1 Introduction

Physicists have searched for a fundamental theory of quantum gravity (QG) for nearly a century. Despite much progress on the theoretical side – including whole avenues of research toward how to develop such a theory (e.g. loop quantum gravity or string theory), little has been claimed on the empirical side. According to standard lore, this is entirely unsurprising given the Planck energy scale compared to the energy scales probed in high-energy particle physics; Zimmermann (2018) for instance pointedly illustrates the remoteness of the Planck energy scale for our usual collider technologies based on acceleration of charged particles in electric and magnetic fields:

> An ultimate limit on electromagnetic acceleration may be set by the Sauter-Schwinger critical field, above which the QED vacuum breaks down. …Assuming these fields, the Planck scale of 10^{28}eV can be reached by a circular or linear collider with a size of about 10^{10}m, or about a tenth of the distance between earth and sun, for either type of collider (!). (pp. 36–37, exclamation mark in original)

An astrophysical benchmark may further help to communicate just how remote the physics in question is from our more familiar empirical world: The phenomenon of Hawking radiation, by whose detection we would like to corroborate the formal apparatus of quantum field theory in curved spacetime (merely *on the way to* a theory of QG apt for the Planck energy scale) is so weak that “Trying to detect astrophysical Hawking radiation in the night’s sky is thus like trying to see the heat from an ice cube against the background of an exploding nuclear bomb” (Thébault 2016, p. 4).

What is the empirically minded QG researcher to do? Despair not being an option, maybe desperation is: One could search for an evidential or confirmatory framework that leaves room for *non*empirical forms of support for developments on the theoretical side of QG research. From this perspective, the framework for nonempirical theory confirmation developed by Dawid (2013) in the context of string theory may be attractive. Alternatively, one could follow the subcommunity of *quantum gravity phenomenologists*: Those empirically minded QG researchers who have not stopped working on finding empirical signatures of QG, despite awareness of the naive estimate of the difficulties as that of, say, Zimmermann already quoted here. This is the tack we intend to take.

One strategy in quantum gravity phenomenology is to look for QG effects within traces of high-energy astro-particle phenomenology in the early universe (famously motivated by using the universe as the "poor man's accelerator"). Another strategy is to systematically search for effects that "cascade" from high energies to low energies, such as in many cases of Lorentz invariance violation (in either the astrophysical-cosmological arena or in more controlled experimental settings). On this strategy, one accepts that the relevant energy scale is the Planck scale, but rejects a tacitly assumed fact of decoupling (Amelino-Camelia [2013] for a review).[1] A third strategy though, which will become our focus here, has only recently become relevant. It begins by noting that the Planck mass, rather than the Planck energy, might better serve as the quantity of interest in probing the quantum nature of gravity. As Christodoulou and Rovelli (2019) write:

> Puzzling is the fact that – unlike Planck length and Planck energy – m_{Planck} falls within a very reachable physical domain: micrograms. It has long been hard to see what sort of quantum gravity effect can happen at the scale of the weight of a human hair. (p. 65)

It has long been hard, but perhaps it will not be so hard any longer, and QG effects might indeed be in reach of tabletop experiments. (And if not literally tabletop, at least not solar-system sized!) Or at least, this is what recent claims amount to, in the emerging experimental research program known as *tabletop quantum gravity*.

It is important to note that thus restricting attention to the weak-field, Newtonian regime involves a significant change in the object of empirical study: One is no longer probing fundamental QG, only the low energy physics that different fundamental theories likely have in common. The question that faces us then is how might we read distinctively quantum traces of gravitational physics in such experiments? Answering this question seems to be key in making sense of the nascent tabletop quantum gravity research program. And in fact, only recently has it become clear that there is significant dissent among those physicists interested in quantum gravity phenomenology over the answer to this question.

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[1] What of the "decoupling theorem" of quantum field theory? This strategy in quantum gravity phenomenology explores QG effects beyond field theory in the relevant sense.

