add something to the standard formalism to explain classical phenomena. Examples of such interpretations are Bohmian mechanics and collapse theories. I agree entirely with EV in their criticism of Bohm and collapse theories within the specific context of quantum gravity. For instance, Bohmian mechanics is not Lorentz-invariant (which is an obvious setback for relativistic applications of quantum theory);

[7] Independent of decoherence considerations, one might point out that 1C* not only assumes that quantum systems ("whatever they are") are nevertheless distinct entities from the macroscopic systems they comprise—which is false due to widespread entanglement—but this assumption subjects the whole proposition to the fallacy of composition.

like-wise, GRW-type theories rely on a so-called “mass density ontology” which is not Lorentz-invariant. EV mention flash ontologies as a Lorentz-invariant alternate to collapse interpretations; however, flash ontologies do not pick out a unique foliation of space-time and so cannot resolve the measurement problem without assuming frame dependence. And presumably, frame dependence is something one very much wishes to avoid for the sake of consistency with diffeomorphism-invariant theories like general relativity.

The most poignant criticism EV launch against conservative interpretations is their commitment to position in space-time as primitive. This is a contentious commitment from outside the context of canonical quantum gravity, and becomes prodigiously more so within it. With such great costs, and given the benefits of incorporating decoherence into one?s explanations, clinging to an ontologically distinct classical regime by refusing to give up 1C/1C* seems a dark and complicated path. But what about other solutions?

Rejecting Maudlin’s 1C or EV’s 1C* amounts to accepting that the wave function is a complete description and that the Schrödinger equation appropriately describes the unitary evolution of—and I here insert effectively closed—systems, but forsaking definite outcomes. EV invoke decoherence at this juncture, but make a few key mistakes in so doing. First, they bring it up in connection to Everett-style interpretations. If Everettianism is understood as an approach that makes *any additional ontological claim(s) whatsoever* beyond the standard formalism, than decoherence says nothing more for it than for any other interpretation—recall that decoherence just is the standard formalism as applied to environmental interactions. There is nothing in decoherence which extends beyond commitment to the usual equations, and so this association with Everettianism is misleading.

The more important mistake EV make regarding decoherence unfortunately hap- pens to be one of the most common, and it is the claim that decoherence makes quantum correlations disappear. EV say it this way (ibid., p. 50):

> As far as the formalism of quantum mechanics is concerned, decoherence hence means a development of the wave function (or state vector or density matrix) in a high-dimensional mathematical space such that the interference terms between the superposed correlations vanish.

This is false: decoherence does not cause anything to vanish or disappear, but only damps such correlations extremely effectively. To insist that phase relations become *in-principle* instead of *in practice* irretrievably randomized more or less amounts to assuming some sort of collapse; that is, it implies non-unitarity occurs within the system-environment composite. This is emphatically not a part of of the standard formalism, and not true of decoherence processes. Though it is extremely difficult to engineer situations in which one might observe recurrences of coherence (referred to as "periodic coherence revivals" in the literature), it has been accomplished (for example, see [28]). Even were such empirical data unavailable, the re-coherence of interference terms remains a statistical possibility for any finite system-environment composite due to the periodic nature of decoherence master equations.[8]

Immediately after the above statement, EV continue to say that invoking decoherence only raises further questions regarding observers [13, p. 50]:

> Taking simply for granted that such observers [to whom determinate values of dynamical properties appear] somehow emerge out of or supervene on wave functions in a high-dimensional mathematical space evidently does not do the job of a precise physical account.

But no such observers are required, for once a system undergoes decoherence it already has effectively determinate values for eigenvalues in the decohered basis. No observer need exist to whom such approximate eigenvalues become manifest—the dynamics are sufficient for explaining why, *should a further interaction occur*, the system would yield an apparently definite result. Unfortunately, EV's misunderstandings of decoherence and what it entails lead them to believe that the only viable option for those choosing to jettison 1C/1C* is an interpretation wherein decoherence instantiates a branching universe. Luckily a right application of decoherence processes absolves us of making such a commitment. Besides, the extra ontology required for branching-universes introduces a suite of new puzzles, like the following:

> If the idea is that whenever there is decoherence, the whole physical universe develops into many branches, this means that each system in the universe—including its mass, its charge, etc.—is

[8] For more details cf. [37].

