Chapter XII

The Cogency of Semi-Classical Gravity

BOOTCAMP READERS—different from the previous prefatory, preparatory remarks:

1. **I have introduced the idea of "Opportunities" to qualify individual topics, rather than nominating and treating them all as problems, because no one likes an unmitigated Debbie Downer; since there is, as of yet, only one and a half Opportunities, one may feel with some justice that the mitigation is etiolate at best; so, if I'm lucky, at least a few people like an etiolately mitigated Debbie Downer. . .**

2. **this is a *rough* draft, even rougher than the previous; the argumentative structure is, again, not as clean and well formed as I want; there are infelicities; parts of the chapter are given only as an outline of the claims and arguments, sometimes a detailed and thorough outline, and, when so, even though not fully fleshed out, they are, I think, nonetheless both legible and intelligible; some of the sections are outlined not even so skimpily, but exist only as skeletal lists of work and problems to discuss, which I mark with '[*** SKELETAL ***]', and you should feel *very* free to ignore those—as always, *caveat lector***

3. **there are many exegetical passages, not part of the body of the chapter, but rather notes to myself or to the reader indicating where there are questions or problems I need to think more about, where I am dissatisfied with what is there, where I indicate further things I need**

or want to discuss, where I am reminding myself to do something, *etc.*; they are syntactically marked by being surrounded by asterisks enclosed in braces, *e.g.*, ‘[*** foo ***]’; the ones beginning “BOOTCAMP READER” I want you to read; the others you may read or not as you will; some of them I think you should, some are unnecessary for following the career of the argument, and a few will be unintelligible to you; *lector ad libitum*

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In QFT-CST, one attempts to analyze the behavior of quantum fields propagating on a fixed background relativistic spacetime geometry. One wants the quantum field to be responsive to the spacetime geometry in an appropriate sense without worrying about the effect that its stress-energy content has on the spacetime geometry itself, in something like the way that one expects a massive, free test particle to traverse a timelike geodesic while ignoring its contribution to the stress-energy tensor on the righthand side of the EFE—in the argot, one "ignores back-reaction". In SCG, one wants to complete the circle by taking account of the effect of the quantum field's stress-energy content on the ambient curvature while yet keeping the spacetime geometry classical, by incorporating that stress-energy content in a (possibly modified form of the) righthand side of the EFE. In the standard formulation, one first constructs an operator that, in an appropriate sense, represents the stress-energy content of the quantum field in the same way as the ordinary stress-energy tensor does so for classical matter in general relativity, *viz.*, as a tensorial object with two covariant indices, symmetric and covariantly divergence-free; one then takes its expectation value with respect to the state of the quantum field, and sets the classical Einstein tensor (the lefthand side of the normal EFE) equal to that, symbolically,

$$G_{ab} = 8\pi\langle \hat{T}_{ab}\rangle$$

known as the 'semi-classical Einstein field equation' ('SCEFE').

At this point, even if one is a connoisseur of both GR and QFT, one should have no idea what those symbols mean. What is $\hat{T}_{ab}$? It is ticklish enough to make sense of such things—generally quadratic in distributional (!) operator-valued fields—already in special relativity, with all its symmetries. Even if we were to have some grip on it, how are we to calculate its expectation value in a curved spacetime, where we do not have available all the tools we normally use for such things in Minkowski spacetime? And so on.

In so far as one can make sense of those symbols, one expects that such a framework would find its most natural application in the treatment of problems in which, in some sense or other, the curvature of spacetime is well above the Planck length, in so far as there are some theoretical grounds for suspecting that in this regime one can safely ignore any quantum properties of the spacetime geometry itself.[1] (Hence, the framework is often called 'the semi-classical approximation'.) In this vein, its most popular and seemingly successful applications have been to problems involving particle creation in the early universe and in the vicinity of black holes where we expect the classical spacetime geometry to remain well defined, and to the larger field of gravitational thermodynamics those applications have in turn spawned.

In spite of their perceived successes, however, QFT-CST in particular and SCG more generally have many deep problems of a conceptual, physical and technical nature, most of which have not been explored with any systemacity or even to any real depth, either by physicists or philosophers. Perhaps the most overt and pressing problem is that of epistemic warrant: in the face of a complete lack of empirical support, how can we have any confidence in these frameworks that are theoretical contrivances in which we attempt to combine two theories, manifestly in tension if not outright contradiction, in novel ways and then to apply the chimæra to systems we have no proof for the existence of, in regimes we have never probed? In §VI.7, I remarked that one can try to redescribe the current epistemic situation with regard to QFT-CST and SCG more charitably. We are taking well understood, empirically well entrenched principles in each framework (GR and QFT) and in fact combining them in principled ways, relying, *e.g.*, on empirically entrenched postulates such as the equivalence principle, all so as to avoid the manifest tensions and possible outright contradictions between the two frameworks more generally. Thus, we can have some confidence that the new framework gets something right when applied to the new regimes we have some reason to believe it may appropriately treat. In this chapter I shall argue that this is *too* charitable—many of those manifest tensions and possible contradictions *do* make themselves felt on closer examination, in potentially insalubrious ways.

QFT-CST and SCG present us, moreover, with novel conceptual, physical and technical problems *sui generis*, at least as important as those arising explicitly from the clash between GR and QFT, and in many ways of possibly deeper philosophical and physical interest, tantalizing us with the idea that they may be indicating the presence of pathways to a deeper understanding of the world than our current physics can afford us.

XII.1 The Problem of Different Formulations

In light of the notorious difficulty of constructing a theory that wholly incorporates and subsumes quantum mechanics and general relativity—a theory of quantum gravity—it may come as a surprise to learn that there is a consistent, rigorous theory of quantum fields posed on the background of a classical curved relativistic spacetime, *viz.*, algebraic quantum field theory on curved spacetime

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[1] I myself am skeptical of many of the arguments proposed in favor of the idea that the Planck scale has a privileged physical place in our theorizing. I discuss this in §XII.12 below.

(Wald 1994; Fewster and Verch 2015; Hollands and Wald 2015)—'AQFT-CST' for short, or even just 'AQFT' when there can be no ambiguity with regard to its simpler form on Minkowski spacetime. While there have been attempts to extend this framework to include the essential feature of SCG, *viz.*, the SCEFE, they have been, by and large, not entirely successful (as I discuss below).

There are several other approaches to QFT-CST in particular and SCG more generally, some as rigourous as AQFT, some less so but often more easily and fruitfully applied in modeling (more or less) concrete systems, performing calculations and making predictions.[2] These include:

1. perturbative extensions of AQFT (Rejzner 2016)—space constraints do not permit discussion of them, because they give no more insight into our problems than the simpler standard AQFT framework, nor raise new problems of their own[3]
2. axiomatic formulations (Hollands and Wald 2010; Fredenhagen and Rejzner 2016)—space constraints permit only limited discussion of them, because they are not useful for constructing non-trivial models and doing calculations, and, even though—or, better, because—this book is partly philosophical in a way that does not fetishize ontology, I care about the down and dirty details of models and calculations;
3. S-matrix formulations, (in)famously used in the original derivation of Hawking radiation in Hawking (1975)—space constraints permit only limited discussion, as these are rarely if ever used in anger these days;
4. traditional canonical quantization (DeWitt 1975; Parker and Toms 2009)—space constraints do not permit discussion of it, because I find the next more perspicuous and conceptually clearer, and closely related enough to raise essentially all the problems this does;
5. canonical quantization based on a Lagrangian formulation (Jacobson 2003);
6. thorough-going path integral formulations (Kleinert 2009)—space constraints do not permit discussion, because they are messy, do not give useful insight on our problems, and the perturbative truncation analysis of a flat-footed Lagrangian path-integral approach to QG at low energies (see below) is more physically and philosophically interesting;
7. a wide and not clearly demarcated variety of mish-mashes of holographic constructions and some of the approaches listed above (Penington 2019; Akers, Engelhardt, and Harlow 2020; Bousso and Tomašević 2020; Ishibashi and Maeda 2021);
8. a generic effective field-theoretic formulation constructed from a perturbative truncation analysis of a flat-footed Lagrangian path-integral approach to QG at low energies (Burgess 2004)—following Wallace (2022), I will refer to this framework as 'low-energy QG';
9. various low energy, effective constructions based on particular approaches to QG, such as LQG (Rovelli 1996), string theory (Strominger and Vafa 1996), group field theory (Oriti, Pranzetti,

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[2] See §v.9.3 for a brief review of the history of QFT-CST, and §v.9.4 for one of SCG.

