1

The problem with quantum gravity

JEFF MURUGAN, AMANDA WELTMAN & GEORGE F.R. ELLIS

"The effort to understand the Universe is one of the very few things that lifts human life a little above the level of farce, and gives it some of the grace of tragedy."

Steven Weinberg, *The First Three Minutes*, 1997

After almost a century, the field of quantum gravity remains as difficult, frustrating, inspiring, and alluring as ever. Built on answering just one question – *How can quantum mechanics be merged with gravity?* – it has developed into the modern muse of theoretical physics.

Things were not always this way. Indeed, inspired by the monumental victory against the laws of Nature that was quantum electrodynamics (QED), the 1950s saw the frontiers of quantum physics push to the new and unchartered territory of gravity with a remarkable sense of optimism. After all, if nothing else, gravity is orders of magnitude weaker than the electromagnetic interaction; surely it would succumb more easily. Nature, it would seem, is not without a sense of irony. For an appreciation of how this optimism eroded over the next 30 years, there is perhaps no better account than Feynman's *Lectures on Gravitation*. Contemporary with his epic *Feynman Lectures on Physics*, these lectures document Feynman's program of quantizing gravity "like a field theorist." In it he sets out to reverse-engineer a theory of gravity starting from the purely phenomenological observations that gravity is a long-range, static interaction that couples to the energy content of matter with universal attraction. Taken together, these facts hint toward a field theory built from a massless, spin-2 graviton propagating on a flat, Minkowski background, i.e., $g_{\mu\nu} = \eta_{\mu\nu} + h_{\mu\nu}$. The question of quantizing gravity then distills down to how to formulate a consistent quantum theory of this graviton. The consequences

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of this, quite simplistic, viewpoint are profound. For example, in the quantum theory, a massless spin-2 graviton has two helicity states and hence so too must the associated classical gravitational field have two dynamical degrees of freedom. For this counting to match, one is *forced* to incorporate a redundancy to encode that many possible classical field configurations could correspond to a single physical state, i.e., *a gauge symmetry*. Ultimately, it is this gauge symmetry that can be interpreted as the principle of equivalence in the low-energy, classical limit.

By all accounts, Feynman's foray into quantum gravity culminated in the early 1960s with a covariant quantization of the gravitational field to one-loop order. This begs the question then of why, 50 years on and with the general principles laid down, this pragmatic program of quantizing gravity has not reached completion? There are really two main problems with this quantization scheme. The first is as contemporary as they get but is really an age-old issue that goes all the way back to Einstein himself: *the cosmological constant*. Classically, there are no theoretical constraints on Λ and it can, without much ado, be set to zero. In a quantum field theory, however, every field has an infinite number of modes, each of which possess a "zero-point" energy. Consequently, one expects that the vacuum energy of the field is infinite. In flat space this problem is easily overcome by redefining the (arbitrary) zero-point of the energy scale. Gravity, on the other hand, couples to the *energy content* of a system so that when the gravitational interaction is turned on, the vacuum fluctuations of any quantized field generate actual physical effects. Moreover, even if the modes are cut off at some momentum scale, the vacuum energy density generated by the remaining modes can still be quite large, in stark contrast to all observations (about 123 orders of magnitude so, in fact).

The second problem is the equally thorny question of the *renormalizability* of the quantum theory. Although more technical, it can nevertheless be summarized very roughly as follows. Every loop in a covariant Feynman diagram expansion contributes

$$I_{\text{loop}} \sim \int d^D p \, p^{4J-8}$$

in D spacetime dimensions when the interaction is mediated by a spin-J particle. This contribution is finite when $4J-8+D<0$ and infinite when $4J-8+D>0$. In the marginal case where $4J-8+D=0$, loop contributions diverge but only logarithmically and can always be absorbed into a redefinition of various couplings in the theory. This is the case in a renormalizable theory. Gravity in four dimensions is mediated by a spin-2 boson, the graviton, and consequently receives infinite contributions at each loop order. In this case, an infinite number of parameters are required to absorb all of the divergences and the theory is non-renormalizable. An equivalent way of phrasing this is in terms of the coupling constant. In units of $\hbar = c = 1$, any theory whose coupling constant has a positive mass-dimension is finite. If the coupling constant is either dimensionless or has negative mass-dimension then the theory is renormalizable or non-renormalizable respectively. In general relativity, the coupling constant, G_N, has mass-dimension -2 and, again, the theory is non-renormalizable.[1] Nevertheless, this perturbative covariant approach historically illuminated the way forward.

