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QUANTUM GRAVITY MEETS &HPS

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> The world of the very small is a quantum world, and that must be as true of space and time and gravity as of electrons and photons and quarks.
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> John Wheeler

> [H]istorical case studies can be too much like the Bible ... if one looks hard enough, one can find an isolated instance that confirms or disconfirms almost any claim.
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> Thomas Nickles

> Science is what scientists have done, not what a philosopher tells us the scientist meant to do, were *really* doing, or should have done.
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> James Cushing

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PEEKING INSIDE THE BLACK BOX

To paraphrase Otto von Bismarck, as far as most philosophers are concerned, scientific theories are like sausages: it is better not to see them being made! Standard practice amongst philosophers of science is to investigate the finished products of science: the theories that emerge from the scientific process. However, as Kuhn taught us, the finished product, as presented in textbooks for example (usually providing the 'raw data' for philosophers' investigations), usually bears no trace of what is often a highly non-trivial path towards victory—though often for good pedagogical reasons.[1] What philosophers play with are so many black boxes. Until fairly recently they only focused on a handful of such black boxes, often from

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[1] Note that I certainly don't mean to disparage the textbook genre. Textbooks are significant in very many ways, beyond the merely pedagogical; and *qua* historical objects, they are as interesting as any other such objects. The textbooks one learns from can forge social identity and define a community. In the context of quantum gravity research they are especially interesting because the arrival of a textbook signals a certain degree of 'maturity' of the field. Only relatively recently have textbooks on quantum gravity begun to appear. See the chapters in Part III of [59] for more on this fascinating topic.

physics alone.[2] However, they have expanded their horizons to include a broader range of physical theories, and even theories from the social and biomedical sciences. Still, most individual accounts of how science works focus on a small selection of scientific theories, and ignore the historical and sociological details behind their construction *and* their evaluation.

This situation clearly falls way short of an integration of history and philosophy. While it is true that for some philosophical purposes this is as fine a grain of detail as one needs[3], if we are considering methodological issues, a black box approach cannot be sufficient: we need to probe inside to see what factors accounted for the success of some theory (or failure of another), and whether, in hindsight, they were good ones and/or the *only* ones. As James Cushing and others have so ably demonstrated, there is in fact often an enormous amount of contingency in theory-selection, and what appeared to be ‘the only theory for the job’ was really only one amongst several (quite distinct, yet empirically adequate) possibilities. Given such contingency, a variety of non-epistemic factors can enter into the analysis, supposedly leading to the additional input of psychology and sociology. It is precisely this intrusion that so offends philosophers of science—or at least those who cling to the distinction between the contexts of discovery and of justification: yes, social/psychological factors can enter into science *weakly*, in the discovery phase, but they should never spill over into the justificatory phase.

Though, superficially, philosophers, historians, and sociologists of science share the same object of investigation, there are, of course, many subtle (and not so subtle) differences. Chief amongst these differences is the fact that philosophers of science, inasmuch as they think about it at all, usually wish to *use* historical and sociological data to inform a *general* theory of science, or at least some explanatory thesis about the way science works—they ever seek grist for their mills.[4] Hence, their interest in history and sociology tends to be indirect, concerned with

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[2] For example, Newtonian physics, general relativity (very minimally construed: i.e. tending to focus on the ‘famous’ light-bending experiment), and astronomy. Or else, various classic ‘dead’ theories, such as phlogiston and the geocentric view. (Note, this black box analogy was traced, by Trevor Pinch, back to his supervisor, Richard Whitley—see [73], p. 488.)

[3] Philosophers of physics, for example, often need only inspect the formal representation of a theory and consider its space of possible interpretations. For this, one usually does not need to know the intimate historical details of the theory’s construction, though even here I would have grave doubts about the quality of such a *wholly* ahistorical approach. For example, it was his deep knowledge of the historical complexities of general relativity that led John Stachel to uncover the hole argument, surely one of the most important arguments in contemporary philosophy of spacetime physics.

[4] As Richard Burian puts it, “[w]hen philosophers turn to particular historical materials or case studies, they often begin with pre-established concepts and sometimes with expected conclusions in mind ... [and the] concepts employed often contain presuppositions about the nature of theory, evidence, and explanation, about the relation of experiment to theory, the objectivity and intellectual autonomy of scientific work, and the like” ([11], p. 398). Case studies can, of course, be useful for exploratory purposes (as they are in the social sciences for example). However, unless one performs an analysis of a sufficiently large sample of scientific theories (preferably chosen at random), the evidence they confer on some *general* theory of science is rather weak. (I might add that changing the terminology from “case studies” to “episodes” will not improve the quality vis-à-vis evidence for methodological theories.)

their utility (in the form of degree of support) rather than their intrinsic worth. Historians and sociologists tend to favour a more descriptive (and they might say, objective) approach; wishing to describe as faithfully as possible, present and preconceptions to one side, some scientific episode.[5] They will trawl a wide variety of sources in order to piece together an image of what happened—albeit an imperfect image, as they will acknowledge, tainted with various biases. They like to see what goes in to the sausage machine, and observe how it works, rather than just focusing on what comes out at the other end.

These professional differences can lead to some animosity between the various disciplines, and any 'integrative' approach to philosophy of science will have to try to balance these differences in outlook. Finally, it has to be said that when philosophers do employ history, it is often *bad* (or lazy) history (*cf.* [72]), for example, failing to take proper account of the different modes of presentation of a theory (and its manner of construction and justification) depending on the intended audience—journals, notebooks, interviews, textbooks, and public lectures can reveal an enormous disparity despite sharing common subject matter. Whether philosophers buy into sociological elements deep within science or not, this feature of tailoring a description to an intended audience simply cannot be doubted. As I indicated above, philosophers usually refer to textbooks. But textbooks are just as bespoke as any other public account. For the historian, 'primary sources' (especially original notebooks and correspondence) weigh especially heavily in terms of understanding theory construction and justification—*cf* [53]. For sociologists, it is the *actual practice* of science (as it unfolds) that is most revealing.

How do we encourage and enable philosophers to look inside the black box of science? How do we persuade them to look beyond the slender 'internal' (or rationally reconstructed) histories that they favour? The strong programme advocates a perfectly symmetrical treatment of 'true' (or 'good' or 'selected') science and 'false' (or 'bad' or 'rejected') science. In practice, this is rather difficult to achieve in an objective fashion because of the spectre of the way history *actually* unfolded![6] However, the most trouble-free way to achieve what this so-called symmetry postulate set out to achieve, is to probe what Bruno Latour labeled 'science in action': situations in which the truth values of the theories aren't yet settled. In a sense, such cases render the symmetry postulate redundant, for there is no fact of the matter and so no broken symmetry in need of repair. Unfortunately, there aren't many philosophically interesting (that is, interesting to *philosophers*) situations of this sort, and the examples that have been conducted have tended to be of a somewhat mundane character, often involving the discovery of some substance (such

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[5] As von Humboldt famously (though, from our present temporal location, somewhat naively) put it: "The historian's task is to present what actually happened. The more purely and completely he achieves this, the more perfectly has he solved his problem" ([54], p. 57).

[6] The strong programme is also just as problematically generalist as the standard philosophers' accounts: we need evidence to convince us that some family of once competing theories were indeed *equally* viable before consensus was reached. This might well be true of some episodes but not in others.

as a particular vaccine) rather than the construction of a theory. Philosophers, for better or for worse (though this is changing somewhat), tend to be attracted by the bright lights of *revolutionary* episodes in science and especially by overarching, universal, fundamental theories.

However, quantum gravity research offers exactly a such a situation, where the symmetry between the various competing approaches appears not yet to have been broken *and* in which we have an example of a revolution, albeit a revolution *in process*.[7] Moreover, it directly involves theory construction, and fundamental theory construction at that. Indeed, it is widely believed to be the greatest unresolved problem in fundamental physics. Despite over 80 years of hard labour, by the finest physicists, all agreed on the importance of the problem posed by quantum gravity, there is still no finished product to speak of: no culminating theory packaged in a neat black box that philosophers of science can utilise without worrying about its complex historical trajectory. Or, to return to my earlier metaphor: there's no sausage to speak of; it's still in the machine!

Quantum gravity research is all the more enticing from the point of view of (integrated) history and philosophy of science [&HPS] since (for reasons to be discussed more fully below) it is not principally guided by the standard methodological devices of empirical testing via experiments, novel predictions, or observations. Yet one can still find all of the evaluative moves (selections and rejections) ordinarily seen in 'run of the mill' scientific endeavours: theories of quantum gravity have come and gone despite being experimentally inaccessible. If not the standard methodological virtues, what is guiding theory construction and selection in this case?

In this chapter I aim to answer this question, but more generally I aim to to highlight the ways in which quantum gravity research provides an excellent example for &HPS. It enforces a 'mixed methods' approach since it involves a situation with no definitive theory coupled with an awful lot of nontrivial history containing several important theoretical casualties, despite the absence of direct experimental support.[8] This points quite naturally to a greater consideration of 'external' fac-

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[7] Schweber defines Whiggish history as "the writing of history with the final, culminating event or set of events in focus, with all prior events selected and polarized so as to lead to that climax" ([2], p. 41). While I am not strongly anti-Whiggish (I don't see that the whiff of the present can ever sensibly be eradicated from historical studies), evidently, since quantum gravity is still under construction, there is no definitive 'endpoint' towards which Whiggish histories can retrospectively chart the progression of the theory—though one can envisage the possibility of 'local' Whiggism, involving smaller historical steps. Moreover, the 'justification/discovery' distinction (the central culprit behind the disconnect between history of science and philosophy of science) looks far more flimsy in the context of quantum gravity research since the circumstances surround the construction of the theory (such the desire to have universal theories that do not have limitations of scale) become the very mode of justification. That is, a successful theory (i.e. successful to the extent that it ought to be pursued) is simply one that meets this desire in a consistent way.

[8] The historical nature of quantum gravity will also please those historians who bemoan the trend towards specialization. It has almost a century of development with no closure. Quantum gravity is a distinctively international field of research; it incorporates elements from a very wide variety of theories, and many branches of mathematics. It has witnessed both military and industrial support,

tors (if we must persist with this notion) controlling theory evaluation. Hence, we have a natural convergence of history, philosophy, and sociology. I submit that a study of quantum gravity along any one of these lines (philosophical, historical, sociological) will inevitably soon find itself incorporating the others.

Let me nail my colours firmly to the mast regarding matters methodological: I advocate a view broadly similar to that espoused by James Cushing (himself borrowing crucial ideas from Arthur Fine), according to which history is of vital importance to philosophical theses, but if one looks sufficiently closely at a wide enough sample of historical episodes in science one very quickly sees that there is no one size fits all scheme: even methodology can change if the context so demands it. The process of constructing and evaluating scientific theories, much like an economic time-series, is distinctly non-stationary. Just when it seems to be acting according to some pattern, the pattern shifts. We find this to be especially true in the case of quantum gravity research.[9] The methodological lessons of quantum gravity do not stop there: quantum gravity research is important too in our primary theories; namely, the standard model of particle physics and classical general relativity (both of which inform the standard model of cosmology). These theories would look very different were it not for the impact of quantum gravity research and the concepts and tools it has generated—indeed, this external utility has been adopted at various times to support continued research on quantum gravity.