[3] One might try to argue that such frameworks may pose novel philosophical problems regarding the nature of approximations in physical theories, but in fact I think all the interesting problems regarding that issue arise already in AQFT itself or in low-energy quantum gravity (see below). I discuss in §xii.16 below the philosophical problems concerning approximations by and of these frameworks.

and Sindoni 2016), causal set theory [*** references ***], and so on—space constraints do not permit discussion of these in this chapter; I discuss some of them on an *ad hoc* basis in various places, *e.g.*, some those just cited in several sections of ch. vii on the possible interpretations of black hole entropy;

10. find some place appropriate to discuss Witten (2021)

11. There are several other, more niche formulations that I do not have space to treat here. One I find of particular interest, *prima facie* worth philosophical attention, is [*** "Entropic Dynamics: Reconstructing Quantum Field Theory in Curved Space-time", Selman Ipek, Mohammad Abedi, Ariel Caticha, Classical and Quantum Gravity, 36(2019, 20):205013, arXiv:1803.07493 [gr-qc], 10.1088/1361-6382/ab436c; "The Entropic Dynamics of Quantum Scalar Fields Coupled to Gravity", Selman Ipek, Ariel Caticha, Symmetry 2020, 12(8):1324, arXiv:2006.05036 [gr-qc], 10.3390/sym12081324 ***]. Another such is the "locally covariant quantum field theory" approach of Brunetti, Fredenhagen, and Verch (2003), based on a category-theoretic generalization of the Haag-Kastler axiomatic-algebraic framework (Haag and Kastler 1964). Another such are those based on the Hamilton-Jacobi framework [*** references ***]. There are also restricted formulations that depend essentially on the nice properties of special spacetimes, such as a semi-classical Friedmann equation proposed in the context of FLRW spacetimes [*** "Semiclassical and Quantum Polymer Effects in the Flat Isotropic Universe", Gabriele Barca, Paolo Di Antonio, Giovanni Montani, Alberto Patti, Phys. Rev. D 99, 123509 (2019, 12, 15 June), arXiv:1902.02128 [gr-qc], 10.1103/PhysRevD.99.123509 ***]. Neither will I here be able to consider such work.

The Problem of Different Formulations

> What are the virtues and demerits of each formulation on its own? How do they relate to each other, and in what sense, if at all, may they be understood as fitting together or complementing each other?

To begin with, quantum field theory on curved spacetime and SCG, in any of their guises, differ from standard quantum field theory (set on the flat Minkowski spacetime of special relativity) in one profound respect, that difference ramifying into every part of the theory: a generic relativistic spacetime has no group of symmetries comparable to the Poincaré Group for special relativity. There is correspondingly no distinguished vacuum state and no natural notion of a particle. This means, for instance, that one cannot employ many familiar and useful techniques across most approaches, and one must take care in the use of most of the others.

1. AQFT (a synopsis of the framework is given in §§v.7–v.8):[4]
    a. recall the Hadamard condition: the singularity structure of the two-point functions of a state of a quantum field recapitulates that of states of quantum fields on Minkowski spacetime in the limit as the two points approach each other (Kay 1988; Radzikowski 1996); motivated, in part, by the equivalence principle; for various reasons, primarily that it allows one in a principled way to calculate an expectation value for a stress-energy

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[4] See §v.9.3 for a brief review of the history of AQFT-CST.

tensor operator, it is often required that a state be Hadamard in order to be physically reasonable (Wald 1994; Hollands and Wald 2002, 2005; Ruetsche 2011, ch. 10);

b. determination of the state in such a way as to respect the background spacetime geometry: if Hadamard, can prove uniqueness but not in general existence (Kay and Wald 1991); when exists, generally not Hadamard (Fewster 2018)

c. In general, the stress-energy tensor operator cannot be identified in the algebra of observables in the purely formal and abstract way the algebra is standardly introduced, nor even can it be proved that an appropriate operator exists in the algebra whose physical significance is that of one representing the stress-energy content of the quantum field at issue. Indeed, the most compelling ways of introducing the stress-energy tensor operator into AQFT, such as Hollands and Wald (2005), do not even make use of the machinery of the algebra of observables, relying rather on perturbative methods based on an associated Lagrangian formulation of the QFT. One must, therefore, go strictly beyond the ambit of the framework in order to achieve a formulation of full SCG; how much further one must go, and whether there are constraints on such extensions that single out a uniquely privileged one in a principled way, are open questions.

d. Such calculations of the stress-energy tensor operator, nonetheless, can sometimes be performed in more or less principled and (often) unambiguous ways (Wald 1994; Hollands and Wald 2002, 2005). For the purposes of formulating the SCEFE, however, the calculation of its expectation value and subsequent introduction of that quantity to attempt to formulate the SCEFE itself, depend on *ad hoc*, often unsatisfying, moves. I postpone detailed discussion of the technical and conceptual problems associated with calculating the stress-energy tensor operator itself as well as its expectation value until §XII.3 below.

e. there has been very little philosophical examination of QFT-CST, and that has been almost wholly restricted to AQFT; discuss:
   i. Arageorgis (1995)
   ii. Arageorgis, Earman, and Ruetsche (2002)
   iii. Butterfield (2007, §3)
   iv. Ruetsche (2011, ch. 10, §2; ch. 11, §2)

   there has been none of SCG, except by the author in a number of talks given at summer schools, conferences and colloquiums over the last several years [*** include in the biblio links to online videos of the lectures? ***]

f. the primary philosophical challenges AQFT faces:
   i. some are shared by standard AQFT in Minkowski spacetime [*** Wallace versus Fraser ***], in particular the complaint that it is difficult to use to model "real" systems and calculate experimental outcomes; this is, however, mitigated by the fact that, in the context of QFT-CST, one *can* in fact use it to model and make predictions about non-trivial physical phenomena, *viz.*, Hawking radiation (Fredenhagen and Haag 1990; Janssen and Verch 2022); and all problems are exacerbated

    by the fact that we do not have the results of decades of sustained and deep examination by philosophers to draw upon as we do for AQFT on Minkowski spacetime (Ruetsche 2011, and references therein)

    ii. given the generic lack of ambient symmetries such as the Poincaré group, it becomes difficult, if not impossible, to identify the physical interpretation of an arbitrary element of the algebra of observables associated with any given spacetime region—how can we attribute empirical content to the elements of the algebra? how can we determine what the physically significant quantities are? and even if we do determine them on the basis of extrinsic principles, there is no guarantee that there will exist an element in the algebra suitable for supporting an interpretation as a representation of any given quantity; a poignant example is provided by Klein-Gordon, *e.g.*, for which the stress-energy tensor provably has no corresponding self-adjoint operator in the Weyl algebra constructed over the symplectic vector space of classical solutions (Wald 1994, ch. 4, §5)