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A new call to interpret hypothesized results in a proposed class of tabletop quantum gravity experiments, which Bose et al. (2017) and Marletto and Vedral (2017b) have each independently noted may soon be viable, brings the question to the fore.

In this new class of *gravitational induced entanglement* (GIE) experiments (sometimes, Bose-Marleto-Vedral experiments; sometimes, gravitationally mediated entanglement experiments – names to be explained in Section 4.2) one employs spatially separated pairs of "gravcats,"[2] or gravitationally coupled Schrödinger cats (macroscopic, uncharged massive bodies) placed in spatial superposition, as the relevant quantum matter probes. Within these experiments, the hypothesized role for the underlying *quantum nature* of Newtonian gravity is to mediate entanglement between the two gravcats in a pair. The proposal, then, is that if such experiments indeed produce the predicted gravcat entanglement, then this outcome would provide a first ever laboratory *witness* of the quantum nature of gravity. And while this achievement would not amount to a direct observation of QG (and especially not to a direct observation that would distinguish between various current approaches to developing a fundamental theory of QG), it would still be an enormous advance. Yet, even the nature of this achievement in terms of a first tabletop quantum gravity witness is questioned by some in the community.

The stage thus set, three philosophers of physics came together, hoping to clear up for themselves a puzzle. How could it be that this specific, newly proposed class of experiments in tabletop quantum gravity could be a locus of dispute when all those involved in the dispute would seem to agree on their expected outcomes? And what do those results have to do with the supposed underlying *quantum* nature of gravity, anyway? This Element is our best attempt to provide a satisfying, unified answer to both of these interrelated questions. It is an answer that the three of us are, finally (after considerable friendly disagreement), content each to call our own.

The result (instead of a series of idiosyncratic articles written by each of us in turn, arguing back and forth through a thicket of distractions) is the following discursive work. It intertwines a review of the relevant

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[2] The name "gravcats" goes back to Anastopoulos and Hu (2015).

physics suitable for philosophers of physics and physicists looking for a sketch of the field, with philosophical analysis giving both philosophers and physicists (whether internal or external to the field) a framework for understanding the conceptual and epistemic issues. We have thus aimed to provide the formal and philosophical background necessary to make everything comprehensible to our intended audience(s).

Following this Introduction, we offer a theoretical prelude (Section 2) on *semiclassical gravity* – a bit of theoretical architecture relevant for research in quantum gravity phenomenology, but whose conceptual status within our current best physics is equivocal. We distinguish three views of semiclassical gravity, including two different ways to deny that semiclassical gravity is itself to be understood as a candidate for future fundamental theory in the discipline, given today's best physics. In parallel with these two denials, we then provide in Section 3 an experimental prelude, noting two experiments in the early history of tabletop quantum gravity that are by now unavoidable in any conversation about quantum matter probes in a Newtonian gravitational context. Crucially, we will explain how these two experiments are importantly distinct from each other – particularly in the kinds of conclusions drawn from their successful execution. This observation occasions our identifying two traditions of experimental testing that will become relevant in our assessment of the GIE experiments, beginning in the section thereafter: On one tradition, the goal is to witness the quantum nature of gravity; on the other, one rather is interested in control of (or access to) the same.

With these preludes in place, we turn then to the GIE experiments, and our analysis spans the remaining four sections before the conclusion. After a preliminary naive rehearsal of the GIE experiments in Section 4, including a discussion of how they indeed would rule out semiclassical gravity as a candidate fundamental theory, we then turn to comment on the central question at stake when the experiments are considered in the tradition of witnessing (Sections 5–6): To what extent may we take the experiments as capable of witnessing a quantum nature of the gravitational state? We ultimately argue that one's answer to this question very much hinges on a choice of modeling "paradigm" (a term we use carefully) for the GIE experiments, even given agreement about fundamental physics. In particular, we develop what we have come to understand are

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the two major such paradigms in play in the relevant literature: what we call the “Newtonian model” paradigm and “tripartite models” paradigm, respectively. Only according to the latter does gravcat entanglement actually witness the quantum nature of gravity. Finally, in Section 7, we shift gears to offer the suggestion that the GIE experiments may at least as well be conceived in terms of their standing in the tradition of controlling and accessing – rather than witnessing – the quantum nature of gravity. And then we conclude.