I begin with a brief description of some peculiarities of historical research on quantum gravity, introducing the basic idea of the problem of quantum gravity by way of its beginnings. I then describe the problem of quantum gravity in more detail, focusing on the energy, length, and mass scales that characterise it, and consider the role that these scales played in early work. I then go on to introduce a variety of the main ways of proceeding with respect to the problem in these early days. My focus throughout is on the early history, pre-1960s, since beyond this the entanglement with cosmology significantly complicates matters (but see Kaiser [56; 57; 58] for some interesting work on the entanglement of *classical* general

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in addition to standard university-based support. More recently it has begun to utilise cosmology, computer simulation, condensed mater physics, and the new range of particle accelerators. A historian would be hard pressed to give a local account of quantum gravity.

[9] Quantum gravity might look unappealing for those philosophers steeped in 'the new experimentalism', for, *prima facie*, there simply are no experiments to analyse! However, recent work in quantum gravity attempts to make contact with experiments (using astrophysical data and the LHC, for example), though so far without success. The reasons behind the lack of success is interesting in itself. But even the early work which lacked experiments *simpliciter* is interesting from the point of view of how scientists go about evaluating their theories when so important a resource as experiment is unavailable. Thought experiments play a more important role (I will discuss below a foundational one, associated with Bohr and Rosenfeld's analysis of measurability of quantum fields). But also, theoretical predictions of the ingredient theories of quantum gravity (i.e. general relativity and quantum field theory) are used as (proxy) experimental data points for quantum gravity research. Most notably, perhaps, is the computation of the black hole entropy formula, that any approach worth its salt must be able to derive—Eric Curiel [14] has argued that this kind of usage of still-unconfirmed claims as *evidence* (if an approach is able to reproduce it) is illegitimate (here stemming from the semiclassical theory involving quantum fields on a classical, black hole background). However, the illegitimacy depends on what one views as the 'laws of scientific development'.

relativity and elementary particle physics), as does the emergence of string theory (on which, see: [78]). Finally, I consider, rather more directly, the implications of the development of quantum gravity research for &HPS.[10]

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ONE REVOLUTION TOO FEW?

In 1940 Einstein wrote the following words:

> The development during the present century is characterized by two theoretical systems essentially independent of each other: the theory of relativity and the quantum theory. The two systems do not directly contradict each other; but they seem little adapted to fusion into one unified theory. [...] [T]his theory, like the earlier field theories, has not up till now supplied an explanation of the atomistic structure of matter. This failure has probably some connection with the fact that so far it has contributed nothing to the understanding of quantum phenomena. To take in these phenomena, physicists have been driven to the adoption of entirely new method. [...] [T]he quantum theory of to-day differs fundamentally from all previous theories of physics, mechanistic as well as field theories. Instead of a model description of actual space-time events, it gives the probability distributions for possible measurements as functions of time. [...] All attempts to represent the particle and wave features displayed in the phenomena of light and matter, by direct course to a space-time model, have so far ended in failure. [...] For the time being, we have to admit that we do not possess any general theoretical basis for physics, which can be regarded as its logical foundation. [.] Some physicists, among them myself, can not believe that we must abandon, actually and forever, the idea of direct representation of physical reality in space and time; or that we must accept the view that events in nature are analogous to a game of chance. ([36], pp. 489–492)

It is a little curious that so many great revolutionary episodes happened almost simultaneously at the beginning of the twentieth century. Perhaps one revolution made it easier for others to follow, via some kind of snowball effect? Whatever the reason, the revolution that resulted in general relativity and the revolution that resulted in quantum theory were close neighbours in time. Einstein was profoundly involved in the creation of both theoretical frameworks, though the former more so than the latter. At the time of the construction of the general theory of relativity he firmly believed in the existence of quanta of radiation. But this only involved a belief in the property of discreteness (with no real sense of ontological

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[10] I am indebted to the brief review of the early history of quantum gravity by John Stachel: [93].

substrate beyond this), rather than belief in what would become quantum mechanics (or quantum field theory—though here too his contributions on emission and absorption of radiation proved crucial). Most physicists believe *another* revolution is required to bring quantum theory and general relativity—*cf.* [83].

Since such quanta, with their discrete energies and other properties, would inevitably couple to the gravitational field (in however small a way, the gravitational interaction being *universal*), Einstein couldn't ignore the fact that *something* would need to be said about the nature of this interaction.[11] Indeed, almost as soon as general relativity was completed, Einstein became aware of a possible conflict between it (or, more specifically, the existence of gravitational waves) and the principles of quantum theory[12], and, therefore, the need for a quantum theory of gravity. Thus, he writes that

> [A]s a result of the internal-atomic movement of electrons, atoms must radiate not only electromagnetic but also gravitational energy, if only in minuscule amounts. Since this cannot be the case in nature, then it appears that the quantum theory must modify not only Maxwellian electrodynamics but also the new theory of gravitation ([33], p. 696).[13]

In this case Einstein is clearly troubled by the potential clash between the theoretically predicted gravitational radiation combined with the empirically observable stability of atoms: *any* moving mass (even the electrons in atoms) will radiate gravitational energy. In other words, something like Planck's law of radiation would have to be found for gravitation in order to account for the stability. He repeated this claim again in 1918, stating that "an improved version of quantum theory would lead to changes in the gravitational theory" ([34], p. 167).[14]

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[11] A little later it would also come to be understood that there is a 'formal interaction' between general relativity and quantum objects stemming from the peculiar nature of fermions: including objects with half-integer spins imposes a variety of constraints on the spacetime structure, and therefore on the gravitational field (resulting in a slightly modified theory of gravitation). This was a rather slow lesson.

[12] As Kragh has pointed out, the version of quantum theory that Einstein would have been thinking about at this early phase of general relativity's development was the Bohr-Sommerfeld theory—see [64], p. 965. Einstein would have been particularly impressed with the way the Sommerfeld theory integrated (special) relativity and quantum theory. Helmut Rechenberg claims (though doesn't provide a source) that Sommerfeld published his results after Einstein informed him that the general relativity would not modify the results in any appreciable way ([76], p. 160).

[13] "Gleichwohl müssten die Atome zufolge der inneratomischen Elektron-enbewegung night nur electromagnetische, sondern auch Gravitations-energie ausstrahlen, wenn auch in winzigem Betrage. Da dies in Wahrheit in der Natur nicht zutreffen dürfte, so scheint es, dass die Quantentheorie nicht nur die Maxwellsche Elektrodynamik, sondern auch die neue Gravitationstheorie wird modifizieren müssen."

[14] By 1919 he was already going down the path of unitary field theories that would mark much of his later work: "there are reasons for thinking that the elementary formations which go to make up the atom are held together by gravitational forces" ([35], p. 191). As Stachel notes ([93], p. 526), this marks a reversal in the priority given to the two theories, general relativity and quantum theory. Whereas prior to 1919 he believed that the latter might lead to modifications in the former; here general relativity (coupled with the electromagnetic field) is now being used to *explain* the quantum structure of matter. We can surmise that it was as a result of the work by others on general relativity and its unification with electromagnetism. Max Born writes that Einstein, up until 1920, was still very concerned with the relation between quantum and relativity. Einstein wrote him: "I always brood in

This looks like a potential *empirical* motivation for pursuing quantum gravity. However, as Gorelik correctly points out, whilst atomic radiation (computed along the lines of Maxwell's theory) leads to the collapse of the atom in (order of) 10^{-10} seconds (a fact inconsistent with observations), atomic *gravitational* radiation, computed using Einstein's formula, has a collapse time of the order of 10^{37} seconds. Therefore, there would in fact be no empirical inconsistency as a result of gravitational radiation and we should not be puzzled by the stability of atoms in this case.

Gorelik ([44], p. 365) argues that an "analogy with electrodynamics" lay behind this comment of Einstein's. This analogy was a persistent feature of early research on quantum gravity—see below. One must also bear in mind that the issue of absorption and emission of radiation must have occupied a central place in his thinking at the time of writing, for his paper on the emission and absorption of radiation in quantum theory appeared very shortly afterwards—replete with the statement that "it seems no longer doubtful that the basic idea of quantum theory must be maintained". What is remarkable, given what we know of the certainty he professed about general relativity, is that he openly considered the possibility that the quantum theory would demand some kind of 'modification' (what we would now refer to as a quantum correction) of general relativity![15]

However, similar claims were made intermittently over the next decade or so, though nothing amounting to a serious attempt to construct a full-blown quantum theory of gravity was undertaken. These claims were primarily from German (or German speaking) physicists. For example, as early as 1919, Arthur von Haas writes (on the basis of 'unification' ideals) that:

> Arguably, one of the most important future tasks of the axiomaization of physics is the implementation of quantum theory in the system of the general theory of relativity. ([50], p. 749)[16]

Though he doesn't explicitly name the individual constants associated to the ingredient theories (viz. $c, \hbar, G$—see the next section), it is reasonable to surmise

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my free time about the quantum problem from the standpoint of relativity. I do not think the theory will have to discard the continuum. But I was unsuccessful, so far, to give tangible shape to my favourite idea, to understand the quantum theory with the help of differential equations by using conditions of over-determination ..." ([10], p. 257: from their private correspondence). By 1926 he was "toiling at deriving the equations of motion of material particles regarded as singularities from the differential equations of general relativity" (ibid., p. 258).

[15] This openness of Einstein to the possibility of a quantum theoretical modification of general relativity would not last for long, of course, and was already beginning to sour at this stage. His taste for quantum theory soon soured to the extent that towards the end of his life he was searching for ways to reproduce quantum mechanical phenomena using a purely classical field theory. Suraj Gupta (who developed a special-relativistic theory of quantum gravity in the 1950s) has a different (inverted) interpretation of Einstein's underlying reasons for distrusting quantum mechanics: "Because his theory is different from other field theories, he tried to construct unified field theories and because he could not see how his theory in the curved space could possibly be quantized, he criticized quantum mechanics" ([48], p. 253).

[16] "Eine der wichtigsten Zukunftsaufgaben, die in dieser hinsicht der physikalischen Axiomatik gestellt ist, ist wohl die Einfügung der Quantentheorie in das System der allgemeinen Relativitätstheorie."

that this is what Haas had in mind in the following passage:

> The main task of the axiomatization of physics will be the problem concerning the integration of the universal constants of physics. Also the solution of this question may be expected to reveal deeper knowledge of the relations, only intimated by Hilbert, holding between gravity and electricity, and of a further integration of these relations with the quantum hypothesis. (ibid., p. 750)[17]

This interpretation is strengthened by the fact that Haas went on to consider the various possible combinations of other constants in other contexts, investigating the way they demarcate domains [51].