2. canonical quantization based on a Lagrangian (a synopsis of the framework is given in §§v.7–v.8):

    a. How does the sober, rigorous and precise Apollonian convocation of classical Lorentzian geometry and the exuberantly inexact and informal Dionysian fandango of standard quantum field theory come into mutually fruitful contact, so as to give the joy of material content to the former and the restrained discipline of consistent structure to the latter?

    b. Einstein famously complained of his own field equations (the EFE) that, while the lefthand side had the purity of marble, the righthand side, the classical stress-energy tensor, was a contraption of wood, spit and rubber bands [*** that's the gist, anyway; track down the quote—ask Dennis, the Einsteinsmeister ***], primarily because it was a classical representation of classical matter and not quantum, as even he believed matter to be at bottom; one wonders what he would have made of the grotesquerie that is the expectation value of the stress-energy tensor operator in the Lagrangian-canonical formulation, quantum though it be

    c. the primary philosophical challenges the Lagrangian-canonical formulation faces, but also a virtue:

        i. some challenges are shared by standard by standard Lagrangian formulations of QFT on Minkowski spacetime [*** Fraser versus Wallace ***], in particular, the lack of rigor, precision and, often, even clarity in the conceptual and mathematical machinery; these problems are compounded in the context of CST, given that we do not have the conceptual and mathematical simplicity and tractability of Minkowski spacetime to rely on, and we do not have the decades of sustained and wildly successful use by physicists to draw upon as fodder for philosophical examination as we do for standard Lagrangian formulations on Minkowski spacetime

        ii. a virtue, for my preferred pragmatic point of view: there is rarely doubt about how to identify and construct physically significant observables, and so how to render empirical content to the formalism

3. low-energy QG (a synopsis of the framework is given in §§V.7–V.8):
    a. why I don't like the linearized perturbative graviton picture, "effective field theory of first-order QG":
        i. David Wallace (conversation in Tel Aviv, at conference "Many Worlds Interpretation", late Oct 2022) emphasized to me that there is no empirical evidence for relevance of general-relativistic global structure in real, empirical physics; I don't agree, but it is an issue that certainly needs serious consideration and measured response. Wallace: can't all the questions one may want to pose about the relations among different levels of structure (topological, differential, causal, projective, conformal, affine, metric) be obviated simply by relying on the linearized graviton picture in low-energy quantum gravity, which (as I acknowledged and even pointed out to him to substantiate his point) is what is actually used in a lot of the work we were discussing (GIE, *viz.*, gravity-induced entanglement, and table-top QG experiments, (Bose et al. 2017; Marletto and Vedral 2017; Christodoulou and Rovelli 2019), and how to understand the idea of superposed classical null-cone structures) to derive their results
        ii. my reply is two-fold: it is extremely difficult at best to see how to pose question about different levels of spacetime structure in the graviton picture, especially given that it is formulated on a fixed background spacetime structure (linearized graviton picture likely not fruitful for foundational work); and it *a fortiori* can't handle questions of *quasi-local* structure, which even David must admit, by his own criterion, can have empirical support accrue to it, especially the results about quasi-local structure that depend on non-trivial, subtle relations among structures "at different levels", as I spell out in Curiel (2021), *e.g.*, the subtle interplay among topology, conformal structure and affine structure exploited by Hawking (1972) to prove that a stationary, asymptotically flat black hole is topologically $\mathbb{S}^2$; but it is exactly results of that kind that we need in SCG in order to get BHT off the ground
        iii. Also, to recur to a *Leitmotif*: this picture gets the epistemic order wrong. To demand that the graviton picture must work—indeed, that, as Wallace would have it (again, conversation in Tel Aviv, at conference "Many Worlds Interpretation", late Oct 2022), we have more evidence for that then for the use of GR on cosmological scales, because we have always used QFTs thus (*cf.* Feynman's argument at the 1957 Chapel Hill conference), may be the same kind of mistake as Kelvin's (and those of his ilk) when he demanded a mechanical model of the electromagnetic field, on pain of rejecting Maxwell theory entirely, because that is how substantial physical systems had always worked before. In the face of radically new phenomena, of new kinds of stuff obeying laws and principles radically different from any of those known before, however, one should not be blithe in assuming that they are amenable to appropriate and adequate treatment by the same framework as had worked in all previous cases.

iv. Those who champion low-energy QG, I think, turn the epistemic situation as it actually stands on its head. We have perturbative derivations of the SCEFE that look "standard" (Burgess 2004), *i.e.*, formally the same as we use in other cases of interest that also result in the coupling of a classical quantity to an expectation value, *e.g.*, as in standard calculations in the Standard Model, and in EFTs in condensed matter physics. The point, however, is that in those standard cases, we have independent *empirical* evidence that the perturbative methods work (are adequate), and thus that the approximative scheme is appropriate. I understand what those calculations in the standard cases mean physically because of independent evidence we have that the underlying theories we construct EFTs from are themselves appropriate and adequate, and most especially from the sources of that evidence and how it forms part of the infrastructure of the epistemic content of theories about the relevant physical systems in general: because we have in hand many and variegated successful empirical applications of the theory (testing, confirmatory, exploratory and engineering), and we have used them as the fruitful basis for further theoretical investigations that have themselves been successful in the same ways.

v. We do not have this for semi-classical gravity, so any phenomena derived using the scheme cannot be used as evidence for the propriety of the scheme, much less for anything else (*e.g.*, evidence for the thermodynamic character of black holes based on Hawking radiation). We need to verify independently—and empirically—the propriety and adequacy of the semi-classical approximation *before* we can use its deliverances as evidence for anything else, at least, evidence in the most full-blooded sense—at that means, in large part, acquiring compelling empirical evidence for the existence of the phenomena that the semi-classical approximation characterizes and predicts, in conformity with those characterizations and predictions. When we do want to use it as evidence for something else, therefore, we must keep in mind that the evidential warrant it can bestow, being of only a purely theoretical, is etiolate at best. We do not have the understanding of and the confidence in underlying theories that show what these approximations, calculations, truncations mean and why they are justified, and so why we should expect them to work.

xii.2 The Problem of the State

[*** SKELETAL, IGNORE ***]

1. AQFT
   a. we want Hadamard: why? what is physical significance?
   b. determination of the state in such a way as to respect the background spacetime geometry: if Hadamard, can prove uniqueness but not in general existence (Kay and Wald 1991); when exists, generally not Hadamard (Fewster 2018)
2. see discussions in `hrad-unruh-equiv-princ.tex` and `fixing-vac-st-reality-unruh-quanta.tex`

XII.3 The Problem of the Stress-Energy Tensor Operator

> This is a victory declaration in the theory of a quantized field propagating in a given curved background space-time. We now have an unambiguous, internally consistent quantum theory of such a system, in which any physical quantity can in principle be calculated. Whether this kind of model is relevant to the real world is a separate question.
>
> "Two-Point Functions and Renormalized Observables"
> S. A. Fulling (1983)

The classical stress-energy tensor in general relativity represents that property of all matter that directly couples with spacetime geometry. When one moves to quantum field theory on curved spacetime, the standard constructions one uses in the classical case, and in the case of quantum field theory on flat spacetime, are no longer available, and it becomes difficult to see how to construct a meaningful quantum operator to represent stress-energy at all. What, then, is the stress-energy tensor operator mathematically, what is its physical significance, and what does its expectation value mean?

Because of the lack of unambiguous, rigorous definition of a localized operator corresponding to the classical stress-energy tensor, and the lack of symmetries like the Poincaré Group in special relativity, standard techniques for calculating expectation values fail. In standard quantum field theory, moreover, the standard energetic quantities with well understood physical significance are the scalar energy density and the 4-momentum constructed by choosing a local standard of time-translation.

The Problem of the Stress-Energy Operator

> What is the stress-energy operator mathematically, what is its physical significance, and what does its expectation value mean, and how does one choose among the many different ways proposed to define and renormalize it, given that they may disagree with each other?