> many times copied; but it is unclear how such a physical multiplication of mass and charge could be brought about. [13, p. 51]

EV further mention that it?s unclear how branching is meant to participate in space-time, and it's anyone's guess as to whether individual branches would heed relativistic constraints. Indeed, when decoherence is understood as the process which generates branches for an Everett-type interpretation, this "resolution" of the measurement problem merely begets further problems, like how one arrives at classical observables—the very concern with which EV began.

EV conclude that the quest for closing the explanatory circle—decreed by them a necessary component of any purported fundamental physical theory like quantum gravity—results in a dilemma. On the one hand, "conservative" solutions take position in space-time as primitive; but this is problematic in the context of quantum gravity, present approaches to which make reference to quantized volumes and areas, as well as other exotic entities like triads, holonomies and the like. The role of classical observables like space-time position in such theories is far from obvious. Worse yet, foundational papers in approaches to quantum gravity and cosmology seem to suggest that traditional observables may not even come in to it, in the end. On the other hand, the authors believe the cost of rejecting 1C/1C? is to allow for branching, and with branching come other ontologically dubious propositions. In either case, troubles abound.

However, if one frames the measurement problem with an eye to decoherence and entanglement, and if one understands decoherence properly as well as independently of interpretation, one not only avoids Esfeld and Vassallo's dilemma and has the tools to provide detailed, dynamical, context-sensitive stories regarding the appearance of classicality, but in addition one can remain optimistic at the point EV admit defeat: decoherence can help us make important conceptual advances within quantum gravity and cosmology. More will be said on this last point in the conclusion.

4. The "Surmountable Only Via Interpretation" Response: Okon and Sudarsky

The motivations cited by [34] for adopting a collapse interpretation of quantum mechanics are similar to those found in [13]. All of these authors assume one must first solve the measurement problem before addressing new problems in cosmology and quantum gravity (though Okon and Sudarsky couch the problem in yet another way from Maudlin, Esfeld, Vassallo and myself—more on which anon). All authors also seem committed to the idea that adopting a specific interpretation will help to resolve new issues stemming from relativistic theories. And finally, all four express the conviction that any proposed fundamental theory should be independent of subjective/external notions like measurement, measuring devices, observers and observations.

Okon and Sudarsky (hereafter "OS") regard the measurement problem as having its roots in the necessity of regarding measurement as "a fundamental and unanalyzable term" in order to apply quantum mechanics, and as stemming from the lack of a unified explanation for "the quantum behavior of mircosystems and the absence of superpositions at the macro-level (without ever having to invoke observers or measurements)" [34, p. 116]. Understanding measurement in any particular, not-fully-general manner does not strike me as at all necessary for the application of quantum mechanics, given the lessons of decoherence. One will recall that extremely weak interactions may be considered measurements, as such interactions satisfy all the essential aspects of what is typically associated with measuring events. As to OS's problem with the lack of superpositions, this aspect of the measurement problem has already been discussed.

But OS wish to do more than resolve the measurement problem by tackling three looming problems in cosmology and quantum gravity. In the end, they want to show that "objective collapse theories" (defined in Sect. 2 of their paper) satisfy all of the above—i.e., that collapse models can solve (their construal of) the measurement problem, explain the origin of (asymmetric) cosmic structure, address the problem of time in quantum gravity and resolve the black hole information-loss paradox. Though one might argue that the way in which OS define objective collapse theories introduces as many black boxes as it purports to explain, I will leave analysis of this point to those more familiar with the wider class of collapse theories. I focus instead on their application of the collapse interpretation to the three problems listed above, describing what decoherence reveals in each of those situations.