Quantum theory was invoked several times (in discussions of general relativity, and unified field theories) to mark some kind of boundary of the *applicability* of a theory.[18] Einstein himself expressed just this view, in a lecture entitled "Ether and the Theory of Relativity" at the University of Leyden, May 5th 1920. This address is interesting for many reasons, historical and philosophical. For our purposes it is interesting because Einstein once again speculates on the possible restrictions that the quantum theory might place on general relativity:

> Further, in contemplating the immediate future of theoretical physics we ought not unconditionally to reject the possibility that the facts comprised in the quantum theory may set bounds to the field theory beyond which it cannot pass.

Indeed, we can find several examples of Einstein expressing this kind of sentiment. Inasmuch as his comments (here and in his 1916 paper) have been investigated by historians, it has tended to be in the context of the study of gravitational waves. It is true that gravitational waves are naturally involved here, but since Einstein is considering the possibility that the radiation of such waves is quantized, we ultimately have what can also be seen as heralding the beginning of quantum gravity.[19]

Perhaps the most famous interplay between gravity and quantum prior to 1930 was Bohr's usage of general relativity to argue against Einstein's 'photon in a box' critique of his interpretation of quantum mechanics, at the 1927 Solvay Congress. As Oskar Klein explains:

> We know from BOHR's account how ingeniously EINSTEIN defended his standpoint—the essential incompleteness of the quantal description of nature—and how BOHR refuted every one of his arguments with more than ingenuity. What impressed us younger people most was, I think, the "Einstein box," where BOHR successfully turned general relativity theory against EINSTEIN. ... And still EINSTEIN, who accepted all defeats with the utmost fairness but without changing his basic view, may

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[17] "Aufgabe der physikalischen Axiomatik sein wird; es ist das Problem des Zusammenhanges zwischen den universellen Konstanten der Physik. Auch die Lösung dieser Frage darf vielleicht erhofft werden von einer tieferen Erkenntnis der von *Hilbert* erst angedeuteten Beziehungen zwischen Gravitation und Elektrizitat und von einer Verknüpfung dieser Beziehungen mit der Quanten-hypothese."

[18] For example, Goldstein and Ritter note how Weyl adopts this position in his *Raum, Zeit, Materie* ([43], p. 104).

[19] The beginnings of quantum gravity are usually traced back to a 1930 paper of Léon Rosenfeld's; however, there was, aside from Einstein's remarks, quite a lot of activity dealing with the general problem of quantum gravity, i.e. concerning the joint treatment of quantum and gravity. Though Rosenfeld's paper was, so far as I know, the first paper to apply the then newly developed methods of quantum *field* theory to the problem, thus treating the gravitational field like the successfully quantized electromagnetic field.

> have felt that on the side of the quantum physicists the importance of the general relativity claim in the search for the laws of the microworld was usually underestimated. ([63], p. 117)

Einstein used quantum theory and special relativity to try to circumvent the Heisenberg relations. Bohr used a combination of quantum theory and general relativity in order to eliminate the inconsistency that Einstein derived. As Christian Møller recalls:

> Well I remember of course the excitement when Bohr was able to beat Einstein with his own weapon. That was at a Solvay meeting; Einstein invented a way of showing that quantum mechanics was not consistent. He proposed to determine the energy of the photon which had come out of the box by weighing the box before and afterwards. Then Bohr could show that if one takes Einstein's formula for the rate of a clock in a gravitational field then it comes exactly to making the thing consistent again. And Gamow even made a model of this box with a spring and clock and shutter, which opened at a certain time and closed again at a certain time. Möller [`http://www.aip.org/history/ohilist/4782.html`]

We can see from this brief look at the early days of the quantum-gravity interface that there was a real desire to join the two theories together and 'complete the revolution'.[20] Moreover, there was a general belief that constructing such a theory would be 'business as usual'. That is, it was generally assumed that there would be no *special* difficulty in quantizing the gravitational field. The earliest attempts to bring these theoretical frameworks together involved the same methods as had and would be used for the other fundamental interactions.[21]

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PLANCK SCALE PRAGMATISM

The issue of *defining* quantum gravity is itself fraught with some historical difficulties. The notion has changed as other areas of physics (and mathematics and cosmology) have advanced. Ashtekar and Geroch, in their review of quantum gravity, characterize quantum gravity as "some physical theory which encompasses the principles of both quantum mechanics and general relativity" (Ashtekar and Geroch, 1974, p. 1213). This leaves a fair amount of elbowroom for the form such a theory might take.

We can, however, say with certainty at what scales quantum gravitational effects would be expected to manifest themselves. This follows from the fact that there is a unique way to mix the fundamental constants that characterise the 'ingredient' theories so as to generate units of (L)ength, (M)ass, and (T)ime. From general relativity we have the gravitational (or Newton) constant G_N (equal to $6.67 \times 10^{-11} m^3/\mathrm{kg\ sec}^2$), characterising the scale at which generally relativistic effects

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[20] Though this barely skims the surface of a deep vein of early work on quantum gravity. For a more detailed, thorough study of the very earliest research on quantum gravity, see [77].

[21] As Abhay Ashtekar puts it, the methodology was "to do unto gravity as one would do unto any other physical field" ([2], p. 2). As is becoming clear after decades of intense effort, gravity is not like any other force, at least not in terms of its formal representation, nor, many believe, in terms of how it is (or ought to be) conceptualized.

matter, and from quantum field theory we have c (the velocity of light *in vacuo*) and $\hbar$, Planck's constant of quantum action. These combine to give us:

$$L_P = \sqrt{\frac{\hbar G_N}{c}^{3}} = 1.616 \times 10^{-35} m \tag{1}$$

$$T_P = \sqrt{\frac{\hbar G}{c}^{5}} = 5.59 \times 10^{-44} \mathrm{sec} \tag{2}$$

$$M_P = \sqrt{\frac{\hbar c}{G_N}} = 2.177 \times 10^{-5} g \tag{3}$$

At these scales, all three physical theories are expected to play a role, and (if we accept that general relativity is a theory of spacetime geometry) it is this scale that we expect quantum geometry to dominate. Curiously, these units were discovered by Planck almost three decades before quantum field theory was discovered, and almost two decades before general relativity was completed (and six years before special relativity): [75]. Planck was interested in producing *universal* descriptions of the world, that could even be understood by extraterrestrial civilisations! For this reason he pursued a set of natural scales that would make no reference to such local circumstances as the size of the Earth or aspects of its orbit and rotation.[22]

A kind of (quite understandable) pragmatism guided the early neglect of quantum gravity research. The scales at which phenomena would be apparent were known then to be well out of reach of direct tests.[23] Though Dirac believed quite firmly that general relativity and quantum theory would have something to say to each other (and indeed did important work on the subject), he nonetheless accepted the pragmatic argument:

> Since the time when Einstein's general theory of relativity first appeared, various more general spaces have been proposed. Each of these would necessitate some modifications in the scheme of equations

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[22] See [44] for more on the curious discovery of these units and their subsequent propagation into early quantum gravity research. Note that by the mid-1950s the notion of the Planck length was understood by those working on the so-called canonical approach as a measure of the fluctuations of spatial geometry. For those working along spacetime covariant approaches, the Planck length marked a natural boundary to the wavelengths of quantum fields. See §4 for more on these two approaches.

[23] The characteristic 'Planck length' is computed by dimensional analysis by combining the constants that would control the theory of quantum gravity into a unique length. As shown above, this is $l_p = \sqrt{\hbar G/c^3} = 1.6 \times 10^{-33} cm$: a minuscule value, making gravity (effectively) a 'collective phenomenon' requiring lots of interacting masses. That quantum gravitational effects will not be measurable on individual elementary particles is, therefore, quite clear: indeed, the Planck energy is $\sqrt{\hbar c^5/G} = 10^{22} MeV$! Bryce DeWitt devised rigorous arguments to show this to be the case: the gravitational field itself does not make sense at such scales. He showed that the static field from such a particle (with a mass of the order 10^{-20} in dimensionless units) would not exceed the quantum fluctuations. The static field dominates for systems with masses greater than 3.07×10^{-6}. The gravitational field is from this viewpoint an 'emergent' "statistical phenomenon of bulk matter" ([23] p. 372). An earlier version of this viewpoint was suggested by van Dantzig [18]. The idea that gravity is emergent, has gained in popularity recently: see [69] for a review.

> of atomic physics. The effects of these modifications on the laws of atomic physics would be much too small to be of any practical interest, and would therefore be, at most, of mathematical interest. ([28], p. 657)

However, Oskar Klein (describing his own approach as a contribution to "an intimate alliance of the two fundamental viewpoints of present physics, that of complementarity and that of relativity" ([63], p. 117)) describes and rejects this pragmatist attitude:

> Now, it is very usual to regard the point of view of general relativity as insignificant in quantum theory because the direct effects of gravitation in ordinary atomic phenomena are very small.This, however, may easily be the same kind of fallacy, which it would have been to regard the electron spin as unimportant for the formulation of the laws of chemical binding, because the direct interaction between spin magnetic moments is, in general, negligible compared with chemical binding energies.
>
> [W]e shall tentatively take the point of view that general relativity is fundamental for the formulation of the laws of quantum field theory and that the demand of an adequate formulation of other invariance claims, e.g. that of gauge invariance, should be regarded as an indication of the need for a natural generalization of the relativity postulate. [ibid, p. 98]

As I suggested above, a second factor behind the neglect was that the early views on quantum gravity were tightly bound to the quantization of the electromagnetic field in quantum electrodynamics. It was thought that there would be no *special* puzzles caused by quantizing the gravitational field, since surely one classical field is much like any other. For example, in their famous paper marking the birth of QED Heisenberg and Pauli wrote:

> We might also mention, that quantization of the gravitational field, which also appears to be necessary for physical reasons, may be carried out by means of an analogous formalism to that applied here without new difficulties. ([52], p. 3)[24]

These days we have a few more physical, quasi-empirical reasons to think that a quantum theory of gravity is necessary. General relativity is now (thanks to the singularity theorems) firmly believed to generically predict spacetime singularities. It is thought that our own universe may have emerged from such a singularity (= "the big bang", and may wind up in another (= "the big crunch"). It is also thought that they may exist in within our Universe inside black holes. General relativity does not apply to singular situations, so a theory of quantum gravity is expected to tell us what happens here. Such reasoning was not open to the earliest researchers on quantum gravity since inasmuch as they were understood at all, singularities were thought to be fictional. Nor did the big bang model (and the notion of a big crunch) exist during the initial phases of quantum gravity research—even when it was conjectured, it was not taken up easily).