The basics:

1. many different ways to define it
2. not all rigorous or even just tolerably clear
3. not all available in generic spacetimes
4. many give different values from each other

In somewhat more detail:

1. In general there is no preferred prescription for defining a unique stress-energy tensor, a particularly acute problem for polynomial interactions Hollands and Wald 2005.
2. In ordinary quantum field theory, one deals with vacuum energy by absorbing it into other terms, effectively stipulating that only energy differences matter, not absolute energy. In the semi-classical Einstein field equation, however, it is the *absolute* value of stress-energy

that matters. Geometry/spatiotemporal structure is sensitive to the absolute value of stress-energy, so differences in renormalization schemes cannot be ignored.

3. a sampling of techniques currently on offer for defining the operator and calculating expectation values (Parker and Toms 2009):
    a. point-splitting regularization (Hadamard and not)
    b. proper-time regularization
    c. dimensional regularization
    d. zeta-function regularization
    e. adiabatic regularization
    f. trace-anomaly cancellation
    g. Gaussian approximation of the propagator
4. Bob W., in conversation (at Peyresq Physics Workshop, June 2022), claimed that Hadamard point-splitting uniquely satisfies a list of desirable physically significant criteria, so this is really not a problem (at least, ignoring some ambiguities in the point-splitting construction): point-splitting always does the job, and does it in the way we want
5. BUT: can those ambiguities be ignored?
    a. more precisely, assume some set of reasonable conditions for a stress-energy tensor operator to satisfy, *e.g.*, the Wald axioms (Wald 1977, 1994) (locally covariant, symmetric, covariantly divergence-free, . . .); consider, as the simplest example, free Klein-Gordon, and carry out the standard point-splitting construction (Wald 1978)
    b. then $\hat{T}_{ab}$ is determined up to a finite renormalization degree of freedom, which can always be written as covariantly divergence-free curvature terms having the right dimension
    c. in physically simple situations (asymptotic boundary conditions, symmetries, . . .), there may be enough information to fix the ambiguity in a privileged way; generally, there will not be
    d. I think Bob's attitude is something like the following (but I need to talk with him more before putting words in his mouth)—in any event, the following is not a contemptible attitude to take towards this issue, whether it is Bob's or not: in all the physical situations we want to use the framework of SCG to treat, to wit, slowly radiating black holes ("stationary") and cosmological spacetimes, we do in fact have enough physical information about general properties of the spacetime to fix the renormalization ambiguities in a privileged way; this is all only an effective, approximative scheme anyway, so any problems it may face outside the limited domain of circumstances we want to bring it to bear on are nugatory
    e. that may be all right FAPP, but one may well want something more for foundational work (NOT, recall, ontology): one wants to know, among other things, the regime of

applicability of the renormalization scheme and the reasons *why* it breaks down or is not applicable to particular kinds of physical situations, if SCG and BHT are going to be capable of giving us clues about deeper, underlying physics

6. another problem: the fact that, generically in black hole spacetimes, $\langle \hat{T}_{ab} \rangle$ diverges near the event horizon for some observers (*e.g.*, the static ones in Schwarzschild spacetime) but not others (*e.g.*, inertial in Schwarzschild spacetime) shows that it is difficult to understand it as having intrinsic physical significance—this becomes particularly clear when one recalls that one can make the acceleration of static observers in Schwarzschild spacetime as small as one likes even right next to the horizon—as close to inertial motion as can be—if one makes the black hole big enough, and yet $\langle \hat{T}_{ab} \rangle$ still diverges

XII.4 The Problem of Divergences and Instabilities

Perhaps the most important use of the SCEFE in black hole thermodynamics is to justify the heuristically motivated idea of black hole evaporation. There are few known exact solutions for even the most trivial cases, and none known for the case of most interest, an evaporating black hole. For the most part, we have only approximate solutions and general properties of classes of solutions in particular physical regimes extrapolated from numerics and from stability analyses. Our confidence in even those general properties, however, and consequently our trust in the known approximate solutions, is not epistemically secure, because of possibly countervailing features of generic solutions—particular kinds of instabilities and divergences in closely related regimes—that we *are* more confident in.

First, do exact solutions exist in cases of physical interest?

1. cosmological spacetimes (Pinamonti and Siemssen 2015; Meda, Pinamonti, and Siemssen 2021)
2. static spacetimes (Sanders 2022)
3. some recent progress for solutions sourced by conformally coupled massless fields for gravitational collapse and black hole evaporation (Meda et al. 2021)

Discuss:

1. the classic on divergences and pathologies: DeWitt (1975)
2. a later classic: Flanagan and Wald (1996)
3. possible problems peculiar to wished-for black hole solutions, in particular tension between emission of Hawking radiation and formation of an event horizon:
   a. Physical black holes in semiclassical gravity, Sebastian Murk, Daniel R. Terno, arXiv:2110.12761 [gr-qc]
   b. Semiclassical black holes and horizon singularities, Pravin K. Dahal, Sebastian Murk and Daniel R. Terno, AVS Quantum Sci. 4(1), 015606 (2022); https://doi.org/10.1116/5.0073598

The main physical and conceptual problem that these pathologies give rise to: how to have confidence that we are extracting what, if anything, and only what, is of physical significance from solutions? And, again—as always—the lack of empirical contact makes this problem all the more poignant and pressing.

XII.5 The Problem of Semi-Classical Coupling

The Problem of Semi-Classical Coupling

> What justification, if any, can there be for the form of the semi-classical Einstein field equation? Why should classical geometry couple to the expectation value of the stress-energy tensor operator in the semi-classical approximation?

Expectation values standardly represent averages of possible experimental outcomes; does the semi-classical Einstein field equation assume that classical geometry effectively acts as a "continual measurement probe" of the quantum field? The standard understanding of the expectation value of an operator in quantum theory seems in no straightforward way to support a cogent interpretation of the semi-classical Einstein field equation.

One way to approach this problem—or, perhaps more accurately, to avoid it—which I have not seen developed in the literature: is it the case, *e.g.*, that the stress-energy tensor of the classical Maxwell field is the expectation value of the stress-energy tensor operator defined by the underlying quantum electrodynamics field? If so, that would be *prima facie* reason to believe that this form of coupling is correct for semi-classical gravity. Moretti (2003) provides some evidence for this.

On the problem of treating semi-classical gravity as an effective field theory, and so trying to characterize its breakdown scales: usually, regime of applicability of an effective field theory is circumscribed by energy scales, or spatial and temporal scales; in this case, however, one of the most important, if not the most important, circumscription is imposed by the coherence of the state of the quantum field, *viz.*, that its variance not be too large, something like "expectation value is sharply peaked around most probable states". (Note, however, that the same issue arises for other EFTs that couple classical quantities to expectation values as well.) Thermal states, however, in the energy eigenbasis, are *not* coherent in the appropriate sense. How, then, can we trust derivations of Hawking radiation?

We have no evidence for how quantum field theory "couples" to classical geometry. It is a postulate—albeit, one with some seeming—that it is by way of the expectation value of the stress-energy tensor. (This seems to depend on the idea that "gravity" effectively acts as a continuous "statistical sequence of measurements" of the quantum field theory—although compare to other cases of "quantum to classical coupling" in which the interaction is mediated by an expectation value, and arises from direct calculation of a perturbative series truncated at first order.) Derivations of Hawking radiation in general, and the arguments of, *e.g.*, Unruh and Schützhold (2005) in particular, are convincing only in so far as one accepts this postulate, *i.e.*, only in so far as one has faith in semi-classical gravity. I discuss this further in §XII.13 below.