Essentially, the more symmetric a theory is, the more tightly constrained are the counter-terms generated by the renormalization process and consequently, the more convergent it will be. Apparently then, one way to improve the ultraviolet behavior of a theory is to build more symmetry into it. This of course is the line of reasoning that led, in the 1970s, to the idea of *supergravity*, a theory of local (or gauged) supersymmetry that mixes bosonic and fermionic fields in a way that necessarily incorporates general covariance and hence gravity. The ultraviolet behavior of the quantum theory is under better control essentially because divergent bosonic (fermionic) loop contributions are cancelled by the associated contribution coming from the fermionic (bosonic) super-partner. For a time these supergravity theories (and the $\mathcal{N} = 8$ theory in four dimensions in particular) provided an enormous source of comfort for a community still reeling from prolonged battles against the infinities of quantum gravity.[2] However, it was soon realized that, even this much enlarged symmetry could not guarantee finiteness at all orders in the loop expansion and the supergravity machine lost a lot of its momentum.[3] Fortunately, another juggernaut loomed on the horizon.

Touted variously as the most promising candidate for a theory of quantum gravity, the "only game in town," or even the "theory of everything," string theory is a quantum theory of one-dimensional objects whose size is Planckian and whose different oscillation modes constitute the different members of the particle zoo. In particular, the first excited mode of a quantum closed string is a massless, spin-2 state that is identified with a graviton. String theory then appears to be a mathematically consistent (anomaly-free) quantum theory of gravity but, and perhaps more

[1] In contemporary terms, this non-renormalizability can be understood as a result of the fact that general relativity is an *effective field theory*, encoding low-energy gravitational dynamics (as summed up beautifully in Chapters 2 and 3). At small scales and high energies, this effective treatment breaks down and can manifest in a number of rather interesting phenomena. One such phenomenon, a change in the number of dimensions of space, can be found in Carlip's study of the small-scale structure of spacetime in this volume.

[2] So much so, in fact, that in his inaugural lecture for the Lucasian chair, Stephen Hawking declared that $\mathcal{N} = 8$ supergravity might just be the final theory signaling the "end of theoretical physics"!

[3] This momentum has resurfaced with a vengeance in the past few months (of editing this book). Following from an astounding observation of Witten on the relation between perturbative string theory and perturbative gauge theory formulated in twistor variables, a remarkable new insight appeared about the structure of gluon scattering amplitudes. When combined with the Kawai–Lewellen–Tye relations, this provided just the ammunition needed to resume the assault on the finiteness problem of $\mathcal{N} = 8$ supergravity. Indeed, initial reports from the front seem quite positive (see the discussions by Stelle and Nicolai in this volume).

importantly, it also *necessarily* contains quantum versions of the remaining fundamental interactions. Here, for the first time, was a theory where one was *forced* to consider *all* the fundamental forces of nature at once. However, famously, even after 30 years of painstaking work, string theory remains incomplete.

The problems with string theory are manifold. Historically, the first one to emerge was its dimensionality. In string theory "dimension" is no longer a fixed concept. It is instead a property of particular solutions of the theory. For example, any anomaly- and ghost-free solution of the superstring equations of motion possessing $\mathcal{N} = 1$ supersymmetry on the worldsheet must have a spacetime dimension[4] $D = 10$. While this problem can be circumvented by the old idea of Kaluza–Klein compactification it leads directly to the more thorny question of the uniqueness of solutions of the theory. Each compactification leads to a different vacuum state of string theory and since, if it is correct, at least one such state should describe our Universe in its entirety, the potentially enormous number ($\sim 10^{500}$ at last count) of consistent solutions, with no perturbative mechanism to select among them,[5] leads some critics to question the predictive power of the theory. Even more worrying is the fact that, while the theory is perturbatively finite in the sense discussed above (i.e., order by order), the perturbation series does not appear to converge. The veracity of the claims of finiteness of the theory is consequently unclear. By the time the 1990s rolled around the field found itself, somewhat understandably, in a state of malaise.