Taking a step back, our first goal is thus to inspect the claim that a GIE experiment could amount to a tabletop quantum gravity witness, in light of the dissensus found within the physics community. We will find, on disentangling the various threads in the literature, that there is meaningful ambiguity as to whether the predicted outcomes of these experiments, if successful, would indeed provide such a witness. In particular, one’s assessment depends on how one chooses to model the experimental setup, while our current best physics provides justifications for two distinct choices. However, our second goal is to provide a view of the GIE experiments that we believe adequately captures their payoff, as a matter at the frontiers of experimentation in tabletop quantum gravity, and which critically does not depend on choice of paradigm. Our hope in writing this Element is thus also to clarify that the successful completion of a tabletop quantum gravity experiment would be an enormous achievement for the experimental research program, regardless of further disagreements regarding the matter of witnessing.

2 Theoretical Prelude: “Semiclassical Gravity”

The problem of QG is generally understood as a need to unify two elements of our current corpus of fundamental physics: on the one hand, a classical and geometrized theory of gravity, general relativity (GR), which recovers Newtonian gravitation in an appropriate limit, while on the other hand, a quantum theory of (special) relativistic matter, the standard model of particle physics, which recovers nonrelativistic quantum mechanics (NRQM) in a different limit. But matter explicitly appears in GR as classical, contrary to our simultaneous embrace of a quantum field theory (QFT) description of matter, in the form of the standard model of particle physics – hence, the problem.

Of course, when seeking to unify a conflicted corpus, physicists typically proceed by trying to hold onto what are believed to be its crucial insights. In the case of QG, one obvious reconciliatory strategy begins with the observation that perhaps it is no requirement of GR that matter have a *fundamentally* classical nature. Rather, perhaps, at least for the sake of phenomenological modeling, there exists an *effective* classical description of the quantum matter – a classical stress-energy tensor. In the context of QFT, it is natural to associate any such effective classical quantities with expectation values of quantum observables, where (in a Hilbert space representation of the quantum state space) the latter are modeled as operators. Thus, one arrives at the Møller–Rosenfeld equation, dating back to the early 1960s (Møller et al. 1962; Rosenfeld 1963):

$$G_{\mu\nu} = \frac{8\pi G}{c^4}\langle \hat{T}_{\mu\nu}\rangle. \tag{2.1}$$

That is, the Einstein tensor $G_{\mu\nu}$, familiar from GR, couples to the expectation value of (what is now) a *quantum* stress-energy tensor operator, understood to act on any given prepared state of matter. (2.1) thus modifies the Einstein field equation of classical GR, replacing the stress-energy tensor on the right-hand side with its expectation value derived in quantum theory.

It is worth stressing that, despite looking (perhaps) innocuous as a modification to the Einstein field equation from GR, the Møller–Rosenfeld equation is a substantive conceptual departure from the classical equation. In the first place, whereas the left-hand side features a quantity that is meant to be descriptive of a single system, the right-hand side appears to describe a statistical property of a whole ensemble of systems. To see the point, imagine a version of (2.1) in which the right-hand side is an expectation value of a classical quantity, denoted by the same brackets, but no hat: It would describe a system in which each run of an experiment had the same Einstein tensor, determined by the statistical average of the different stress-energy tensors found in each run. In other words, the geometry on any particular run would depend on the stress-energy of all past and future runs, though only those that somehow are determined to be a part of the same ensemble. The acausal structure of this statistical modification of classical GR should make it apparent that

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our physics is simply not like that (deep down)! But the same point would apply to (2.1) itself if we took $\langle \hat{T}_{\mu\nu} \rangle$ as a classical expectation value over an ensemble of runs of a (quantum) experiment.