Now, look at:

1. Jacobson (2016) may provide a more interesting, albeit no more epistemically secure, route to the SCEFE, from thermodynamics of spacetime

2. derivations of SCEFE from thermodynamics of spacetime in AdS/CFT: Lashkari, McDermott, Van Raamsdonk, Faulkner, Guica, Hartman, Myers, Swingle, *et al.*

3. from an open discussion by participants at the 1957 Chapel Hill Conference on Gravity on the topic "The Necessity of Gravitational Quantization" (DeWitt and Rickles 2011, ch. 23, pp. 249–250,):

   > BELINFANTE insisted that the Coulomb field is quantized through the ψ-field. He then repeated DeWitt's argument that it is not logical to allow an "expectation value" to serve as the source of the gravitational field. There are two quantities which are involved in the description of any quantized physical system. One of them gives information about the general dynamical behavior of the system, and is represented by a certain operator (or operators). The other gives information about our knowledge of the system; it is the state vector. Only by combining the two can one make predictions. One should remember, however, that the state vector can undergo a sudden change if one makes an experiment on the system. The laws of nature therefore unfold continuously only as long as the observer does not bring extra knowledge of his own into the picture. This dual aspect applies to the stress tensor as well as to everything else. The stress tensor is an operator which satisfies certain differential equations, and therefore changes continuously. It has, however, an expectation value which can execute wild jumps depending on our knowledge of the number and behavior of mass particles in a certain vicinity – if this expectation value were used as the source of the gravitational field then the gravitational field itself – at least the static part of it – would execute similar wild jumps. One can avoid this subjective behavior on the part of the gravitational field only by letting it too become a continuously changing operator, that is, by quantizing it. These conclusions apply at least to the static part of the gravitational field, and it is hard to see how the situation can be much different for the transverse part of the field, which describes gravitational radiation.

   Discuss further DeWitt's argument, in ch. 22 of the same, "The Possibility of Gravitational Quantization"

4. Tipler, F. J. (1986). Interpreting the wave function of the universe. Physics Reports 137(4, May), 231–275, 10.1016/0370-1573(86)90011-6: criticizes singularity resolution criteria in QG based on finiteness of expectation value, because Tipler argues we should always think of expectation value at bottom as average of repeated measurements

5. what is problematic about deriving the SCEFE using low-energy quantum gravity? The linearized graviton QFT does not make sense off-shell. More precisely (at least a little):

   a. If you expand the Einstein-Hilbert action in a metric peturbation h_{ab}, the piece that is linear in h_{ab} takes the form $h^{ab}\, G_{ab}$ where G_{ab} is the Einstein tensor of the unperturbed metric.

   b. If you keep this leading term, then the h_{ab} variations require you to expand around an on-shell solution. But if you throw this term out and just look at the $O(h^2)$ piece around a non-solution, then the gauge-symmetries don't close properly.

c. You can say, well of course it is possible to take Einstein gravity off-shell because you can always add a matter source. This is, morally, true but I think that the details do matter and are highly non-trivial, as any such matter source will necessarily increase the number of dynamical degrees of freedom beyond the two of vacuum general relativity.

d. In particular, note that it is not consistent to simply specify a T_{ab} as a source in the abstract, as this will not generally be diffeomorphism-invariant, in the sense that the varying the Einstein-Hilbert action will respect diffeomorphism freedom. One would need some information about how the T_{ab} of matter behaves with respect to the metric perturbations in order to ensure diffeomorphism invariance.

another way to see the ground of the problem: one needs a fixed background structure to define particles of any kind in GR (a privileged timelike direction), so gravitons cannot cogently define anything but perturbations off a fixed background spacetime geometry

XII.6 The Problem of Conformal Freedom

Recall that the Weyl tensor, that part of the Riemann curvature tensor encoding the conformal structure of spacetime, is not dependent on the value of the stress-energy tensor at any given point (Malament 2012). Because the semi-classical Einstein field equation equates the Einstein tensor to the *expectation value* of the stress-energy tensor of the quantum fields on spacetime, how can one guarantee that the conformal structure of spacetime appropriately respects the structure of correlations, and other distinctive quantum features, of those fields?

The Problem of Conformal Freedom

> How can the semi-classical Einstein field equation appropriately constrain the conformal structure of the geometry?
>
> Is the metric curvature of the classical geometry consistently and cogently defined, given that the Ricci curvature is determined by quantum effects but the Weyl curvature (*i.e.*, the conformal structure) is determined as in the purely classical case?

1. gravitational systems are non-linear (they source themselves); why expect them to evince linear superposition themselves or to support linear superposition in other systems?

2. for our purposes here, the issue is: if we're dealing with a quantum-matter system that is reasonably localized spatially, then it is reasonably spread out in momentum space

3. the Weyl tensor is sensitive to gradients in the stress-energy tensor, and that means *4-dimensional* gradient, the covariant derivative, which picks up change in timelike directions, which in this case includes contributions from momentum spread in the matter

4. so the geometry should have quantum spread, quantum fuzziness, already, if the matter does

5. Bernard Kay (in conversation, 22 July 2022, at MCMP conference "Global Structure in SCG") told me that Duff (1981) points out a similar problem (I haven't read the paper yet,

the following is a description provided by Bernard): for the Lagrangian of a scalar-field ϕ in the Einstein-Hilbert action, one can always transform the field in such a way that the metric picks up what is effectively a conformal factor of the form e^{ϕ}; thus the expectation value of the stress-energy tensor operator cannot guarantee "the right fit" between the conformal structure and Ricci curvature

xii.7 The Problem of Energy Condition Violations

[*** SKELETAL, IGNORE ***]

The Problem of Energy Condition Violations

Given that quantum fields on curved spacetime generically violate all the known energy conditions, and Hawking radiation in particular always does so, and that they are the essential ingredient in almost all of the most important and deepest theorems in classical general relativity—about singularities, black holes, asymptotic structure, the initial-value problem, superluminal propagation, and so on—what happens to the results of those theorems in the semi-classical approximation?

Very few people discuss this explicitly. These do:

1. Flanagan and Wald (1996)
2. "Gravitational vacuum polarization. I. Energy conditions in the Hartle-Hawking vacuum" Matt Visser, Phys. Rev. D 54, 5103, Vol. 54, Iss. 8, 15 October 1996, doi:10.1103/PhysRevD.54.5103; "Gravitational vacuum polarization II: Energy conditions in the Boulware vacuum", Matt Visser, arXiv:gr-qc/9604008, Phys.Rev.D 54(8, Oct):5116–5122,1996, doi:10.1103/PhysRevD.54.5116
3. Semiclassical black holes and horizon singularities, Pravin K. Dahal, Sebastian Murk and Daniel R. Terno, AVS Quantum Sci. 4(1), 015606 (2022); https://doi.org/10.1116/5.0073598
4. Curiel (2017)

xii.8 The Problem of Causal Structure (Cogency)

The Problem of Causal Structure (Cogency)

Many pictures of Hawking radiation have it effectively "traveling from within the event horizon to outside" (*e.g.*, tunneling); recent calculations of the Page Curve for evaporating black holes relying on the so-called island mechanism imply extreme non-locality for modes of Hawking radiation, with modes inside the black hole effectively being identified with asymptotically distant late-time modes outside. How, then, is the causal structure of the classical geometry constituting the semi-classical spacetime to be understood?

That many pictures of Hawking radiation have it effectively "traveling from within the event horizon to outside" (*e.g.*, tunneling) calls into question the idea that semi-classical gravity relies on a purely classical spacetime geometry, for the event horizon is an entity characterized solely by causal structure, but "processes traversing it from within to without" contravene that causal structure. This is also related to the problem of conformal freedom: if "derivatives of the causal structure (Weyl tensor)" are related to the degrees of freedom of derivatives of quantum fluctuations, even if only in the form of expectation values, this may also introduce some quantum uncertainty into the null cone structure.