4.1. Problem #1: The Seeds of Cosmic Structure

Current theories of cosmology typically include the paradigm of an inflationary period—a time during which the very young universe underwent extremely violent and rapid expansion, obliterating any structural features that might have existed prior. What is left, then, is a homogeneous, isotropic, flat universe wherein all fields occupy Bunch-Davies vacuum states. The problem is then explaining the current state of the universe: how did structure at all scales—from macroscopic galaxies and globular clusters to microscopic elements—arise out of a perfectly symmetric, structureless, post-inflation universe? Enter quantum mechanics: in a fundamentally quantum universe there is no true vacuum, as even fields in vacuum states undergo quantum fluctuations. It turns out that these fluctuations, when applied to the inflaton field, are sufficient to give rise to the variety of structure now observable; hence, quantum fluctuations are the seeds of cosmic structure.

This is an exceedingly neat story, but OS are convinced that the deep interpretational problems of quantum mechanics must plague the inflationary paradigm as they do *any* account involving quantum dynamics. This claim is not unanimously shared among cosmologists—something the authors readily point out, but follow by citing several instances confirming their perspective. For instance, they quote from Weinberg's seminal cosmology text: "the field configurations must become locked into one of an ensemble of classical configurations with ensemble averages given by quantum expectation values... It is not apparent just how this happens" (as quoted in [34, p. 122]).[9]

OS drive home the point by stating that several recent works all support their conviction that "something beyond standard physics is required in order to provide a reasonable account for the success of the inflationary predictions regarding the emergence of the seeds of cosmic structure" [34, p. 122]. One

[9] The authors even quote another cosmology textbook [32] as specifically denying decoherence's ability to satisfactorily explain translational invariance. However, the quote used by OS is from the manuscript version of Mukhanov's book, and has been omitted from the published version. One naturally speculates.

notes, however, that of the recent works cited, one was written by Sudarsky while the other was coauthored by him. Taking all this into account, it remains far from obvious at this point whether any extra physics—any interpretation, as it were—will indeed be required to rid the inflationary paradigm of its troubles.

I now demonstrate that no extra interpretation is really required—first qualitatively then again quantitatively. Okon and Sudarsky [34, pp. 122-123] write that the standard approach to the inflationary paradigm can only be considered satisfactory "if it is able to explain what exactly is wrong with the conclusion that given an initially symmetric state, the standard quantum evolution, controlled by a symmetric dynamics, cannot lead to anything but a symmetric state." What is wrong with this conclusion is that it fails to account for inevitable non-local quantum interactions when assuming that the initially symmetric state describes a closed system. Even were the system to begin in a pure state, it would not remain so for long; along comes decoherence, and things are not what they seem.

The authors address this point earlier in the paper when they acknowledge that situations in which symmetrical states evolve into asymmetrical ones is not unheard of in quantum theory. By way of example they appeal to the double-slit experiment, saying that from the perspective of one system, the initial situation is symmetric (in position, with respect to the slits) while the final situation is not (the system has asymmetrically interacted with the screen by hitting a spot either right or left of center). They also recall the high probability of measuring an harmonic oscillator?s position as off-center ("asymmetric") despite its initial ground-state symmetry. What accounts for these instances of symmetry evolving into asymmetry is, by OS's lights, the occurrence of a measurement. "Therefore," they conclude, "it is clear that the type of analysis described above relies implicitly either on the Copenhagen interpretation or on some other operational interpretation of quantum theory where special rules are employed whenever some measurement takes place" (ibid., p. 121).

But quantum dynamics alone explains that such symmetries are not in fact destroyed but only become hidden (such that generic interactions with the decohered system will "measure" or "observe" the system as apparently occupying a definite eigenstate). The only measurement required to explain evolution from symmetry to apparent asymmetry is a (likely arbitrary) interaction with some external degree(s) of freedom. For example, the particle flying through a double-slit apparatus is initially describable in terms of a symmetric superposition of the positions of both slits. Subsequent interaction between the particle and *any other system* will introduce entanglement, and that means the initial particle's state will experience decoherence with respect to one or several degrees of freedom (depending on the nature of the interaction). Hence the particle will *appear*, upon further interactions with, say, a screen, to occupy a single, asymmetric eigenstate even though sans collapse or any other supplemental mechanism we must continue to regard the particle's complete state description as one which still possess the initial symmetry. Note that the specific question of *how* the symmetry effectively breaks—e.g., right or left—is of little consequence; we can explain the appearance of the symmetry breaking generally through direct appeal to the bare formalism.