Another piece of information that suggests the need for a quantum theory of gravity came from the consideration of quantum field theory on (fixed) black hole

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[24] "Erwähnt sei noch, daβ auch eine Quantelung des Gravitationsfeldes, die aus physikalischen Gründen notwendig zu sein scheint, mittels eines zu dem hier verwendeten vollig analogen Formalismus ohne neue Sehwierigkeiten durchführbar sein dürfte." Note that Heisenberg and Pauli explicitly mention the remark of Einstein's from 1916, along with Klein's 1927 paper on five-dimensional quantum theory, in a footnote attached to this passage.

spacetimes. Hawking discovered that in such a semi-classical theory (QFT coupled to a *classical* gravitational field), black holes emit radiation and can evaporate (= “The Hawking Effect”). However, the semi-classical theory is not sufficient to analyse all aspects of the process, since the ‘end point’ falls outside. There are several possibilities for the final stage: a (most likely Planck-scale) remnant, unitary evolution (not to be had in the purely semiclassical theory), or total evaporation (and, therefore, information loss).[25]

We might also mention the predicted value of the cosmological constant (the energy of empty space) made by quantum field theory, on the basis of the zero-point modes.[26] The observed value for the energy density comes out very close to zero: $\rho \simeq 10^{-30} gcm^{-3}$. This is a very long way from quantum field theory's prediction. One way to bring this value down is by imposing a cutoff at the Planck length, ignoring those modes that have wavelengths smaller than this, or by turning on the interactions between the vibrational modes.

These other reasons would take some time, and required advances in cosmology and astrophysics, amongst other things. The nature of the problem of quantum gravity adapted itself to these new conditions, by setting itself new puzzles (such as the conditions surrounding the big bang and within the interior of a black hole) and by utilizing any new data as targets that a respectable approach must hit. The construction of renormalized quantum field theory, and the renormalization group, would also stimulate and radically modify new work on quantum gravity. Certainly, by the 1950s, it was no longer believed that the quantization of the gravitational would be a matter of course.

4
THE SLOW AND DIFFICULT BIRTH OF QUANTUM GRAVITY

> Although the quantum description of the gravitational field has many points of similarity to conventional quantum field theory, it nevertheless seems incapable—or capable only with difficulty—of incorporating certain conventionally accepted notions. [Bryce DeWitt [1], p. 330]

Bryce DeWitt was one of the first people to write a doctoral thesis on quantum gravity, which he did under the (somewhat minimal) supervision of Julian Schwinger at Harvard University in 1950, a time when quantum gravity research was still very unfashionable. He wrote the words above in 1962. Though they are

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[25] The fact that black holes are thought to radiate implies that they possess an entropy too. Bekenstein computed this as $k_B \frac{A}{AL_P^2}$ (i.e. the entropy goes up a quarter as fast as the black hole's horizon, or surface area, goes up). This result offers a number that the latest approaches to quantum gravity are expected to be able to derive—many of them are indeed able to do so.

[26] The energy spectrum of an harmonic oscillator, namely $E_N = (N + 1/2)\omega$, has a non-zero ground state in quantum mechanics. This is the zero-point energy, standardly explained by reference to the uncertainty principle (i.e. there's no way to freeze a particle). In the context of (free) quantum field theory the field is understood to be an infinite family of such harmonic oscillators, and as a result the energy density of the quantum vacuum is going to be infinite on account of the nonzero contribution from each vibrational mode of the fields being considered.

expressed with an air of obviousness, they encode within them more than four decades of struggle to try and treat the gravitational field and quantum theory within in the same framework.

Between 1950 and 1962 gravitation research underwent a significant transformation, as a result of several (often interdependent) factors. DeWitt himself dropped gravitation research after his doctoral work, and worked instead on detonation hydrodynamics (specifically computer simulations). This work would in fact turn out to be highly applicable in general relativity in the subfield of numerical relativity. DeWitt became a pioneer in numerical relativity, and discovering ways of programming aspects of relativity. A technique he developed at Livermore—multidimensional (two and three dimensions in DeWitt's example) Lagrangian hydrodynamics [21]—was later used in the gravitational 2-body problem and black hole simulations.[27] One might think of this as a flow of ideas from his war work into gravitational research. However, the flow worked both ways: DeWitt was able to develop his ideas on higher dimensional Lagrangian hydrodynamics on account of his general relativistic background with general coordinate invariance and Jacobians. However, there is no doubt at all that his experience with computing influenced his thinking enormously.

DeWitt was involved in a a chain of interesting events relating to the history of quantum gravity. He was a physicist emerging right at the end of the second world war. Moreover, he did his postgraduate work at Harvard, where there was a close connection between the students' work and military applications—many did their 'work experience' on military projects. Those who had done work for the military developed a certain 'number crunching' mindset. There is little doubt that this *modus operandi* filtered in to the work that resulted after the war. DeWitt's work especially was highly computational.

However, there were other key circumstances that contributed to the fortunes of quantum gravity research (bringing DeWitt back into the fold in a central way), involving (amongst other things) DeWitt's marriage to a French mathematical physicist (Cécile Morette, a great organizer of conferences and people, as well as a great mathematical physicist herself), various interventions in relation to funding opportunities by John Wheeler, an off-hand submission to an essay competition, and industrial, military, and government support. One can see in this story the attempts of various parties to produce a convergence of interests. This wasn't always possible. Scientists desire freedom to pursue whatever research project they desire with sufficient resources to pursue their goals, and the industrial and government sources have a more diverse set of goals and interests, include potential technological applications (leading to financial gains and power gains), prestige, or perhaps understanding of some aspect of the world.

In the case where the scientists have as their goal the "navigation among the potentialities proffered by nature" John Stachel has described this process of conver-

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[27] See [22] for DeWitt's own reminiscences about his time at Livermore working on this approach, and its later relevance—see also [89], p. 13.

gence as one of “negotiation” ([94], p. 143). Any account of scientific discoveries must take account of this milieu, though Stachel is quick to point out that this does *not* imply the neglect of nature. Though there is a certain amount of elbow room in scientific discoveries, and so the evolution of scientific research and the nature of the theories that result, all of this this must be in accord with the “the potentialities proffered by nature”.[28] This is not strong social constructivism, then. The contingency is very heavily constrained by nature.

Stachel goes on to give an alternative possible scientific history, in which a different theory of gravitation was ‘discovered’ that was perfectly in accord with nature’s potentialities (since it matches Einstein’s version on all relevant observables). He borrows the example from Feynman who asked: “Suppose Einstein never existed, and the theory [of GR] was not available” (cited in [94], p. 146).[29] Could one replicate ‘the physics’ of Einstein’s gravitational theory using what other (non-geometrical) tools were available, namely special relativistic quantum particle theory? The answer is Yes[30], as several people had already suspected before Feynman posed his question. One uses the fact that the gravitational interaction has observed qualitative properties that can be encoded into field quanta (named gravitons) with specific properties:

- Obeys inverse-square law—and so is *long range*
- is always attractive
- Macroscopicly observable
- Couples to all massive objects with equal strength independently of their constitution
- Causes a red shift
- Bends light around the Sun
- Causes a correction (relative to Newton’s theory) in the perihelion of Mercury

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[28] Indeed, he gives as a very apt example, the U.S. Air Force’s support of ‘anti-gravity’ projects: no amount of support of any calibre could generate such a phenomenon. In this sense, the goals of the U.S. Air Force were not in accord with the potentialities of nature: no amount of coaxing was able to bring it about. I might add that there was even a convergence between government and industry (in the form of Roger Babson, a wealthy businessman who was searching for a gravity shield). For more on these and related aspects of the history of general relativity, see: [42; 58; 61; 60; 79; 77].

[29] This question was asked in the context of a pivotal conference in the history of gravitational research, including quantum gravity research: *Conference on the Role of Gravitation in Physics*. See [27] for the report of this conference, and a description and assessment of the conference.

[30] Although the matter is not as straightforward as Stachel (and Feynman) suggest. For details on the subtleties involved, see [102]. Note also that Stachel suggests the analysis takes place in the context of quantum field theory; in fact the analysis involves the particle picture only.

One then assigns properties to the exchange particle in a somewhat bespoke fashion. We can see immediately that the particle must be massless in order to satisfy the long-range requirement (and also to get the right value for the bending of starlight around the Sun). The fact that gravitational effects can be seen at macroscopic scales means that the particle must have integer spin. A more complex argument is required for the attractiveness properties. We will skip this here (but see [103] for the full argument), and simply note that a spin-2 particle is demanded in order to have *universal* attraction that couples in the right way to matter. The particle must also self-interact by virtue of universality—it is this that causes the nasty divergences in the quantum theory at high frequencies since it leads to graviton-graviton coupling.

Hence, had certain contingent factors been otherwise, we might have had a very different theory of gravitation.[31] Though it is clear that there isn't an unlimited supply of empirically adequate alternatives, and they are often very hard to construct. Inasmuch as one approach could be rationally (or logically, in Duff's terms) justified, so could the other. The selection of Einstein's geometrical approach is based on reasons outside of the standard cluster of empirical factors. In this case, we have the pragmatism mentioned earlier, coupled with the mere temporal precedence of the geometrical approach.

The two approaches are in fact jointly pursued to this day. Steven Weinberg's textbook on gravitation and cosmology uses the Lorentz invariant particle physics approach. The approach matches his training as a particle physicist. The division into two communities (the geometric relativists, and the analytical particle physicists) is a genuine phenomena that has deeper ramifications. There goes, side by side with the division, attitudes with respect to what are deemed relevant, important, and interesting questions. Feynman in particular was of the opinion that the particle physicist's approach to general relativity involved a healthy rejection of philosophical issues:

> The questions about making a "quantum theory of geometry" or other conceptual questions are all evaded by considering the gravitational field as just a spin-2 field nonlinearly coupled to matter and itself ... and attempt to quantize this theory by following the prescriptions of quantum field theory, as one expects to do with any other field. ([37], p. 377)

The general relativists by contrast show a deep engagement with conceptual issues, having to do with the nature and existence of space, time, change, and so on.

In fact, many early approaches, including Birkhoff's flat-space approach, were rejected because they did not meet the requirements set by these latter tests. But, still it is very possible that had quantum field theory been to hand earlier, and had quantum gravity been seen as more of a pressing problem (e.g. if the pragmatic argument was absent, or there was a greater desire for unification for the sake of

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[31] Michael Duff makes a very similar point very clearly in his discussion of the approach to quantum gravity that follows this 'alternative path' (covariant quantization): "the historical development of a physical theory and its logical development do not always proceed side by side, and logically, the particle physicist has no strong a priori reason for treating gravity as a special case" ([32], p. 79).

unification), the flat space, special relativistic approach to gravity might have superseded Einstein's on account of its greater amenability in terms of quantizability, and its formal coherence with the rest of physics.

We backtrack in the rest of this section, to investigate the earliest work on quantum gravity. The aim here is to highlight the motivations behind construction, selection, and rejection. The key tools are the use of simplification techniques and analogies (with successful, superficially related theories).[32]

4.1 Linearization

Simplification often characterises the earliest work in some field. One might find toy models, for example. Or, in cases were one has a non-linear theory, the use of linearisation techniques. General relativity is a non-linear theory: gravity couples to energy-momentum, and the gravitational field has energy-momentum, therefore gravity gravitates. This is part and parcel of the equivalence principle. The nonlinearities lead to many (but by no means all) of the complications that are faced in quantum gravity. Rosenfeld, in his 1930 work, attempted to quantize the linear theory. This, as most acknowledge, is a preliminary exercise. One would attempt to account for the nonlinearities by adding quantum corrections.