This all changed in 1995 when, building on earlier work, Polchinski discovered D-branes, a class of extended solitonic objects upon which open strings end with Dirichlet boundary conditions. This proved to be the trigger for a second superstring revolution and was followed in quick succession by Witten's landmark discovery of M-theory and the web of string dualities connecting the five known 10-dimensional string theories and 11-dimensional supergravity that same year. Even more importantly, it was the direct antecedent of Maldacena's 1997 conjecture that quantum gravity (in the guise of Type IIB string theory on the 10-dimensional $AdS_5 \times S^5$) is *holographically* dual to a gauge theory (here, a maximally supersymmetric Yang–Mills theory living on the four-dimensional boundary of AdS_5). The impact that this duality has had on contemporary theoretical physics has been enormous, ranging from heavy ion physics and quantum criticality through emergent properties of spacetime[6] and the integrability structures of both string and gauge theories. Unfortunately, even after a decade of development, Maldacena's conjecture remains just that. So while a wealth of results have already been uncovered, there remains much

[4] Although it is worth pointing out that *noncritical* string theories can exist in any dimension ≤ 10.
[5] That an overwhelmingly large number of these solutions are supersymmetric, with no viable supersymmetry-breaking mechanism in sight, does not help much either.
[6] As is discussed in Chapter 9 by de Mello Koch and Murugan.

to be understood about what the AdS/CFT correspondence tells us about the nature of quantum gravity.

The developments outlined above form part of what might broadly be called the "covariant quantization" of gravity. Of course, in a field as diverse as quantum gravity, it is not the only program that has managed to make traction. A different approach to the problem is the "canonical quantization" of gravity. Based on the seminal 1967 work of DeWitt, this scheme utilizes the constrained Hamiltonian quantization, invented by Dirac in 1950 to quantize systems with gauge symmetries, to canonically quantize general relativity. A key characteristic of the canonical approach to quantum gravity is that it is *nonperturbative*. In contrast to perturbative formulations which require a choice to be made for a background spacetime metric from which to perturb, nonperturbative canonical methods have the advantage of being background-independent. This means that *all* aspects of space and time can, in principle, be determined from solutions of the theory.[7] In practice, however, this canonical approach was, for some time, stalled by the sheer intractability of the constraint (Wheeler–DeWitt) equation in the canonical variables of general relativity.

A major breakthrough came in the mid-1980s with Ashtekar's formulation of general relativity in terms of a new set of variables related to the holonomy group of the spacetime manifold. This in turn furnished a new basis for a nonperturbative quantization of general relativity in terms of Wilson loops. The result was the theory known as *loop quantum gravity*.[8] As one of the family of canonical quantum gravity theories, loop quantum gravity is both nonperturbative and manifestly background-independent. Among its major successes[9] are a nonperturbative quantization of 3-space geometry, a counting of the microstates of four-dimensional Schwarzschild black holes and even a consistent truncation of its Hilbert space that suffices for questions of a cosmological nature to be addressed.[10] However, to pursue our analogy, on the battleground of quantum gravity, no single approach has yet proved faultless and the loop program (which includes LQG, spin-foam theories, loop quantum cosmology and, more recently, the group field theory of Oriti as outlined in Chapter 12) is no exception. Its critics point to, among other concerns, the lack of a consistent semiclassical limit that recovers general relativity and the necessarily *a posteriori* incorporation of the remaining interactions (as well as the matter content of the standard model).

[7] By contrast, in string theory for example, the dynamics of the string in spacetime should encode information about the spacetime metric so it would be preferable then that the metric not appear in the formulation of the theory. One solution to this problem is to find a viable nonperturbative formulation of the theory, as the AdS/CFT correspondence promises to provide.

[8] As described in Chapter 10 of this volume.

[9] Although, it must be said, these are not unequivocal.

[10] For an account of this so-called loop quantum cosmology, see Chapter 11 of this volume.

In addition to these two main research programs, the landscape of quantum gravity has been populated by a host of smaller, less developed, approaches that include Penrose's *twistor* program, *Regge calculus*, *Euclidean quantum gravity*, the *causal dynamical triangulations* of Ambjørn and Loll and Sorkin's *causal set theory*, each with its own fundamental tenet. The causal set program – introduced in this volume in Chapters 15 and 16, respectively – for example, is built on the principle that spacetime is fundamentally discrete with events related by a partial order that can be interpreted as an emergent causal structure.