Turning now to the initially symmetric inflaton field, a quantum description of the field would include superpositions of degrees of freedom of the field induced by quantum fluctuations. These interactions will lead to entanglement, then decoherence, in various bases, resulting in approximate eigenstates. And so one has within the inflaton field candidates for systems behaving effectively asymmetrically that nevertheless preserve quantum expectation values because their asymmetry is only apparent.

Thus far the qualitative explanation. Quantitatively, this work has already been carried out in a suite of papers by Claus Kiefer and collaborators;[10] I will focus on the most recent paper, [25], entitled "Why do cosmological perturbations look classical to us?" After describing the symmetry-to-asymmetry problem for the inflationary paradigm, Kiefer and Polarski proceed in a very similar manner to OS: they apply perturbation methods to a quantized inflaton field (a massless, scalar field) and con- sider the various field modes, suitably quantized. The game is then to show how these theoretically-derived quantum field modes in a Bunch-Davies vacuum state will, with the addition of quantum fluctuations, result in the effectively classical modes mea- sured by the relevant physical observable.[11] In other words, all four authors want to demonstrate how superpositions of, e.g., field amplitudes or their conjugate

[10] For example, [21?24,26].

[11] This observable, α_{lm}, is a complicated entity described briefly in [34, pp. 122?123].

momenta in the inflaton field can remain entirely obedient to symmetrical, unitary evolution while at the same time giving rise to classical modes.

OS believe the only way to explain this is by positing an instantaneous collapse mechanism such that "the part of the state corresponding to the mode $\vec{k}$ undergoes a *sudden jump*" and specific, definite values are "selected randomly from within a Gaussian distribution centered at zero with spread one" (ibid., pp. 124-125). It is unclear what has really been explained by this other than to posit a *Deus ex machina* generating the appropriate results. Meanwhile, Kiefer and Polarski present several arguments to the following ends: (1) decoherence must be included in any dynamical description of the inflaton field, (2) decoherence explains why quantum field modes nevertheless resemble a classical statistical ensemble, and (3) others have only gotten away with ignoring (1)—i.e., achieved acceptable solutions to symmetry breaking without invoking decoherence—by assuming an isolated inflaton field, and this assumption can it turn only be justified in virtue of decoherence.

Regarding (1), Kiefer and Polarski make three important points. The first is that entanglement can occur without disturbing a system, and entanglement is necessary and sufficient for decoherence [25, p. 8]; there is no need for measurements within the inflaton field to introduce these dynamics. Secondly, candidates for fundamental theories all contain a multiplicity of fields, and with them, many opportunities to pro- vide external degrees of freedom which might constitute an environment for quantum fluctuations of the inflaton field. Thirdly, even were we to ignore the influence of other fields, the various modes within the primordial field interact with one another and thus may provide the requisite external degrees of freedom. They write: "Such non-linear interactions concern both the interaction with the modes of the inflaton and the perturbations of the metric (containing, in particular, gravitational waves)" (ibid., p. 7). Were one so bold as to deny the inevitability of decoherence in this context even in view of all these reasons, the authors remind us that since field amplitudes are non-discrete (even if minimal uncertainty) Gaussians, we ought to expect quantum correlations among amplitudes in real, physical space-time.

Regarding (2), I will not reiterate the contents of the suite of papers proving this point (some of which cited above), except to briefly state the following. The evolution of quantum perturbations in a primordial, massless scalar field results in particle pair- production with opposing momenta, which means the system will become increasingly squeezed in the momentum basis. Constriction of momentum states indicates necessarily widening states in the basis of the canonically conjugate position variable—in this context, field amplitude—and it is well known that such states are highly susceptible to environmental interactions (ibid., p. 7 and references there cited). If one considers the quantum fluctuations of the inflaton to be the system of interest, we assume initially symmetric superposed values for the amplitudes of the field. Inevitable interaction with some environment[12] will lead to very rapid, extraordinarily effective damping of interference terms between field amplitudes, leaving a system that will henceforth interact as though it were a classical statistical ensemble of values corresponding to the *de facto* classical observable. Note that nowhere is decoherence taken to explain why the classical observable has the particular value it has, only why it appears to be an eigenstate in a preferred basis (asymmetric) instead of a superposition thereof (symmetric).[13] If one remains unconvinced, there's always the argument from the majority: Kiefer and Polarski note that decoherence has become increasingly connected to symmetry breaking phenomena in the literature, and in contexts other than inflationary cosmology (cf. their p. 9 and sources).