George Temple [96] introduced the perturbative method into GR, whereby the metric tensor is expanded in powers of the gravitational coupling constant [96]. The linear expansion is, as mentioned, much easier to quantize: waves of a particular frequency ν are simply quantized according to Einstein's relation $E = \hbar\nu$ (where the energy packets $\hbar\nu$ are the gravitons[33])—the higher order terms are the problematic ones, since they determine graviton interactions (including self-interactions). Solutions of the (unquantized) linearized field equations correspond to weak gravitational radiation in empty space. The quantized radiation would correspond to a small number of gravitons propagating in empty space.

In 1939 Pauli and Fierz, in a general study of the quantization of fields [38], also employed the linear approximation of general relativity, and only considered this linear field in interaction with the electromagnetic field.[34] This approach was im-

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[32] These two often come together as a package. For example, one of the simplification techniques (discussed below) is to linearize the theory, so that the quanta of the theory do not interact and self-interact. This is the case in quantum electrodynamics, the only successfully quantized theory in earlier times. Hence, the simplifying move and the analogy move produce an equivalent result.

[33] According to Stachel, this particle was coined 'graviton' by Blokhintsev and Gal'perin [9]. There is another usage of the term in 1935 by Sir Shah Sulaiman (a mathematician and high-court judge) who put forward a competing theory to Einstein's which postulates the existence of gravitons on which the pull of gravity depends (*Science News Letter*, November 16, 1935, p. 309).

[34] In this paper, we also find for the first time the idea that gravity corresponds to a massless, spin-2 field, so that the particle carrying the force would be massless and spin-2 (note that the presence of spin-2 particles implies that a theory containing them would, ceteris paribus, be generally covariant). Thus, they write: "for vanishing rest-mass, our equations for the case of spin 2 go over into those of the relativity theory of weak gravitational fields (i.e. $g_{\mu\nu} = \delta_{\mu\nu} + \gamma_{\mu\nu}$, neglecting terms of order higher than the first in $\gamma_{\mu\nu}$); the 'gauge-transformations' are identical with the changes induced in $\gamma_{\mu\nu}$ by infinitesimal co-ordinate transformations" ([38], p. 214)—see also (Fierz, 1939; Fierz and Pauli, 1939)

portant for future developments in quantum gravity research, however, it suffered from an inability to recover the perihelion in the classical limit (when coupled to matter).[35] Given the desire to have a theory of gravity that was in step with the other forces, the approach was, nonetheless, developed further.

Suraj Gupta was the first to explicitly split the metric tensor apart, into a flat Minkowskian part and the residue. The residue was conceived as a gravitational field potential, and would represent the gravitational interaction. Hence, the theory amounts to a specially relativistic quantum field theory of gravitation. Gupta tackled the problem in two stages: first he considered the linear theory (as Pauli and Fierz had done). This has the problem that there are negative energy states, with no physical counterpart. He then, in a second paper, considers the gravitational field interacting with the full energy-momentum tensor.

Belinfante and Swihart developed a version of this approach (for which they claim priority over Gupta: [4], p. 2). They initially attempted a quantization of Birkhoff's theory of gravity but were unable to find a Lagrangian that gave Birkhoff's equations of motion—a fact they interpreted as the inability of the theory to satisfy the reciprocity (i.e. action-reaction) principle linking gravity and matter.

The earliest approaches to quantum gravity in this sense (i.e. in the sense of quantization of the gravitational field) were, quite naturally, pursued by those who had skills in quantum field theory. However, even as early as 1938 Jacques Solomon argued that the standard field quantizations methods would fail for *strong* gravitational fields, for then the approach ceases to give a good approximation to Einstein gravity ([92], p. 484).[36] Alternative approaches were suggested as the extent of the problems facing quantum field theory became ever more apparent. In particular, it was argued that quantum field theory might be better adjusted to fit general relativity, rather than the other way around:

> Obscurity is about to come to an end. Quantum field theory has now reached a stage where the emergence of new points of view alone will lead to real progress. A considerable step forward was made immediately after the second world war when Schwinger in this country and Tomanaga in Japan, and with them several other investigators, introduced consistently (special) relativistic procedures into the quantum theory of the electromagnetic field. Further progress on a fundamental level will very likely be brought about by the introduction of general-relativistic approaches into quantum field theory. This opinion is based on the fact that general relativity gives us a deeper understanding of the nature of fields and their relationships to particles than has been achieved anywhere else in theoretical physics. This understanding will be preserved by any theory that will maintain the principle of equivalence (similarity of gravitation with inertial effects, such as centrifugal "forces"), even though it may deviate in its specific details from the general theory of relativity as it was originally conceived by Einstein. ([7], p. 112)

(especially 6 from the latter).

[35] Birkhoff later developed a theory of gravitation based on flat spacetime [8], and this was quantized by his student Moshinsky [67]. However, it suffered from the same empirical problems that Pauli and Fierz's theory faced. Note that the covariant approach is not the only approach to involve flat space. The ADM (Arnowitt, Deser, Misner) approach (a canonical approach: see below) also involves flat space quantization (the flatness is in this case asymptotic).

[36] As Stachel notes ([93], p. 561), Solomon, along with Matvei Bronstein (another early maverick in quantum gravity), were both casualities of the war, of Hitler and Stalin respectively.

The linearization approach was recognized to be a provisional step up the ladder. Even at this early stage of development (and though there were alternative approaches), we can see rational moves in operation guiding the evaluation of the linearization approaches.

4.2 The Electrodynamical Analogy

Formal analogies between general relativity and Maxwell's theory of electromagnetism misled quantum gravity researchers for many years.[37] As Bryce DeWitt nicely put it, at the time of the first studies of quantum gravity "[q]uantum field theory had ... scarcely been born, and its umbilical cord to electrodynamics had not yet been cut" ([25], p. 182).[38] Moreover, renormalized QED was not yet constructed, and so the divergences could not even be properly conceptualized, yet alone resolved. Note, the analogy to electrodynamics even pervaded the early naming of the theory of quantum gravity, which was labeled 'quantum gravidynamics' (a name long since discarded).

Among the very earliest studies was a paper by Leon Rosenfeld in which he undertook a (tree-level, lowest perturbative order[39]) computation of the gravitational self-energy of a photon. It was known that the electron's interaction with its own field (the electron's electromagnetic 'self-energy') suffered from divergences (attributed to its non-vanishing mass), and, as was to be expected, Rosenfeld's calculation revealed (quadratic) divergences for the gravitational case too.[40] This

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[37] However, in many cases the analogies were necessary to get a foot hold on general relativity. For example, in the context of gravitational radiation the notion of electromagnetic radiation offered many essential clues. Felix Pirani is unequivocal about the utility of this radiation analogy (partial though it is): "*Some* analogy has to be sought, because the concept of radiation is until now largely familiar through electromagnetic theory, and one cannot define gravitational radiation sensibly without some appeal to electromagnetic theory for guidance" ([74], p. 91).

[38] However, the formulation of Maxwell's theory, championed by Mandelstam, using path-dependent variables (holonomies) was very productive, leading the way to loop gravity—see (Mandelstam, 1962), p. 353 and also (Gambini and Pullin, 1996).

[39] In field theory one seeks to calculate the *amplitude* for occurrence of processes. The perturbative approach, where one expands some quantity in powers of the coupling constant of the theory, offers the standard methodology. Feynman developed a fairly mechanical diagrammatic notation for doing these computations. There are two types of diagram: those with (one or more) closed loops and those without closed loops. The latter are known as tree diagrams, and they are the simplest to evaluate since the (4-momenta of the) external lines determine the (4-momenta of the) internal lines, with no need to perform integration over the internal momentum variables. By contrast, diagrams with closed loops have internal lines that are not determined by the external ones. For each loop there is a 4-fold integration to be performed (each involving integration over the independent momentum variables).

[40] Though Rosenfeld was one of the earliest quantizers of gravity, by 1966 he was less convinced that there was a problem of quantum gravity [82]. Or at least, in his mind the problem had been radically misconceived as a mathematical one (involving the necessity of unification on the basis of formal inconsistencies) instead of an empirical one. For Rosenfeld that absence of empirical clues meant that one could only ever probe quantum gravity from the "epistemological side", which implied that the considerations could not establish the conformity of any such investigations to the world of phenomena: "no logical compulsion exists for quantizing the gravitational field" (p. 606). Peter Bergmann called the problem one of "esthetic unease" ([5], p. 364).

pointed to a generic problem facing any field theory.[41]

The electrodynamic analogy already began to break down at the classical level, as a result of the investigation into gravitational radiation and the possibility of its measurement. The measurement of electromagnetic radiation involves third derivatives of position—one measures a fluctuating 'jerky' force (accelerations alone would give a *steady* force). Forces are represented by gradients of potentials, in this case the force is given by the first spatial derivative of the potential, so that the radiation must be given by the derivative of this, namely the second spatial derivative of the potential telling us how the force is changing. In general relativity, on the other hand, the situation is more complicated. Radiation would be given by the third derivative of the potential—the second derivative would describe a static gravitational field (i.e. curvature).

A second difference concerns the difference in the respective charges of electromagnetism and gravitation. In the case of the electromagnetic field, there are both positive and negative charges, so one can get neutralising effects. In the case of gravity there are only 'positive' charges (or at least just one kind of charge, all with the same sign).

A third difference concerns the non-linearity of gravitational interactions. Any quantization of the theory would result in quanta that interacted with each other, and *self*-interacted! This is quite unlike the situation in quantum electrodynamics, which turned out to be special in respect of its linearity. These blatant differences did not deter researchers, for the case of quantized Maxwell theory was one of the few available tools to guide theory construction, in spite of its imperfect fit.

The divergence problem (in standard non-gravitational quantum field theory) guided the development of the early work in very large part. Mandelstam was concerned with avoiding the use of an indefinite metric to construct a quantum field theory. Pauli and Källen had argued (in their study of the Lee model with cutoff [70]) that there were a certain values $g > g_{critical}$ of the coupling constant at which the use of a Riemannian metric breaks down and the theory becomes non-renormalizable. Mandelstam (like many others) claimed that this was unphysical.[42]

However, in the 1960s, the electromagnetic analogy did bear some fruit in the detection of quantities of the 'right sort' for classical and quantum gravity; namely, coordinate independent, gauge-invariant observables. If one can find these observables, then one could in principle turn them into operators and construct a Hamiltonian. In the electromagnetic case, one has the electric and magnetic fields at one's disposal. These are gauge invariant entities. However, if one then considers the behaviour of charges interacting with the fields, then operators associated

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[41] DeWitt later performed a gauge-invariant, Lorentz covariant version of Rosenfeld's computation (using the tools of renormalization that had only recently been showcased at the first Shelter Island conference). DeWitt showed that contra Rosenfeld, the analysis revealed the necessity of charge renormalization, rather than a non-vanishing mass).