Note that observations and arguments like those of Geroch (2011) can't satisfactorily settle the matter, for the event horizon of a black hole is special in ways that the null hypersurfaces of generic null cones are not, as event horizons are supposed to demarcate the boundary of what can causally effect other stuff asymptotically far away, and it cannot do that if stuff that is possibly causally efficacious leakes through.

Hawking radiation "leaking/tunneling" through horizon: violates definition of event horizon? What is causal structure of evaporating black hole spacetime when back reaction is taken into account?

1. Causality Constraints on Gravitational Effective Field Theories, Claudia de Rham, Andrew J. Tolley, Jun Zhang, arXiv:2112.05054 [gr-qc]

XII.9 The Opportunity of Matter versus Spacetime Geometry

> Now when the appearance of one thing is strictly connected with the appearance of another, so that the amount which exists of the one thing depends on and can be calculated from the amount of the other which has disappeared, we conclude that the one has been formed at the expense of the other, and that they are both forms of the same thing.
>
> – James Clerk Maxwell (1891)
> *Theory of Heat* (ch. IV, p. 93)

Gibbons (1979, p. 639):

> In classical general relativity this equivalence [between mass and energy] means gravitational potential energy contributes to the masses of bodies but nevertheless matter and gravitational energy are not completely interchangeable. In the classical theory matter cannot be completely converted into gravitational energy (Hawking, 1970). In the quantum theory however this process is possible – a sufficiently strong or rapidly varying gravitational field can create pairs of particles. This means that the notion of a vacuum or no-particle state in a curved spacetime is inherently ambiguous.

('(Hawking 1970)' refers to Hawking (1970).) This seems to have first been realized as a possibility by Schrödinger (1939).

In semi-classical gravity, Hawking radiation (and particle production more generally) is "curvature turning into matter" in this sense: it's an interaction between "geometrical degrees of freedom" and "material degrees of freedom" such that the former excite the latter in a way that lead to more of the latter and less of the former, in a sense one can make precise (energy/mass content, various measures of curvature).

The converse is true as well. Matter can be converted into curvature. One way to characterize matter in general relativity: local degrees of freedom characterized by invariance under symmetry groups beyond diffeomorphisms (irreducible representations of the inhomogeneous Lorentz group, *e.g.*, for quantum matter); when "matter turns into curvature by collapsing into a singularity", for instance, then those degrees of freedom vanish, as made precise by the No Hair theorems.

From standard high-energy particle physics, we are used to matter converting into other forms of matter, where the primary (but not only) governing factor is generally the energy available to transform particles of given masses into those of others. As Heisenberg (1989, p. 73) plaintively remarks,

> [I]t obviously no longer makes sense to speak of a splitting of the original particle. The concept of 'division' had come, by experiment, to lose its meaning. . . . [N]o unambiguous answer could be given any longer to the question about what these [newly discovered] particles consisted of, since this question no longer has a rational meaning. A proton, for example, could be made up of neutron and pion, or Λ-hyperon and kaon, or out of two nucleons and an anti-nucleon; it would be simplest of all to say that a proton just consists of continuous matter, and all these statements are equally correct or equally false. The difference between elementary and composite particles has thus basically disappeared.

I believe we face a similar situation with regard to matter and spacetime curvature in SCG: with the interconvertibility of the two, it may make most sense to say, following the epigraph from Maxwell to this section, that they are but different forms of the same thing. What is odd about this case—odder than the situation in particle physics, and odder than the rejection of traditional ideas of relationalism and substantivalism—is that in these transformative processes, one form of substance (the term I shall use for whatever it is that matter and spacetime curvature are different forms of), *viz.*, matter, possesses localized stress-energy, whereas the other, spacetime curvature, does not (Curiel 2019).

This is redolent of a thermodynamical process, in which some localized energetic quantities, say, the integral of the energy density of an electromagnetic field, is transformed into non-local heat in a ponderable, thermoconducive body, with a concomitant increase in entropy. This suggests by analogy that the transformation of matter into spacetime curvature should always, or almost always except perhaps when there are confounding factors, be accompanied by an increase in entropy. And this seems indubitably right: the Bekenstein entropy of a given amount of mass-energy is, quite generally, tens of orders of magnitude greater than the entropy of the matter possessing that mass-energy. So perhaps this unification, as it were, of matter and spacetime curvature may provide an avenue of approach in the attempt to understand the relations among gravity, QGT and thermodynamics at the heart of BHT and SCG.

Now, discuss:

1. Wheeler (1962) and Kiefer (2009)
2. Do black holes count as matter or geometry? When in equilibrium, they have many properties

of equilibrated ponderable matter: mass-energy, temperature, entropy, angular momentum, electric charge; they can have all of these properties (except for electric charge) when the spacetime is vacuum in the classical sense ($T_{ab} = 0$), having no ordinary ponderable matter, quantum or otherwise.

3. perhaps one way to get a grip on the issue, based on Jacobson, Senovilla, and Speranza (2018): G_{ab} is geometry, in so far as it measures the area deficit of small spatial spheres compared to their counterparts in flat spacetime (note that this gives a more elegant and illuminating interpretation of G_{ab} as geometry than I had previously thought available; it is, however, an ambiguous one, in so far as the geometrical effect is one that is not associated with non-zero values of G_{ab} alone, *contra*, *e.g.*, the interpretation of the Riemann tensor as measuring geodesic deviation); one might therefore have expected no such area deficit in non-trivial vacuum spacetimes, but there still is one, arising from "2nd-order" non-linear effects, "gravitational energy", measured by functions of the Weyl and Bel-Robinson tensor (albeit in complicated ways); so geometry is the area deficit, and one way it can arise is gravity; if this is correct, then perhaps the proper comparison is "matter versus gravity", not "matter versus geometry", or, maybe even better, both comparisons need to be considered, and treated on their own

4. To operationalize curvature and metric structure using matter is common-place and familiar (rods and clocks *à la* Einstein; particles and light rays *à la* Weyl; gravitational gradiometers for components of ${R^a}_{bcd}$; and so on). It is perhaps not so wdely realized that one can operationalize matter using the metric and curvature, which is one very fruitful way to understand part of what Jacobson (2016) is up to, even though he does not explicitly remark on it himself.

It is an opportunity because it opens possibility for exploration of new possible avenues of attack on traditional problems about the nature of matter and the nature of spacetime and their inter-relations, perhaps leading to a reinvigoration of the moribund substantivalism versus relationalism debate (Brown 2005; Pooley 2013; Curiel 2018). [*** see also Butterfield and Gomes, "Functionalism as a Species of Reduction" ***]. Although something like this line of attack has a long and venerable history: the precursor of geometrodynamics (modeling matter as configurations of "empty spacetime") in Newton's arguments against Descartes (the mobile region of impenetrability) in Newton (unpublished).

Discuss:

1. If Unruh radiation is related to the excitation of "spatiotemporal" or "geometrical" micro-degrees of freedom, and it occurs in flat spacetime with a quantum field in its own vacuum state, then this suggests that the distinction between "matter" and "geometry/gravity" is breaking down severely already there; if Minkowski spacetime is the "vacuum state of maximal symmetry" of quantum gravity, then acceleration is the equivalent of exciting some of its modes (by the equivalence principle), and this shows up by Einstein coupling as energy pumped ito the modes of the quantum field—so the Unruh effect *depends on* the equivalence principle; it does not violate it.