Regarding (3): this is once again merely the point that decoherence closes the explanatory circle, and has been explicated in previous sections.

4.2. Problem #2: Time in Quantum Gravity

This well-known bugbear of canonical approaches to quantum gravity was touched upon above in the discussion of Esfeld and Vassallo's 2013 paper. The diffeomorphism invariance of general relativity, when carried into a quantized theory, results in a quantum state that cannot differentiate between space-times—i.e., the quantum description fails to pick out a unique hyper-

[12]And you can take your pick-the conceptual point remains. Though the particular values one might calculate for parameters like the decoherence timescale will depend, of course, on the nature of the environment one chooses and on the interaction Hamiltonian for that specific coupling with the system.

[13]Kiefer and Polarski do not in this paper justify their assumption that field amplitude will be the dynamically-emerging pointer basis, but instead refer the reader to such proofs in [26] and [24].

surface corresponding to our physical universe.

Of course, one can solve this problem rather trivially by introducing non-unitary quantum dynamics like collapse events. This lets one incorporate non-unitary terms into the system's Hamiltonian, a construction forbidden under usual Dirac constraints. But again, OS's proposal to resolve the problem of time by simply introducing non- unitary terms is motivated by the false impression that the dynamics of the system under consideration is properly unitary—and we know this is hardly ever true. When the assumption of purely unitary evolution is dropped (as one must do to evaluate decoherence processes), one recognizes that introducing extra ontology in the guise of a collapse event isn't necessary: unitarity breaks down already within the standard formalism, but widespread decoherence hides this fact by rendering systems effectively isolated from the start. There is then no reason to posit anything extra.

Of course this argument does not itself resolve the problem of time, but I suspect that canonical quantum gravity approaches will make important headway on the matter when they recognize that the problem is deeply tied to our (by-decoherence-justified) assumptions about the isolation of systems. By way of "gesturing in a promising direction" on this front, consider popular attempts to resolve the problem of time "based on the identification of some variable of the gravity-matter theory to act as a physical clock..." [34, p. 129]. OS disdain such approaches, pointing out that such attempts will in the final analysis rely on the introduction of non-unitary terms just as collapse theories do. That various possible solutions to the problem of time end up taking to heart non-unitary aspects of the theory is not at all surprising if one is convinced that the problem arises in the first place in part because those outlining canonical approaches to quantum gravity have largely only considered "pure"', matterless gravity fields, taking for granted the pedestrian fact that any system short of the universe writ large is in truth interacting and hence not evolving strictly in accordance with Schrödinger's equation. Because a given subsystem of the universe is likely already decohered, one can usually assume unitary evolution with impunity. But the problem of time may be an instance of a normally fecund assumption becoming a significant hindrance.

My point here is demonstrated keenly by the very research group cited by OS as an example of an unsatisfactory resolution to the problem of time:

three recent papers by Gambini, Porto and Pullin approach the problem by letting decoherence pick out a suitably robust variable—that is, eigenstates of the dynamically einselected basis—to function as a physical clock [14-16]. Such an account involves non-unitary terms, but only those terms introduced through decoherence considerations. This fact should not discredit the project (as it seems to do for OS) but instead recommend it: Gambini et al. have demonstrated that interesting work can be done in quantum gravity by invoking the specific dynamics of decoherence, without relying on a particular interpretation of quantum mechanics. In fact, the 2004 papers extend beyond the problem of time in quantum gravity to address the information-loss paradox in terms of decoherence. This brings us to the third and final problem OS wish to resolve by appeal to objective collapse interpretations.