Of course, in quantum theory, there is a ready and standard reading of "expectation value" applicable to a single system: The sum of eigenvalues weighted by the amplitudes squared of the corresponding terms in the quantum state of that system in an individual run. While the Born Rule entails that the ensemble average will (probably) agree with that value, the quantity itself is well defined in terms of the state of the single system, unlike the classical case. Even so, we will see in Section 3.2 – in the context of the measurement problem – that there can be ambiguities in how we move between the classical reading of the expectation value and the quantum reading in analyses of quantum experiments.

How one constructs a quantum stress-energy tensor operator in QFT is a subtle business. But, once defined, it is indeed an operator that acts on states $|\psi\rangle$ of a material quantum system. As such, the states will obey the Schrödinger equation, with Hamiltonians describing both the dynamics of matter with itself, and with gravity:[3]

$$i\partial_t |\psi\rangle = \hat{H}_{\text{matter+gravity}} |\psi\rangle. \tag{2.2}$$

A system described by Eqs. (2.1–2.2) is often referred to as "semiclassical gravity" (SCG), and the Møller–Rosenfeld equation rechristened the "semiclassical Einstein" equation. But this usage hides an important ambiguity, which can (and does) lead to significant miscommunication. On the one hand, one might take Eqs. (2.1–2.2) as jointly comprising an *approximation* to the dynamics of a full solution to the problem of QG, perhaps along the lines of string theory or loop quantum gravity. On the other hand, they might be proposed as a *full solution* to the problem themselves: that is, where gravity is fundamentally classical, so that the quantum nature of matter entails a semiclassical theory. Let's take these two possibilities (and a third that will arise) in turn.

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[3] Note in interpreting both (2.1) and (2.2) in terms of a common notion of time, in this Element we sweep under the carpet the "problem of time" familiar in QG research, without further comment.

View 1: SCG as a Mean-Field Description in Low-Energy Quantum Gravity The first reading holds that classical GR succeeds under ordinary circumstances because they reside in a regime in which it provides a good approximation to an as-yet unknown fundamental theory of QG (which need not be a final theory of physics). More particularly, models of GR are taken to be "mean-field" solutions of the unknown theory, and quantum perturbations around those solutions are taken to provide an effective field theory (EFT) for the underlying unknown theory: A ubiquitous implementation of this EFT is quantized linearized general relativity.[4] This picture is discussed and its many concrete applications described in, among other places, Burgess (2004) and Wallace (2022), both of who call it "low-energy quantum gravity" (LEQG).[5] Their point (also made by Crowther and Linnemann [2019]) is that such a theory is both empirically successful and constitutes a quantum theory of gravity in any reasonable sense: indeed, in just the sense that we have a quantum theory of the electromagnetic, weak, and strong interactions (viz., a UV-incomplete EFT of some higher energy physics).

In the LEQG framework, SCG amounts to the low-energy limit of the gravitational EFT, when quantum fluctuations in the gravitational field may be ignored. For instance, in the case of quantized linear gravity, Hartle and Horowitz (1981) derive (2.1) as the lowest order quantum matter corrections, in the large N limit of N gravitating quantum systems (a fact to which we will return). But one also expects SCG as a limit on more general grounds than that derivation: Sakharov's "induced gravity" program (Visser 2002) begins with the observation that (2.1) holds in the limit for *any* theory that dynamically couples a Lorentzian metric to a quantum field.

Within the approach described by Burgess and Wallace, SCG then solves the problem of incorporating quantum matter into classical

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[4] In the applications that we consider, linearization occurs in a Minkowski background, so the perturbations can take the form of massless, spin-2, gravitons. See Huggett and Wüthrich (2020, Section 10) for further discussion. Note too that we leave open whether "mean field" is understood more literally or more analogously, depending on the nature of the underlying QG theory and the limit taken within that theory to obtain the EFT.