Indeed, the classical versions of the equivalence principle already suggests the blurring of the line between matter and geometry/gravity:

   a. gravitational mass is equal to inertial mass—the property that determines geometry is the property that responds to geometry (is "guided by" it in motion); but geometry "can source itself", as in non-trivial vacuum spacetimes, and geometry can be "guided by itself" as well, as in the characteristics of gravitational radiation following null geodesics, so there is no difference between gravity and matter at this level at all

   b. that there can be an "effective gravitational field" even when there is no matter present and a system is "only accelerating" suggests, again, that the difference between matter and geometry/gravity is to some degree—not "conventional", but, perhaps better, only effectively determined (in the sense of effective field theory)

2. Think about the differences in the respective symmetries:

   a. spacetime symmetries, as applied to physical systems, often (probably not always—think of the elements of the Poincaré group not connected to the identity, the weird null 4-screw composed with a translation, something like that) represent "the ways that the experiences of different observers of the same system relate to each other"; in other words, this kind of interpretation of the symmetry depends on the fact that the differences in spatiotemporal location between the observers are relevant to the measurement outcomes only in so far as they can be accounted for by the application of the relevant symmetries

   b. this is not the sort of thing that happens in quantum field theory; presumably all observers partake of every type of quantum field, and they are all (excluding pathological degeneracies) in different states of those fields, even if ever so slightly, so what is relevant to determining the reliability of measurement—the inter-translatability between the results of different observers—is that the "sameness of coupling" between observer and system ought not depend on the fine details of the state each is in, at least to the degree required by the level of approximation at issue

   c. there is no comparable notion of "sameness of coupling" when making measurements of spatial length

   d. distinction between "free" and "interacting" quantum field theories as classified by whether the gauge freedom is global (the irreducible representation that identifies the kind of field/particle at issue) or local (turning on "local interactions" between "particles"); it is non-trivial to assume the existence and, more, the physical significance of "free" theories in the sense of linear quantum field theories with global gauge symmetry—it is a *contingent* fact about the world that any physical system can be well represented/approximated by such a thing

   e. it is interesting to note, in this regard, that if one restricts oneself to diffeomorphisms that are connected to the identity, then "free" theory (special relativity, without gravity) becomes "interacting theory" (general relativity, gravity is turned on) by thinking of a diffeomorphism as a localization of applied Lorentz transformations (in special relativity, a Lorentz transformation, the gauge, is global; in general relativity, a diffeomorphism connected to the identity, the gauge, can be represented as a field of smoothly varying Lorentz transformations applied pointwise)

f. the equivalence principle seems to depend on a distinction between matter and gravity that may not even hold at the level of quantum gravity: "non-gravitational" systems behave the same in gravity versus in constant acceleration; all "non-gravitational mass-energy" has the same motion in a gravitational field? is this true? does the equivalence principle distinguish matter from gravity, or at least depend on a prior distinction having been made? can gravitational phenomena (gravitational radiation, *e.g.*) fall under the purview of the equivalence principle? what about linearized gravity waves against a static background? does that satisfy the equivalence principle?

3. So does the idea of trying to quantize "vacuum" spacetimes even make sense?

4. one reason, perhaps, why so many philosophers, and physicists as well, are reluctant to condone the conceptual independence, and the physical possibility, of "free" fields, not associated with any source charges: because one can never have source charges without accompanying fields. The delicate sensibilities of philosophers and physicists cannot abide the indecorous asymmetry that would represent, especially when it comes to matters of "causality".

5. One can always "redefine" a stress-energy tensor by adding to it a constant times the metric, and then re-write the Einstein field equation using the redefined stress-energy tensor minus the constant times the metric? Thus, given the Einstein field equation for $(\mathcal{M}, g_{ab})$,

$$G_{ab} = 8\pi T_{ab}$$

one can define $T'_{ab} = T_{ab} + \Lambda g_{ab}$ for some constant Λ, so that

$$G_{ab} = 8\pi T'_{ab} - \Lambda g_{ab}$$

6. All of this, I believe, emphasizes and makes more poignant the lessons I try to draw in Curiel (2021), to wit, that traditional and popular debates about the metaphysical, physical and conceptual character of different types of structure in spacetime theories such as general relativity tend to focus on the differential manifold and the metric. There are, however, many different types of structure making up the formalism of such theories, each playing its own peculiar role in making it possible for the integrated whole to be interpreted as a "spacetime". Those roles, moreover, are not independent of each other: each places non-trivial constraints on the other, and that in a number of ways. The resulting pattern has more the texture of a web of interlacing, mutually ramifying structures than the clearly stratified and ordered stack of independent layers they are usually depicted as (when attended to at all). I canvass a number of theorems, constructions and cases exemplifying this glorious mess. They show clearly that traditional and popular debates such as substantivalism versus relationalism and the dynamical versus the geometrical views are, in the context of general relativity, badly misconceived at best and irremediably incoherent at worst. The results,

facts and observations I discuss reveal new, albeit related, questions, new problems, that deserve their own analysis, investigation and exploration—questions and problems natural to general relativity, arising from its intrinsic formal and conceptual structures, in a way that the standard debates are not, having themselves been imposed by historical contingency or imported from the contexts of other theories where they perhaps had more cogency.

xii.10 The Problem of Probabilities

[*** SKELETAL, IGNORE ***]

How to calculate probabilities for event outcomes in SCG? What ought one even try to assign probabilities to?

1. "Probability distributions for quantum stress tensors in four dimensions", Christopher J. Fewster, L. H. Ford, Thomas A. Roman, Phys. Rev. D 85, 125038 (2012, 12, 15 June), arXiv:1204.3570 [quant-ph], 10.1103/PhysRevD.85.125038: this is how hairy the problems are even *in Minkowski spacetime!*

2. Hollands and Wald (2005, pp. 80*ff.*) Fewster (2020) and Fewster and Verch (2020)

xii.11 The Measurement Problem

[*** SKELETAL, IGNORE ***]

The Measurement Problem

How does the Measurement Problem in standard quantum theory require modification for its formulation in this context, and how may that affect a possible resolution?

1. discuss: entanglement is not necessarily inconsistent with, or even in tension with, general relativity, in so far as one understands it "phenomenologically", as encoding certain patterns of distal correlations; it is really superposition and Heisenberg uncertainty that are the problems, both intimately tied up with the Measurement Problem

2. "Black Holes, Information Loss and the Measurement Problem" Elias Okon and Daniel Sudarsky, Foundations of Physics, 47(2017, 1, January):120–131, arXiv:1607.01255 [gr-qc], 10.1007/s10701-016-0048-1

3. The geometry of spacetime from quantum measurements, T. Rick Perche, Eduardo Martín-Martínez, hrefhttps://arxiv.org/abs/2111.12724 arXiv:2111.12724 [quant-ph]

4. Fewster (2020) and Fewster and Verch (2020)

5. "Tests of Quantum Gravity near Measurement Events", Adrian Kent, Phys. Rev. D 103, 064038 (2021, 6, 15 March), arXiv:2010.11811 [gr-qc], 10.1103/PhysRevD.103.064038

xii.12 The Breakdown Problem (or, The Problem of the Regime of Propriety)

[*** SKELETAL, IGNORE ***]

The Breakdown Problem

How, when, where, why does SCG break down? What kinds of evidence can we have for this?