4.3. Problem #3: The Black Hole Information-Loss Paradox

The aspect of this problem emphasized by OS is, once again, the breakdown of unitarity implied by information loss within a black hole. If one is willing to drop unitarity (say, by invoking collapse) then information loss is no longer problematic. What decoherence adds to this situation is subtlety: one need not forsake unitary evolution, only the assumption of unitary evolution for systems partaking in Hawking radiation. This is the tactic described in [14] and [15]: the physical clocks picked out by decoherence are used to calculate the rate of information loss in black hole evaporation. This rate is then compared to rates of real energy loss and found much faster. In other words, the rate of decoherence is so fast in these models that information loss is practically unobservable. If one cannot ever hope to confirm observationally whether or not information has been lost, one might feel a little easier about living with the paradox, as it can never be confirmed as such.

In these calculations, Gambini et al. use an admittedly simple model for the black hole. Nevertheless, their work is an example of precisely the sort of theoretical progress one hopes to see more of: theorizing made possible by incorporating extremely well- confirmed models of decoherence into our calculations, using the lessons of decoherence to adjust our intuitions about interactions at the quantum scale, and opening our eyes to less traditional, more creative realities—and to do so without subjugating cutting-edge research programs to the mire of the interpretation debate.

5. Conclusion

After their concise introduction to canonical quantum gravity, Esfeld and Vassallo outline two significant problems currently barring progress. First is the "baptism problem": if one is handed an already-constrained system and asked to quantize it, there will be no method available for discerning physical from nonphysical degrees of freedom and thus naming the appropriate quantities "observables" for the theory. This is of course a problem already encountered in general relativity—the theory's diffeomorphism invariance leads to obfuscation regarding which variables are physically meaningful or relevant, and which are superfluous. This loss of information about physically relevant degrees of freedom will plague any attempt at a theory of quantum gravity, whether covariant or canonical; it is simply a difference of where in the calculations the problem rears its head.

The second issue named in [13] arises for Hamiltonian (or Dirac-constraint) formulations of general relativity: the Hilbert space of a constrained-then-quantized system is not necessarily identical to—and in fact is typically much higher-dimensional than—that system's physical Hilbert space. It is unclear how to restrict the calculated Hilbert spaces to the desired ones which depict only (physical) solutions for a given system's dynamical equations. EV remark in a footnote that it is this complication in particular that accounts for "why we still do not have a completely worked out canonical theory of QG" (ibid., p. 42 n. 11).

One might readily suggest a new tack regarding these problems in keeping with Kiefer's general research, namely—decoherence processes might provide hints as to baptizing the relevant quantities as observables, and might also help to explain the Hilbert-space mismatch by introducing an effective reduction of the theory-derived Hilbert space to the desired "physical" dimensions. The first suggestion (re the baptism problem) is precisely the sort of issue considered in Kiefer and Schell's article on triads in loop quantum cosmology introduced above: the mathematics provided hints as to convenient candidates for canonical variables in the new theory, and by applying appropriately parameterized decoherence models the authors were able to provide justification for considering triads a physically meaningful variable.

In addition to the problems facing quantum gravity mentioned by Esfeld and Vassallo, [17, p. 279] state what they call the problem of "no outside observer":

> The quantum formalism concerns the interplay between—and requires for its very meaning—two kinds of objects: a quantum system, and a more or less classical apparatus. It is hardly imaginable how one could make any sense out of this formalism for quantum cosmology, for which the system of interest is the whole universe, a closed system if there ever was one.

But what has already been emphatically stressed above is that one can perfectly innocuously assume that arbitrary subsystems are effectively closed, even though they are not, because decoherence is at work between and among the degrees of freedom of whatever we choose to call our system and any or all of the rest. Quantum cosmology can make perfect sense sans interpretation if one allows this little cheat, made possible through decoherence. Chances are good there exist additional problems within quantum gravity and cosmology already articulated or yet to be discovered whose interpretation and resolution will, like those problems addressed in this paper, benefit by summoning the full power of quantum theory. And that means including decoherence.