[42] The introduction of an indefinite metric to resolve divergence problems in quantum field theory was given by Dirac in 1942 [29].

with the particles are not gauge invariant: gauge transformations alter the phase of the particles. The solution is to consider path-dependent quantities known as holonomies (see [66], p. 347):

$$\Phi(x, P) = \phi(x) e^{-ie \int_P^x dz_m A_m(z)} \tag{4}$$

Hence, rather than working with local particle operators, one works with these spread out quantities (and the electromagnetic field). This approach retains the covariance of the original theory. Mandelstam argues that, in this case at least, "there is a close analogy between the electromagnetic field and the gravitational field" ([66], p. 353). The development of Yang-Mills theory saw the development of a new analogy. Because of the closer similarities between Yang-Mills fields and the gravitational field (both are non-linear and have infinite dimensional gauge groups), Murray Gell-Man suggested to Feynman (in the late 1950s), who was becoming interested, that he attempt to quantize Yang-Mills theory first, as a preparatory exercise ([37], p. 378). This led to groundbreaking work in the construction of quantum gauge field theories (of the kind that make up the current standard model of particle physics).

4.3 Gravity as a Natural Cutoff

As explained above, much of the early work was characterised by a desire to construct a theory free of divergences: a finite theory. Or at least a theory with 'controllable' infinities. There were several methods that were developed to achieve this, based around the introduction of fundamental length scales. The potential utility of introducing gravity into elementary particle physics, so as to eliminate divergencies, spurred on work on quantum gravity enormously. The most obvious strategy is to impose a cutoff. There were many suggestions that gravity might act in this way, as some kind of 'regulator'. The divergences in question were those of QED, and meson theories, which were still, pre-WWII, a somewhat mathematically murky territory. The problem concerned the transitions between quantum states, during which time (a very short time, determined by the uncertainty relations) energy conservation is violated. The great hope for introducing gravity into elementary particle physics was that it would terminate the wavelengths before they get to the problematic high-energy (ultraviolet) wavelengths.

Pauli makes several comments to this effect, including the following remarks in a letter addressed to Abrikosov, Khalatnikov, and Pomeranchuk:

> I was very interested in *Landau's* remarks on the possibility of a connection of the cut-off moment of quantum electrodynamics with *gravitational* interaction (his article "on quantum theory of fields" in the Bohr-festival volume). It appeals to me, that the situation regarding divergencies would be fundamentally changed, as soon as the light-cone itself is not any longer a c-number equation. Then every given direction is space-time would have some "probability to be on the light-cone", which would be different from zero for a small but finite domain of directions. I, however, that the *conventional* quantization of the $g_{\mu\nu}$-field is consistent under this circumstances.
>
> (Zürich, 15 August 1955; in [109], p. 329)

He followed up on this same line in his comments after a talk by Oskar Klein:

> It is possible that this new situation so different from quantized theories invariant with respect to the LORENTZ group only, may help to over- come the divergence difficulties which are so intimately connected with a c-number for the light-cone in the latter theories. (Pauli in (Klein, 1956), p. 6928)

However, as with the linearization approaches, and those based on the electromagnetic analogy, the divergences in the gravitational case were more complicated than had been used to, for precisely the reasons Pauli alludes to:

> [I]t must constantly be borne in mind that the bad divergences of quantum gravidynamics are of an essentially different kind from those of other field theories. They are direct consequences of the fact that the light cone itself gets shifted by the non-linearities of the theory. But the light-cone shift is precisely what gives the theory its unique interest, and a special effort should be made to separate the divergences which it generates from other divergences. ((DeWitt, 1962), p. 374).

In his PhD thesis Bryce DeWitt, under the supervision of Julian Schwinger, sought to revisit Rosenfeld's work on the computation of gravitational self-energies. DeWitt would also revisit the idea of Landau that gravity might act as a natural regulator [26]. Though Landau didn't explicitly mention the Planck scale (he placed the location of the cutoff much higher), Pauli appeared to think that Landau had quantum gravitational effects in mind. It is clear that if there is a fundamental length, below which quantum field theoretic processes cannot operate, then one has what Landau sought. DeWitt was able to confirm that (at lowest order of perturbation) when gravity is included, the self-energies of charged particles (and the gravitons themselves) remain finite (though often very large).

More indirect, however, was Peter Bergmann's method of utilising the fact that the gravitational field equations determined particle trajectories free of any notions of divergences. He believed this would follow from the analysis of Einstein, Hoffmann and Infeld, according to which the assumption of geodesy for a free particle's motion was redundant, since it already could be seen to follow (by a method of successive approximation) from the field equations alone.

I might also note here that ultimately string theory emerged from the divergences problems facing quantum field theories of fields other than the electromagnetic field (particularly the strong interaction). In particular, since the perturbative approach breaks down when the coupling constant determining interactions strengths is high (as in strong interaction physics), alternative approaches were sought in the late 1950s and throughout the 1960s. One of the more popular of these approaches combined Heisenberg's S-matrix theory with dispersion theory. The S-matrix is a tool to encode all possible collision processes. Heisenberg suggested that one take this to embody what was relevant about the physics of collision processes. In particular, all that was observable were the inputs and outputs of collision processes, observed when the particles are far enough apart in spacetime to be non-interacting, or free. This back box approach to physics was very much inspired by the Copenhagen philosophy. The dispersion relation approach to physics tried to construct physical theories on the basis of a few central physical axioms, such as unitarity (conservation of probabilities), Lorentz invariance, and causality (effects can't precede causes). These two approaches were combined, by Geoff Chew amongst others, so that the focus was on the analytic properties of the S-matrix. One model for the S-matrix, incorporating some other principles thought to be involved in strong interaction physics, was the Veneziano model. This used the Euler beta function to encode the various desirable properties of the S-matrix. The model was found to be generated by a dynamical theory of strings. (See [16] for a detailed historico-philosophical account of the early development of string theory.)

4.4 Discretization

A development of the cutoff idea, and the idea that there might be a minimal (fundamental) length, leads quite naturally into the idea that space and time might not be continuous, but better modelled instead by a discrete lattice or similar structure. This was suggested by several people. In a paper from 1930 Ambarzumian and Iwanenko [1] argued for the introduction of a spatial lattice structure for physical space as a way of eliminating the infinite divergences from the self-energy of the electron. The basic idea was that the existence of a minimal length would imply a maximal frequency (p. 567).[43]

Arthur Schild [87] investigated the properties of such a discrete lattice in order to see if it would break essential symmetries. In particular, he was responding to the objection that discrete theories would violate Lorentz invariance.[44] He wasn't able to devise a model to preserved all such symmetries, but enough to provide a plausible candidate for a background for a physical theory.

van Dantzig [18; 19] was motivated by a combination of general covariance (as expressed in the Point-coincidence argument) and the definition of observability in such a theory. van Dantzig argued that in a generally covariant theory the observable things will be coincidences: events. van Dantzig argues that in order to not introduce unmeasurable structure into the interpretation or formulation of one's theory, one should dispense with the existence of a four-dimensional continuum, in favour of a discrete manifold of events.[45]

Bergmann describes the approach as one of "constructing 'spaces' that have certain topological properties similar to those of point spaces in the large but do not possess 'points' as elementary constituents" (Bergmann, following Wigner's talk: [108], p. 226). The general approach lives on in several of the current approaches, including causal set theory and dynamical triangulations.

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[43] I might add that this rich background of work on the mixture of geometry and quantum theory provides a nice background out of which Matvei Bronstein's work emerged—see [44].

[44] Rafael Sorkin would later defend the causal set approach from the same charge.

[45] I should point out that van Dantzig steers clear of positivism. He notes that "it is not sufficient to take only observed events; we have to add to these also possibly observable, hence fictitious events" (Comments after Wigner's talk: [108], p. 224). I take it by 'fictitious' he means counterfactual: i.e. they *could* be observed.

4.5 *General Relativisation of the Dirac Equation*

As with the electrodynamical analogy that led to a flow of ideas from quantum field theory to general relativity; so (at around the same time, though somewhat earlier) there was a 'geometrical analogy', responsible for a flow from general relativity to quantum theory. Following Einstein's early remarks about the need for some kind of relationship between gravitation and the quantum theory, much of the work in the field of quantum gravity (up until the 30s) concentrated on bringing the quantum theory in a form conducive to integration with (classical) general relativity.

George Temple argued for a modification of the Dirac equation (of the electron) on pain of "abandoning the theory of relativity" ([95], p. 352). Temple's approach was to construct a system of wave equations which possessed "all the advantages as Dirac's equations and which shall be tensorial in form in accordance with the general theory of relativity" (ibid.). In a slightly different way, both Fock and Weyl also attempted to merge the Dirac equation with the geometry of general relativity. Their strategy (discovered independently) was to modify the structure of the manifold so as to allow for spin—by adding (local) spinor structures. Fock desired (and thought he'd achieved) a "geometrization of Dirac's theory of the electron and its subsumption within general relativity" ([40], p. 275). Together with Iwanenko, they labeled their theory "quantum linear geometry". The basic idea was to modify the geometry in such a way as to include the properties of the Dirac matrices. They suggest introducing a linear differential form $ds = \Sigma\gamma_\nu dx_\nu$ that when squared would deliver the standard Riemann interval ds^2.

Fock believed that this approach could lead to solutions of the most pressing problems in quantum field theory at that time (negative-energy solutions and the ubiquitous divergences). Though he devised a near identical theory to Fock, Weyl would later distance himself from the geometrization programme. See [88] for a nice account of this episode.[46]

The mathematician Dirk Struik was invited to MIT by Norbert Wiener. Struik had experience in the mathematics of general relativity, and had assisted in the development of parts of the differential geometric and group theoretic aspects. Wiener had met Struik on a visit to Göttingen—*cf.* [84], p. 23. Together they worked on a unified theory of general relativity, electromagnetism, and quantum theory. The methodology was as above: to subsume quantum theory (in this case, specifically Schrödinger's wave mechanics) within general relativity.

Wigner, perhaps more than anyone else (save Weyl), recognized the importance of symmetry in quantum theory. Rather than focusing specifically on making the Dirac equation generally relativistic, Wigner adopted the method of showing how quantum mechanics itself was not really in any conflict with what he saw as the two basic principles of general relativity; namely that "coordinates have no indepen-

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[46] The general relativization of other physical systems continued for some time. John Wheeler ran a seminar on Dirac's Equation in general relativity as part of his course on advanced quantum mechanics (in 1955)—this was transcribed by Charles Misner.

dent meaning" and "that only coincidences in space-time can be observed directly and only these should be the subject of physical theory" ([108], p. 219). Wigner appears to have something like Kretschmann's 'point-coincidence' objection to the principle of general covariance in mind when he writes that "[t]his observation is so stringent that, properly considered, every physical theory conforms with it ... this is true also of the present day quantum mechanics" (ibid.).