1. variation of $\hat{T}_{ab}$ can't be too big compared to expectation value: blowing up the Earth
2. Kay-Time problem (§x.8), Brownian Kicks (§x.9)
3. Quantum Singularities, Raphael Bousso, Arvin Shahbazi-Moghaddam, arXiv:2206.07001 [hep-th]
4. Indirect Evidence for Quantum Gravity, Don N. Page and C. D. Geilker, Phys. Rev. Lett. 47, 979–982, number 14, 5 October 1981, 10.1103/PhysRevLett.47.979
5. Quantum Signatures of Black Hole Mass Superpositions, Joshua Foo, Cemile Senem Arabaci, Magdalena Zych, and Robert B. Mann, Phys. Rev. Lett. 129(18), 181301, 28 October 2022, 10.1103/PhysRevLett.129.181301
6. specialness of Planck scale:
    a. see `whats-planck-scale-for.tex`
    b. terrible arguments about how measurements below Planck scale result in black holes, or other such nonsense, always based on the idea there is only way to measure things; great counter-example: Jonathan W. Richardson, Ohkyung Kwon, H. Richard Gustafson, Craig Hogan, Brittany L. Kamai, Lee P. McCuller, Stephan S. Meyer, Chris Stoughton, Raymond E. Tomlin, Rainer Weiss, Interferometric Constraints on Spacelike Coherent Rotational Fluctuations, Report number FERMILAB-PUB-20-558-E, arXiv:2012.06939 [gr-qc]:

    > Precision measurements are reported of the cross-spectrum of rotationally-induced differential position displacements in a pair of colocated 39 m long, high power Michelson interferometers. One arm of each interferometer is bent 90° near its midpoint to obtain sensitivity to rotations about an axis normal to the plane of the instrument. The instrument achieves quantum-limited sensing of spatially-correlated signals in a broad frequency band extending beyond the 3.9 MHz inverse light travel time of the apparatus. For stationary signals with bandwidth $\Delta f > 10\text{kHz}$, the sensitivity to rotation-induced strain h of classical or exotic origin surpasses $\text{CSD}_{\delta h} < t_P/2$, where $t_P = 5.39 \times 10^{44}\text{s}$ is the Planck time. This measurement is used to constrain a semiclassical model of

nonlocally coherent rotational degrees of freedom of spacetime, which have been conjectured to emerge in holographic quantum geometry but are not present in a classical metric.

   c. what is justification for assuming that QT is valid all the way down to the Planck scale? why is it always GR that must give way to QT?

7. "Tests of Quantum Gravity near Measurement Events", Adrian Kent, Phys. Rev. D 103, 064038 (2021, 6, 15 March), arXiv:2010.11811 [gr-qc], 10.1103/PhysRevD.103.064038

XII.13 The Problem of Epistemic Order

Fifteen jugglers
Fifteen jugglers
Five believers
Five believers
All dressed like men
Tell yo' mama not to worry because
They're just my friends

The issue isn't so much that we don't know what it means for "gravity to act as a continual measurement of the stress-energy of quantum fields" in the appropriate approximative regime (as the form of the SCEFE naively suggests), but rather that those who champion the SCEFE turn the epistemic situation as it actually stands on its head. We have perturbative derivations of the SCEFE (using the saddle-point approximation in path-integral formulation of perturbative quantum gravity and cutting off at first order) that look "standard", *i.e.*, formally the same as we use in other cases of interest that also result in the coupling of a classical quantity to an expectation value, *e.g.*, as in standard calculations in the Standard Model, and in EFTs in condensed matter physics. The point, however, is that in those standard cases, we have independent *empirical* evidence that the perturbative methods work (are adequate), and thus that the approximative scheme is appropriate. I understand what those calculations in the standard cases mean physically because of independent evidence I have that the underlying theories we construct EFTs from are themselves appropriate and adequate, and most especially from the sources of that evidence and how it forms part of the infrastructure of the epistemic content of theories about the relevant physical systems in general: because we have in hand many and variegated successful empirical applications of the theory (testing, confirmatory, exploratory and engineering), and we have used them as the fruitful basis for further theoretical investigations that have themselves been successful in the same ways.

[*** rewrite this paragraph, make the flow more logical, the discussion of underlying theories shouldn't go at the end ***] We do not have this for semi-classical gravity, so any phenomena derived using the scheme cannot be used as evidence for the propriety of the scheme, much less for anything else (*e.g.*, evidence for the thermodynamic character of black holes based on Hawking radiation). We need to verify independently—and empirically—the propriety and adequacy of the semi-classical approximation *before* we can use its deliverances as evidence for anything else, at least, evidence in the most full-blooded sense—at that means, in large part, acquiring compelling empirical evidence for the existence of the phenomena that the semi-classical approximation characterizes and predicts, in conformity with those characterizations and predictions. When we do want to use it as evidence for something else, therefore, we must keep in mind that the evidential warrant it can bestow, being of only a purely theoretical, is etiolate at best. We do not have the understanding of and the confidence in underlying theories that show what these approximations, calculations, truncations mean and why they are justified, and so why we should expect them to work.[5]

It is therefore, *e.g.*, not legitimate to use the SCEFE to argue that Hawking radiation does not suffer from a trans-Planckian problem (the absence of Hawking radiation would violate the SCEFE).

The Problem of Epistemic Order

> The contemporary theoretical use of BHT and SCG gets the epistemic order backwards gets the epistemic order backwards: they cannot be used as evidence for anything else; in particular, we should not trust the derivations just because they give us results that we like, results that *seem* fruitful, but that we have no empirical evidence for the *physical* fruitfulness of

We have perturbative derivations of the SCEFE (cutting off using the saddle-point approximation in path-integral formulation of perturbative quantum gravity) that look "standard", *i.e.*, formally the same as we use in other cases of interest, *e.g.*, as in standard calculations in the Standard Model. The point, however, is that in those standard cases, we have independent *empirical* evidence that the perturbative methods work (are adequate), and thus that the approximative scheme is appropriate. We do not have this for semi-classical gravity, so any phenomena derived using the scheme cannot be used as evidence for the propriety of the scheme, much less for anything else (*e.g.*, evidence for the thermodynamic character of black holes based on Hawking radiation). We need to verify independently—and empirically—the propriety and adequacy of the semi-classical approximation *before* we can use its deliverances as evidence.

Do calculations in cosmology count? Good enough to compare to observations? Even if so, carries over to context of BHT?

1. Backreaction in Cosmology, S. Schander, T. Thiemann, arXiv:2106.06043 [gr-qc]: a review
2. Contact Susanne Schander on her work. From an abstract of a recent talk (May 2020):

> Our results show that quantum backreactions imply non-trivial corrections that are potentially phenomenologically significant.

Think about inflation. Is there anything peculiarly *quantum* about the way the inflaton is used to generate the exponential expansion phase (source in SCG)? Or the anisotropies in the CMB (mechanism in QFT-CST)? Can it really be used as a test of (*i.e.*, something like: showing minimal consistency of with observation), or even more strongly, confirmatory evidence of QFT-CST or SCG? Only if we can show that the the results of the calculation could not convincingly have come

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[5] I thank David Wallace for several conversations in which we achieved a metastable state of equilibrated constructive disagreement on these matters.

by producing the effect from a classical mechanism or source, *i.e.*, only if we can show that the underlying mechanism or source must (in at least some weak sense of the modality) be quantum in nature.

xii.14 The Opportunity of Spin Statistics

In curved spacetimes, one can derive the fundamental connection between spin and statistics—Bose-Einstein statistics for fields of integral spin and Fermi-Dirac statistics for those of half-integral spin—from the dynamics itself of the quantum fields in a way unavailable in the ordinary QFT of Minkowski spacetime, where the connection depends on Lorentz invariance and positivity of energy (Pauli 1940). See:

1. Parker (1969, 1971)
2. Parker and Wang (1989)

xii.15 Bringing It All Back Home

[*** SKELETAL, IGNORE ***]

xii.16 Concluding Philosophical Postscript

[*** SKELETAL, IGNORE ***]

Detailed discussion of philosophical literature:

1. equivalence of theories:
    a. `formal-equiv-theors.tex`, `theory-id-equiv-taxonomy-psyss.tex`, `idealns-and-regimes.tex`, `how-phys-epist-blend-in-reg.tex`, `ideal-approx-phys-gr.tex`
2. Glymour (2013)
3. Weatherall (2019a, 2019b)
4. . . .

xii.17 Concluding Scientific Postscript

[*** SKELETAL, IGNORE ***]

Technical details:

1. Hadamard condition

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