References

1. Adler, S.L.: Why decoherence has not solved the measurement problem: a response to P.W. Anderson. Stud. Hist. Philos. Mod. Phys. 34B, 135-142 (2003)

2. Albert, D., Loewer, B.: Some alleged solutions to the measurement problem. Synthese 86, 87-98 (1990)

3. Anderson, P.: Reply to Cartwright. Stud. Hist. Philos. Mod. Phys. 32, 499-500 (2001a)

4. Anderson, P.: Science: a 'dappled world' or a 'seamless web'? Stud. Hist. Philos. Mod. Phys. 32, 487-494 (2001b)

5. Arndt, M., Nairz, O., Vos-Andreae, J., Keller, C., van der Zouw, G., Zeilinger, A.: Wave-particle duality of C_{60} molecules. Nature 401, 680-682 (1999)

6. Arndt, M., Nairz, O., Zeilinger, A.: Interferometry with macromolecules: quantum paradigms tested in the mesoscopic world. In: Bertlmann, R., Zeilinger, A. (eds.) Quantum [Un]Speakables: From Bell to Quantum Information, pp. 333-351. Springer, Berlin (2002)

7. Bacciagaluppi, G.: The role of decoherence in quantum mechanics. The Stanford Encyclopedia of Philosophy (Winter 2012 Edition), In: Zalta, E.N. (ed.) (2012). `http://plato.standford.edu/archives/win2012/entries/qm-decoherence/`

8. Bernu, J., Deleglise, S., Sayrin, C., Kuhr, S., Dotsenko, I., Brune, M., Raimond, J.-M., Haroche, S.: Freezing coherent field growth in a cavity by the quantum zeno effect. Phys. Rev. Lett. 101, 180402 (2008)

9. Bitbol, M.: Decoherence and the constitution of objectivity. In: Bitbol, M., Kerszberg, P., Petitot, J. (eds.) Constituting Objectivity: Transcendental Perspectives on Modern Physics, The Western Ontario Series in Philosophy of Science, pp. 347?357. Springer, Berlin (2009)

10. Brezger, B., Hackermüller, L., Uttenthaler, S., Petschinka, J., Arndt, M., Zeilinger, A.: Matter-wave interferometer for large molecules. Phys. Rev. Lett. 88, 100404 (2002)

11. Crull, E.: Exploring philosophical implications of quantum decoherence. Philos. Compass 8 (9), 875-885 (2013)

12. Cucchietti, F., Paz, J., Zurek, W.: Decoherence from spin environments. Phys. Rev. A 72 (5),052113. (2005)

13. Esfeld, M., Vassallo, A.: From quantum gravity to classical phenomena. In: Sauer, T., Wüthrich, A. (eds.) New Vistas on Old Problems. Max Planck Research Library for the History and Development of Knowledge (2013)

14. Gambini, R., Porto, R.A., Pullin, J.: Realistic clocks, universal decoherence and the black hole information paradox. Phys. Rev. Lett. 93, 240401 (2004a)

15. Gambini, R., Porto, R.A., Pullin,J.: A relational solution to the problem of time in quantum mechanics and quantum gravity: a fundamental mechanism for quantum decoherence. New J. Phys. 6, 45 (2004b)

16. Gambini, R., Porto, R.A., Pullin, J.: Fundamental decoherence from quantum gravity: a pedagogical review. Gen. Relat. Gravit. 39(8), 1143-1156 (2007)

17. Goldstein, S., Teufel, S.: Quantum spacetime without observers: Ontological clarity and the conceptual foundations of quantum gravity. In: Callender, C., Huggett, N. (eds.) Physics Meets Philosophy at the Planck Scale: Contemporary Theories in Quantum Gravity, pp. 275-289. Cambridge University Press, Cambridge (2001)

18. Hackermüller, L., Uttenthaler, S., Hornberger, K., Reiger, E., Brezger, B., Zeilinger, A., Arndt, M.: Wave nature of biomolecules and fluorofullerenes. Phys. Rev. Lett. 91, 090408 (2003)

19. Hatano, N.: Non-Hermitian quantum mechanics and localization in physical systems. In: Ono, Y., Fujikawa, K. (eds.) Quantum Coherence and Decoherence, North-Holland Delta Series, pp. 319-322. Elsevier, Amsterdam (1999)