[5] Note that LEQG does not comprise all that might be termed "low-energy quantum gravity": For instance, perturbative quantum cosmology (or even nonperturbative, yet symmetry-reduced quantum cosmology in general).

dynamical spacetime theory, provided that we restrict attention to an appropriate regime in LEQG.[6] (Indeed, one could take SCG as showing that there was no real "problem" in the first place.) On this picture, given the strength of the gravitational interaction relative to others, one would expect in this regime leading order corrections to the expectation value of stress-energy, for a given state of matter, to come from quantum fluctuations of the matter field itself, and not from gravitational fluctuations (at least away from strong curvature). Then, even though one expects deviations from classical gravity in the long term because of the nonlinear character of (2.1), for short durations of time, it is sufficient to model such a system in terms of accumulating effects of back-reaction by matter corrections on the spacetime curvature, which would otherwise determine the left-hand side of the equation. Such a modeling program is known as "stochastic gravity," and has been successfully developed (see, e.g. Hu and Verdaguer [2008]). As noted by Wallace (2022, p. 39), stochastic gravity may be derived from LEQG, indicating no tension between the present view of SCG and the expectation that stochastic noise due to the quantum nature of matter influences the effective classical description of gravity.

What is crucial to this view is that gravity is understood as fundamentally quantum in nature, and only effectively treated as classical, that is, for the purposes of approximation. This approximation is summarized by the dictates of SCG, interpreted in terms of LEQG, perhaps improved with corrections from stochastic gravity.

View 2: SCG as a Candidate Fundamental Theory of Gravity

According to a second possible view, (2.1–2.2) constitutes an *alternative* to string theory, loop quantum gravity, and so on for a fundamental theory in its own right, not approximation. This view is not taken as a serious possibility by the QG community, yet there have long been efforts to rule it out definitively: Huggett and Callender (2001) critique theoretical arguments and also the experimental approach that we discuss

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[6] And, we would add, an appropriate regime within the underlying QG theory that recovers LEQG in a suitable limit. This regime may not be quite identical to that picked out in LEQG – indeed, one would hope not, if QG research is to resolve outstanding problems in LEQG like the cosmological constant problem and black hole evaporation.

later. Likely, that this view is not taken seriously in part reflects the fact that it is beset by mathematical difficulties. Namely, it is not clear that any spacetime and quantum field could simultaneously satisfy both equations. One can define a QFT satisfying (2.2) in a given curved background spacetime (and perhaps solve the equations), but once one has, likely its stress-energy will not satisfy (2.1) in the background. One might then introduce a new background for which (2.1) is satisfied, but now (2.2) will likely not hold, and a new QFT must be defined. And so on. As we say, perhaps the equations can be solved simultaneously, perhaps the process described even converges on a solution (and perhaps merely approximately so), but it is far from sure.

Still, physicists are no strangers to mathematical difficulties in the course of theoretical research. Arguably, what is more *conceptually* troubling is the disunity involved in accepting that some parts of the world are classical while others are quantum even at the most fundamental level. Moreover, there are difficulties contemplating what even some basic physical models of such a theory would look like, beyond maybe the vacuum sector. For these reasons, in conjunction with the mathematical difficulties, it is perhaps not surprising that this view is given little credence by physicists. At the same time, it is considered worth eliminating as a live possibility.

Moreover, it is not even clear that Eqs. (2.1–2.2) adequately capture what is claimed: a meshing together of classical gravity and quantum matter, as we currently understand them. Recall in view 1 that corrections due to matter fluctuations, as studied in stochastic gravity, plausibly are relevant to the gravitational properties of a classically gravitating quantum system, so that it is a virtue of that view that LEQG recovers familiar stochastic gravity techniques. As just stated, view 2 categorically denies the role for any such corrections. The result is that there is rampant loss of physical information in coupling the material quantum system to gravity: After all, expectation values are insensitive to all higher-order correlators in the QFT.

View 3: SCG as A Mean-Field Description of X Now, taking SCG as an approximation does not in itself commit one to the view that SCG is an approximation to *LEQG*; perhaps the low-energy approximation