This book has its origins in a (today, all too common) argument regarding the merits of string theory versus loop quantum gravity. After months of animated debate about quantization, symmetries, dimensions, background independence and innumerable other facets of the discussion, we realized that some of the questions we were meditating on might actually be useful to a broader community. These thoughts eventually crystallized in a wonderful workshop on the *Foundations of Space & Time* held at the Stellenbosch Institute for Advanced Study during August 2009 in honor of the 70th birthday of one of us (G. F. R. E.). The meeting brought together proponents of all the major programs in quantum gravity for a week of intense discussion and debate on the pros, cons, accomplishments, and shortcomings of each area.

By asking each speaker to be as open as possible about their own area and as curious as possible about each other's, we hoped to stimulate the kind of cross-field discussion that would make clear to everyone how far down the path to quantizing gravity we really are. The individual sessions were kept deliberately informal to facilitate such discussions. Interspersed among these were a number of focussed discussion sessions, with the most memorable of these revolving around two questions in particular. The first, "*Is spacetime fundamentally discrete or continuous?*," elicited several, varied responses with Lenny Susskind's (only partially tongue-in-cheek) "Yes!" being one of the most unexpected and (after some elaboration) interesting. For the second, open discussion, we posed the question: "*What do you want from a theory of quantum gravity?*," with the hopes of eliciting a wish-list of sorts from participants. The ensuing discussion was exactly what we expected; stimulating and insightful with answers ranging from testability at low energies to a complete understanding of the microscopic constituents of black holes.

In some ways we believe that we were enormously successful. In others not. On the one hand, language remains a significant problem in cross-field communication with only a very small set of researchers able to understand the technical nuances of other fields (and, consequently, appreciate some of the results therein). On the other hand, as disparate as they were in their approaches to the problem, almost everyone agreed that, even after all this time, the battle to reconcile quantum theory

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Excerpt
More Information

with gravity is far from over. These discussions, debates, and arguments were documented in the various contributions and synthesized into this volume. In this sense, this is arguably the most up-to-date account of where the field of quantum gravity currently stands. We hope that the reader will find reading this book as enjoyable as we did in putting it together.

2

A dialogue on the nature of gravity

T. PADMANABHAN

I describe the conceptual and mathematical basis of an approach which describes gravity as an emergent phenomenon. Combining the principle of equivalence and the principle of general covariance with known properties of local Rindler horizons, perceived by observers accelerated with respect to local inertial frames, one can provide a thermodynamic reinterpretation of the field equations describing gravity in any diffeomorphism-invariant theory. This fact, in turn, leads us to the possibility of deriving the field equations of gravity by maximizing a suitably defined entropy functional, without using the metric tensor as a dynamical variable. The approach synthesizes concepts from quantum theory, thermodynamics and gravity, leading to a fresh perspective on the nature of gravity. The description is presented here in the form of a dialogue, thereby addressing several frequently asked questions.

2.1 What is it all about?

Harold:[1] For quite some time now, you have been talking about 'gravity being an emergent phenomenon' and a 'thermodynamic perspective on gravity'. This is quite different from the conventional point of view in which gravity is a fundamental interaction and spacetime thermodynamics of, say, black holes is a particular result which can be derived in a specific context. Honestly, while I find your papers fascinating I am not clear about the broad picture you are trying to convey. Maybe

[1] Harold was a very useful creation originally due to Julian Schwinger [1] and stands for Hypothetically Alert Reader Of Limitless Dedication. In the present context, I think of Harold as Hypothetically Alert Relativist Open to Logical Discussions.

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you could begin by clarifying what this is all about, before we plunge into the details? What is the roadmap, so to speak?

Me: To begin with, I will show you that the equations of motion describing gravity in *any diffeomorphism-invariant theory* can be given [2] a suggestive thermodynamic reinterpretation (Sections 2.2, 2.3). Second, taking a cue from this, I can formulate a variational principle for a suitably defined entropy functional – involving both gravity and matter – which will lead to the field equations of gravity [3,4] without varying the metric tensor as a dynamical variable (Section 2.4).