20. Joos, E., Zeh, H.: The emergence of classical properties through interaction with the environment. Zeitschrift für Physik B 59, 223-243 (1985)

21. Kiefer, C.: Decoherence in quantum electrodynamics and quantum cosmology. Phys. Rev. D 46,1658 (1992)

22. Kiefer, C.: Topology, decoherence, and semiclassical gravity. Phys. Rev. D 47, 5414-5421 (1993)

23. Kiefer, C., Lesgourgues, J., Polarski, D., Starobinsky, A.A.: The coherence of primordial fluctuations produced during inflation. Class. Quantum Gravity 15(10), L67 (1998b)

24. Kiefer, C., Lohmar, I., Polarski, D., Starobinsky, A.A.: Pointer states for primordial fluctuations in inflationary cosmology. Class. Quantum Gravity 24(7), 1699 (2007)

25. Kiefer, C., Polarski, D.: Why do cosmological perturbations look classical to us? Adv. Sci. Lett. 2 (2), 164-173 (2009)

26. Kiefer, C., Polarski, D., Starobinsky, A.A.: Quantum-to-classical transition for fluctuations in the early universe. Int. J. Mod. Phys. D 7(3), 455 (1998)

27. Kiefer, C., Schell, C.: Interpretation of the triad orientations in loop quantum cosmology. Class. Quantum Gravity 30(3), 035008 (2013)

28. Kokorowski, D.A., Cronin, A.D., Robers, T.D., Pritchard, D.E.: From single-to multiple-photon decoherence in an atom interferometer. Phys. Rev. Lett. 86(11), 2191-2195 (2001)

29. Longhi, S.: Optical realization of relativistic non-Hermitian quantum mechanics. Phys. Rev. Lett. 105, 013903 (2010)

30. Maudlin, T.: Three measurement problems. Topoi 14, 7-15 (1995)

31. Monroe, C., Meekhof, D., King, B.E., Wineland, D.J.: A 'Schrödinger cat' superposition state of an atom. Science 272, 1131 (1996)

32. Mukhanov, V.F.: Physical Foundations of Cosmology. Cambridge University Press, Cambridge (2005)

33. Myatt, C.J., King, B.E., Turchette, Q., Sackett, C.A., Kielpinski, D., Itano, W.M., Monroe,C., Wineland, D.J.: Decoherence of quantum superpositions through coupling to engineered reservoirs. Nature 403, 269-273 (2000)

34. Okon, E., Sudarsky, D.: Benefits of objective collapse models for cosmology and quantum gravity. Found. Phys. 44(2), 114-143 (2014)

35. Raimond, J.-M., Haroche, S.: Monitoring the decoherence of mesoscopic quantum superpositions in a cavity. Séminar Poincaré 2, 25?64 (2005)

36. Rotter, I.: A non-Hermitian Hamilton operator and the physics of open quantum systems. J. Phys. A: Math. Theor. 42(15), 153001 (2009)

37. Schlosshauer, M.: Decoherence and the Quantum-to-Classical Transition, 2nd edn. Springer, Berlin (2007)

38. Shimony, A.: Search for a worldview which can accommodate our knowledge of microphysics. In: Cushing, J., McMullin, E. (eds.) Philosophical Consequences of Quantum Theory, pp. 25-37. University of Notre Dame Press, Notre Dame (1989)

39. Tolkunov, D., Privman, V.: Short-time decoherence for general system-environment interactions. Phys. Rev. A 69(6), 062309 (2004)

40. Wallace, D.: Quantum Mechanics, ch 1. In: RicklesD. (ed.) The Ashgate Companion to Contemporary Philosophy of Physics, Ashgate, Burlington, VT (2008)

41. Zeh, H.: On the interpretation of measurement in quantum theory. Found. Phys. 1, 69-76 (1970)

42. Zeh, H. Ch 2: Basic concepts and their interpretation. quant-ph/9506020v3(2002)

43. Znojil, M.: Scattering theory using smeared non-Hermitian potentials. Phys. Rev. D 80(4), 045009 (2009)

44. Zurek, W.: Environment-induced superselection rules. Phys. Rev. D 26, 1862-1880 (1982)