His approach was to formulate a version of quantum mechanics without coordinates, using just field components. Fred Hoyle was in agreement with Wigner, calling the use of coordinates "a psychological survival from the Newtonian era" ([108], p. 224):

> Now that we realize that coordinates are nonmore than parameters that must be eliminated in determining relations between observables, it becomes natural to ask whether we are using the most advantageous parameters, or even whether any such parameters are necessary. (ibid.)

This introduces another distinction between the approaches: there are those that seek to quantize the gravitational field, and those that seek to general relativize quantum theory. Again, these two broad categories can be found in the current crop of approaches to the problem of quantum gravity.

4.6 Canonical versus Covariant

Two distinct lines were clear very early on: that involving quantizing the full metric, and that of quantizing a perturbation on a flat spacetime. The former 'canonical' approach involves the Hamiltonian formulation of general relativity in which the canonical 'configuration' variable is the spatial metric (on a spatial hypersurface). However, the decomposition of the Einstein equations generates two quite distinct families: six are ordinary evolution equations, but four are constraint equations on the initial data (the metric and its conjugate 'momentum'. This approach was widely believed to suffer a fatal flaw, in the factor ordering problem. However, shifting to a new set of variables (in the 1980s) eradicated the problem.

The first (detailed) flat space quantization approach (to first order) of the gravitational field (nonlinearities and all) was conducted by Suraj Gupta [47; 46]. The approach is distinctively particle physics-based. One begins with the classical Lagrangian and then applies standard field quantization methods to it. This involves the detachment of the gravitational potential $g_{\mu\nu}$ from its geometrical role in general relativity as ordinarily conceived. The particle physics approach is able to 'mimic' the geometrical aspects we normally associate with general relativity by utilization multi-graviton exchange.

In more modern terms, covariant quantization of a gauge theory amounts to the derivation of the Feynman rules for the propagators and the vertexes describing the quanta of the theory. In other words, the approach proceeds by constructing Feynman diagrams. These to hand, one can then ascertain whether the theory is renormalizable at various loops.

The starting point in the covariant approach is a Lagrangian for gravity (with matter or without, giving pure gravity). The Einstein-Hilbert Lagrangian is:

(5) $\mathcal{L} = -\sqrt{g}R$

Now recall that the perturbative approach involves the split into a flat background $\eta_{\mu\nu}$ (Minkowski spacetime) and a deviation from the flat background $h_{\mu\nu}$ (which, when quantized, will represent gravitons):

(6) $g_{\mu\nu} = \eta_{\mu\nu} + h_{\mu\nu}$

If we make this split, then the Lagrangian must be modified accordingly:[47]

(7) $$\mathcal{L} = -\Big(\frac{h_{\alpha\beta,\nu}}{4}\Big)^2 + \Big(\frac{h_{\alpha\alpha,\mu}}{4}\Big)^2 - \Big(\frac{h_{\alpha\alpha,\beta}h_{\beta\mu,\mu}}{2}\Big) + \Big(\frac{h_{\nu\beta,\alpha}h_{\alpha\beta,\nu}}{2}\Big) + \mathcal{O}(h^3)$$

In addition to the problems of non-linearity (from the higher order terms: the $O(h^3$ terms represent interactions, and are given as vertices in the Feynman diagrams) that we met earlier, causing the divergences; there is also the problem of the invariance group of general relativity. While the divergence problem characterises much of the earlier work (certainly the covariant approach), the curious nature of the symmetry group characterises the later work (especially in the canonical approach). Just as the symmetry in the equations of Maxwellian electromagnetism involves gauge freedom, leading to redundancy in the space of solutions (to be removed by choosing gauge invariant quantities, or fixing a gauge) so to does general relativity. In this case it is invariance under (infinitesimal) coordinate transformations:

(8) $h_{\mu\nu} = h_{\mu\nu} + \xi_{\mu;\nu} + \xi_{\nu;\mu} + \mathcal{O}(\xi_\mu^2)$

Here $\xi_\mu(x)$ encodes the gauge freedom: one can choose arbitrary values without changing the physical state (the solution: $h_{\mu\nu}$). One must deal with this gauge freedom somehow. The standard method is to impose a gauge condition on the fields. One can then proceed to solve the Euler-Lagrange equations. The classical (or 'tree level') case involves the computation of the scattering of classical gravitons via the vertices and the propagators. The quantum case builds in closed loops to the tree diagrams. The Feynman diagrams describe the possibilities of breaking apart, forming loops, and joining together. Carrying out this procedure led Feynman to the discovery of 'ghosts', a compensatory (unphysical) field needed purely to render quantum Yang-Mills field theories consistent.[48]

There are several ways to achieve a covariant approach. Bryce DeWitt used Rudolph Peierls (coordinate independent) version of the Poisson bracket to define the commutator in terms of Green's functions. DeWitt was able to quantize

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[47] For simplicity, I have written this out under the assumption that the Wick rotation has been performed, taking $\eta_{\mu\nu}$ to $\delta_{\mu\nu}$ with positive signature (+,+,+,+).

[48] In 1974 't Hooft and Veltman were able to show that Einstein's theory of gravity (without matter and in 4D) was finite at one loop; however, adding matter to the theory destroyed this [97]. In 1985 Goroff and Sagnotti later did the two loop computation and found that the theory was non-renormalizable [45].

the gravitational field without restricting himself to flat spacetime. The method, known as the 'background field method', invoked physical degrees of freedom (in this case, a stiff elastic medium, like a physical ether, and a field of clocks) to localize points, and there allow for the localization of physical quantities. The gravitational field is taken to interact with this background field.[49] Computationally, the approach marked a great advance. However, the background is unphysical in most realistic cases—but this highlights DeWitt's (much like Feynman, and other physicists with war experience) focus on getting the job done, and computing numbers, over conceptual issues.

The canonical (Hamiltonian) approach was pursued in slightly different ways by several schools. Bergmann was the first to apply canonical quantization methods to non-linear covariant field theories [6]. The Hamiltonian formulation of classical general relativity was perfected by Dirac, in 1958 [30].[50] The basic idea is expressed by DeWitt as follows:

> A canonical theory looks at spacetime as a sequence of 3-dimensional slices, each characterized by its intrinsic 3-geometry. The slicing is, of course, not unique. However, if two 3-geometries are chosen, one may try to solve the 'Sandwich Problem': find those spacetimes (4-geometries) which can have these 3-geometries as slices. ([25], p. 186)

The four dimensional spacetime (diffeomorphism) symmetry of general relativity is clearly broken (at least superficially) in this approach. The symmetry is canonically rendered using four constraint functions on the chosen spatial manifold, a scalar field (known as the scalar constraint) and a vector field (known as the diffeomorphism constraint). These have the effect, respectively, of pushing data on the slice onto another nearby (infinitesimally close) slice and shifting data tangentially to the slice. In a canonical approach (to field theories) one writes theories in terms of fields and their momenta. Spacetime covariant tensors are split apart into spatial (tangential) and temporal (normal) components. This naturally obscures general covariance, but the theory is generally covariant despite surface appearances.

The general covariance of the Einstein equations, reflecting the spacetime diffeomorphism invariance of the theory, is encoded in constraints.[51] Taken together, when satisfied, these constraints are taken to reflect spacetime diffeomorphism invariance; together they tell us that the geometry of spacetime is not affected by the

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[49] It is possible that this approach was developed by DeWitt using material he'd had to master for his work in higher-dimensional Lagrangian hydrodynamics, in which one has to consider a mesh that forms a dynamical background for the materials one is studying.

[50] Donald Salisbury has investigated the early history of the canonical quantization formalism, with its introduction of 'constraints' (on which see below): [85; 86]. He traces crucial details back to Rosenfeld. However, he entirely omits from his story equally crucial work by Paul Weiss (a student of Max Born and Paul Dirac): [106; 107].

[51] Hamiltonian constraints, in general relativity, constitute an infinite set of relations holding between the canonical variables of the theory (the spatial metric and its conjugate: $(g_{\mu\nu}, p^{\mu\nu})$). Any choice of physical variables must satisfy these constraints on some initial hypersurface. The constraints are taken to generate (infinitesimal) coordinate transformations of the initial hypersurface. Since the theory is independent of coordinate transformations (on pain of underdetermination), these are taken to constitute gauge transformations. Dirac developed a general framework for such constrained systems, classically and quantum mechanically.

action of the diffeomorphisms they generate. This job is done by two, of course, since the diffeomorphism constraint deals with aspects of the spatial geometry and the Hamiltonian constraint deals with aspects of time. Imposing both delivers the desired full spacetime diffeomorphism invariance.

One then quantizes the theory, including the quantization of the constraints, so that quantum states are annihilated by the full Hamiltonian constraint (a combination of the scalar and diffeomorphism constraints):

$$\hat{H}\Psi[g] = 0 \tag{9}$$

This equation (known as the Wheeler-DeWitt equation) contains all of the dynamics in quantum geometrodynamics. The quantum states (the wave-functionals Ψ) depend only on the 3-metric, not on time—hence, the solutions represent stationary wavefunctions. In fact, the diffeomorphism constraint implies that the quantum states are only dependent on the 3-geometry rather than the metric—that is to say, the states are invariant under diffeomorphisms of Σ thanks to the diffeomorphism constraint. However, the geometrodynamical (so expressed) approach ran out of steam due to technical difficulties. New variables based on a canonical transformation of the phase space of general relativity led to a more tractable formulation.

From the earliest phase of quantum gravity research to the present day, their is a vast array of approaches, most highly distinct. They are constrained by much the same factors: the facts we know about the world already, basic physical principles, and mathematical consistency. However, analogy and simplification also play a crucial role. The methods of simplification, and the analogies chosen are determined in large part (and quite naturally so) by the traditions of the practitioners: by the tools they have to hand and by the range of theories that they are acquainted with. This has elements of both Andy Pickering's and Peter Galison's approaches to the development of theory. I'll draw some of these points of contact out in the next sections.

5

METHODOLOGICAL LESSONS

The historical review presented above is incomplete, and rather rough. However, it contains elements that ought to be of interest to philosophers, historians, and sociologists. QG research shows quite clearly many cases in which none of the standard predictivist models of scientific evaluation are operating. Novel predictions are not an issue; certainly not in the earliest research. The pragmatic argument was trundled out time and again to show that novel testable predictions were out of the question, but research continued in spite of this consensus.[52] Yet the fact that 'casualties' have occurred shows that evaluation *is* nonetheless in operation. It also

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[52] There are, on the other hand, approaches which are inconsistent with old evidence. For these, we can at least adopt some empirical criterion: accommodationism—though as a *criterion* it is still applicable only to some episodes; other cases will violate it if an approach has other things going for it.

reveals, even in the earliest work, a great diversity in competing approaches, many of which are empirically indistinguishable (i.e. the canonical versus covariant approaches) yet conceptually quite distinct and with their own distinct set of internal problems. As we saw with Stachel's example of the geometric vs spin-2 field representation of gravity, this can lead to genuine underdetermination: the geometric understanding of general relativity that prevails might easily have been replaced by a very different highly non-geometric one. The reasons are not experimental or observational: it is more a matter of timing—*cf.* [17].