Harold: Suppose I have an action for gravity plus matter (in D dimensions)

$$A=\int d^Dx\sqrt{-g}\Big[L(R^{ab}_{cd},g^{ab})+L_{\text{matt}}(g^{ab},q_A)\Big] \tag{2.1}$$

where L is any scalar built from metric and curvature and L_{matt} is the matter Lagrangian depending on the metric and some matter variables q_A. (I will assume L does not involve derivatives of curvature tensor, to simplify the discussion.) If I vary g^{ab} in the action I will get some equations of motion (see, e.g., [5,6]), say, $2E_{ab}=T_{ab}$ where E_{ab} is[2]

$$E_{ab}=P_a^{cde}R_{bcde}-2\nabla^c\nabla^d P_{acdb}-\frac{1}{2}Lg_{ab};\qquad P^{abcd}\equiv\frac{\partial L}{\partial R_{abcd}} \tag{2.2}$$

Now, you are telling me that (i) you can give a thermodynamic interpretation to the equation $2E_{ab}=T_{ab}$ just because it comes from a scalar Lagrangian and (ii) you can also derive it from an entropy maximization principle. I admit it is fascinating. But why should I take this approach as more fundamental, conceptually, than the good old way of just varying the total Lagrangian $L+L_{\text{matt}}$ and getting $2E_{ab}=T_{ab}$? Why is it more than a curiosity?

Me: That brings me to the third aspect of the formulation which I will discuss towards the end (Section 2.5). In my approach, I can provide a natural explanation to several puzzling aspects of gravity and horizon thermodynamics, all of which have to be thought of as mere algebraic accidents in the conventional approach you mentioned. Let me give an analogy. In Newtonian gravity, the fact that inertial mass is equal to the gravitational mass is an algebraic accident without any fundamental explanation. But in a geometrical theory of gravity based on the principle of equivalence, this fact finds a natural explanation. Similarly, I think we can make progress by identifying key facts which have no explanation in the conventional approach and providing them a natural explanation from a different perspective. You will also see that this approach connects up several pieces of conventional theory in an elegant manner.

[2] The signature is $-+++$ and Latin letters cover spacetime indices while Greek letters run over space indices.

Harold: Your ideas also seem to be quite different from other works which describe gravity as an emergent phenomenon [7]. Can you explain *your* motivation?

Me: Yes. The original inspiration for my work, as for many others, comes from the old idea of Sakharov [8] which attempted to describe spacetime dynamics as akin to the theory of elasticity. There are two crucial differences between my approach and many other ones.

To begin with, I realized that the thermodynamic description transcends Einstein's general relativity and can incorporate a much wider class of theories – this was first pointed out in [9] and elaborated in several of my papers – while many other approaches concentrated on just Einstein's theory. In fact, many other approaches use techniques strongly linked to Einstein's theory – like, for example, the Raychaudhuri equation to study the rate of change of horizon area, which is difficult to generalize to theories in which the horizon entropy is *not* proportional to horizon area. I use more general techniques.

Second, I work at the level of action principle and its symmetries to a large extent so I have a handle on the off-shell structure of the theory; in fact, much of the thermodynamic interpretation in my approach is closely linked to the structure of action functional (like, e.g., the existence of surface term in action, holographic nature, etc.) for gravitational theories. This link is central to me while it is not taken into account in any other approach.

Harold: So essentially you are claiming that the thermodynamics of horizons is more central than the dynamics of the gravitational field while the conventional view is probably the other way around. Why do you stress the thermal aspects of horizons so much? Can you give a motivation?

Me: Because thermal phenomena is a window to microstructure! Let me explain. We know that the continuum description of a fluid, say, in terms of a set of dynamical variables like density ρ, velocity $\mathbf{v}$, etc. has a life of its own. At the same time, we also know that these dynamical variables and the description have no validity at a fundamental level where the matter is discrete. But one can actually *guess* the existence of microstructure without using any experimental proof for the molecular nature of the fluid, just from the fact that the fluid or a gas exhibits *thermal phenomena* involving temperature and transfer of heat energy. If the fluid is treated as a continuum and is described by $\rho(t, \mathbf{x})$, $\mathbf{v}(t, \mathbf{x})$, etc., all the way down, then it is *not* possible to explain the thermal phenomena in a natural manner. As first stressed by Boltzmann, the heat content of a fluid arises due to random motion of discrete microscopic structures which *must* exist in the fluid. These new degrees of freedom – which we now know are related to the actual molecules – make the fluid capable of storing energy internally and exchanging it with surroundings. So, given an apparently continuum phenomenon which exhibits temperature, Boltzmann could infer the existence of underlying discrete degrees of freedom.