The 'specialness' of quantum gravity, and the challenge it poses for both scientists and philosophers, sociologists, and historians, is that there is no *direct* empirical reason for thinking that the gravitational field exhibits quantum-like behaviour. Nor is there any consensus on the 'correct' theory. If there were, the various other approaches we have looked at would have been closed up and marked as at fault. It is this open situation that renders the close inspection of quantum gravity's history a pressing task.

In what follows I make some initial headway on this task by comparing the two strongest (historicist) methodologies with the development of quantum gravity. I will then argue that a large part of the development of quantum gravity involves tool-based reasons (in addition to more obvious factors such as the appeal to unification).

5.1 Lakatos Meets Quantum Gravity

How does the development of quantum gravity match up to Lakatos' methodology of scientific research programmes? Not so well, in some ways; but quite well in others. According to this methodological model, fledgling research programmes should not initially be rejected as a result of anomalies or inconsistencies, if the theory is progressing well in other ways—this was, of course, implemented to take care of the various problems cases facing Popper's more stringent methodological model. However, the central principle guiding the construction of quantum gravity directed research programmes are precisely mathematical consistency and the absence of anomalies (and the ability to be consistent with known, already observed data). Moreover, the empirical detachment of quantum gravity research means that the leniency that Lakatos' methodology allows for (namely, when there is empirical progress in general) can not easily be applied here. Lakatos' model seems inapplicable to quantum gravity.

That being said, there are ways of making sense of the notion of an auxiliary belt of assumptions, used to protect a programme's hard core (describing the central concepts and laws of the programme), in the case of quantum gravity. And there are examples in which the modifications made to rescue a quantum gravity programme from inconsistency and anomaly are not "blatantly *ad hoc*" (to use a phrase of Cushing's—[15], p. 78). In other words, we can find cases where the addition of an auxiliary assumption to solve one problem (and so, a blatantly *ad hoc* move) has uses beyond the reason it was initially introduced. The most obvious examples of this kind come from superstring theory, which I have ignored up until now but will briefly discuss since it has been the subject of some philosophical discussion recently.

In their recent Lakatosian-inspired assessment of superstring theory, Nancy Cartwright and Roman Frigg draw attention to the broad range of possible factors (other than testability) that can play a role in our evaluation of theories: ability to unify, beauty, simplicity, etc. They also explain how string theory performs well in some of these other "dimensions". But, ultimately, they come down negatively on the scientific status of string theory (and, by extension, quantum gravity research in general):

> [A] research programme that progresses only in some dimensions, while being by and large stagnant in the others, surely does not count as being progressive. Contrasting string theory with Maxwell's unification of electricity and magnetism, for example, we can see that the latter was genuinely progressing and eventually successful in every dimension. It used the new and powerful concept of a field, which made the theory simple and elegant, while at the same time giving rise to a whole set of new phenomena that led to new predictions. ([12], p. 15)

This seems somewhat arbitrary and overly dogmatic. After pointing out several ways in which string theory *is* progressive, they claim that, nonetheless, the theory is in fact degenerative (or stagnant) overall! Yet what (non-question-begging) basis is there for ranking the dimensions? In the final analysis, Cartwright and Frigg defend, more or less, a traditional view of scientific method:

> The question of how progressive string theory is then becomes one of truth, and this brings us back to predictions. The more numerous, varied, precise and novel a theory's successful predictions are, the more confidence we can have that the theory is true, or at least approximately true (see box). That a theory describes the world correctly wherever we have checked provides good reason to expect that it will describe the world correctly where we have not checked. String theory's failure to make testable predictions therefore leaves us with little reason to believe that it gives us a true picture.
>
> Although string theory has progressed along the dimensions of unifying and explanatory power, this in itself is not sufficient to believe that it gives us a true picture of the world. Hence, as it stands, string theory is not yet progressive because it has made progress only along a few of the many dimensions that matter to a research programme's success. (ibid.)

Cartwright and Frigg slide here from the evaluation of theories (whether it is rational to pursue them), to talk of truth. But while the various virtues exhibited by string theory might not warrant belief in its absolute truth, they might yet warrant an increase in the credibility of the theory. That is, they can make it perfectly *rational* to pursue string theory (and quantum gravity) in spite of the lack of direct experimental support.

From the Lakatosian perspective—according to which research programmes that are able to make novel predictions are considered progressive and those that don't are considered degenerative—it is not enough to fit a body of evidence, however varied and variegated that body might be. But this tags as degenerative pretty much all quantum gravity research. Each approach we looked at above was completely detached from experiment[53]; any novel predictions they might make will

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[53] Save, perhaps, Schild's analysis of the symmetry violation by discrete spacetimes, which have recently led on to potential phenomenological applications.

be (most likely, forever) untestable by any direct means. Hence, if the Lakatosian approach has this implication, then the historical record suggests that the approach itself is at fault: it is too restrictive.

And indeed, it simply doesn't account for the comings and goings of the approaches we looked at. Mathematical consistency (i.e. freedom from 'bad' divergences, consistent union of general relativity and quantum theory, etc.) and the ability to accommodate the empirical successes of the ingredient theories (general relativity and quantum theory) were paramount. How well an approach managed to perform with respect to these desiderata determines the strength of support. It is also clear that often an approach is pursued simply because the tools are available to pursue it. I have in mind here the particle physics approach to general relativity, which involved using skills that had not been previously applied in this area. Both note too that although these considerations are used to guide the construction and evaluation of the various approaches, no one of them is definitive. Often, even when there were severe consistency problems or even mismatches with old evidence, a approach continued to be pursued.

5.2 Kuhn Meets Quantum Gravity

Kuhn's model for the structure of scientific theory development is often taken to be a more accurate description of actual events. Certain elements of his analysis of revolutions in science seem to provide a fitting model for quantum gravity. For instance, Kuhn writes:

> Confronted with anomaly or with crisis, scientists take a different attitude toward existing paradigms, and the nature of their research changes accordingly. The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and the debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research. ([65], pp. 9091)

However, Audretsch [3] has argued that Kuhn's approach flounders when it tries to deal with the realities of quantum gravity.[54] Firstly he sets up the problem of quantum gravity as one involving a pair of "incompatible paradigms", i.e. rather than merely incommensurable paradigms which Kuhn allows. Moreover, argues Audretsch, both of these paradigms involve an "all claim".

I think this exposes an error in Audretsch's formulation of the problem for Kuhn, for neither general relativity nor quantum theory have an all-claim built into them of necessity. And indeed there are various problems of principle—such as the problem of explaining the final stages of black hole entropy, the small value of the cosmological constant, the conditions at the beginning of the universe and within black holes, and so on—which strongly suggest that neither has sufficient warrant to make such an all-claim without some serious revisions being made.

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[54] Diane Crane [13] conducted a sociological analysis of theoretical high-energy physics (between 1960-75), and also found it at odds with Kuhn's model. In my view, Crane chose far too short a time span. Had she continued, certain of her conclusions would have been altered (such as the demise of string theory in the early 70s as a result of a failure of experimental testability).

Audretsch also makes the claim that the paradigms of general relativity and quantum field theory do not show any anomalies (p. 332). This is overly simplistic. While there is no experimental result like the perihelion of mercury that renders general relativity problematic, there are observable factors. For example, the value that quantum field theory gives to the energy density of a vacuum (the cosmological constant) is very many orders of magnitude too large. Audretsch does mention the prediction of singularities by general relativity, but dismisses this as a problem since they are "hidden behind horizons" (p. 333), and therefore not empirically observable. It is certainly true that there are no 'naked singularities' in general relativity, and therefore that one would not be measurable. However, the 'cosmic censorship hypothesis' outlawing naked singularities might not be true in quantum gravity theories. Furthermore, it seems like a step back to positivism to bracket worrying about a direct theoretical prediction on the grounds that we couldn't measure it.

One more immediate problem with the comparison of quantum gravity research to Kuhn's methodological model is that was nothing corresponding to paradigms in the early work. Nobody pursued a single programme for long enough. The approaches didn't stay fixed for long enough. There was no period of unquestioning study and application of any of the approaches. In this very simple sense, Kuhn's model is inapplicable. This is a problem, for if Kuhn's model cannot account for 80 years of scientific development then what good is it?

One possible response is that I have largely ignored the present day research landscape, and this *can* be described as consisting of research traditions of a sort, stemming from the particle physics and geometrical schools. Once one had fairly complete frameworks for quantum gravity, in the canonical and covariant formulations, then physicists quickly began to focus all their attention on one or the other, but not both: they *chose sides*. As I have mentioned, and will discuss further below, the choice was not based on empirical factors, but on their training and mentorship. Kuhn argued that traditions of scientific research "spring" from paradigms. If we have these schools and traditions, then perhaps we have underlying paradigms? The problem with this view is that there is no equivalence proof connecting the covariant and canonical formalism in the case of quantum gravity, so we would have two separate paradigms dealing with the same subject. String theory adds to the complications, though it might be held separate from 'pure' quantum gravity research since it aims to be a unified theory of all four forces, rather than just gravity.

Hence, there are some serious problems that need to be addressed by Kuhnians if they are to capture the development of quantum gravity research. However, there are elements of Kuhn's philosophy that seem to fit much better, particularly those having to do with the role of pedagogy.

Before I discuss these aspects, I should first like to say something about Cushing's view of the driving forces behind scientific change. I do this since there are points of similarity to the episodes Cushing discusses, namely S-matrix theory versus quantum field theory on the one hand and the Copenhagen interpretation versus Bohmian mechanics on the other. In the quantum gravity case we have a split between a field theoretic approach, according to which gravity is mediated by exchange particles just like any other force, and an approach whereby gravity is a manifestation of curved geometry. As with Cushing's examples (or at least as he argues), we have underdetermination here: there is no empirical basis that could guide selection or rejection of one over the other. And yet physicists do make decisions about which to endorse, and they stick to their decisions.

James Cushing [15] has argued that, given the lack of an invariant methodological principle that guides theory selection and rejection, a better explanation for the convergence of opinion of scientists is the existence of a "pyramid structure" in which a handful of highly creative individuals occupy the apex, serving to generate new concepts, methods, and theories (ibid., p. 3). Quantum gravity seems to strike a blow against Cushing's idea of a pyramid structure to science. The best physicists all tried their hands at the theory without success, and no single theory was pursued as a result of this. Instead there was a massive proliferation of distinct lines, not always generated by 'great men'. Indeed, the approaches of the great men were often ignored.

However, while Cushing's scheme was suggested as a way of understanding why a particular line of research is pursued, it can just as easily be utilized to show why a particular approach or theory *wasn't* pursued. The failure to produce a consistent theory along some research line by *so many* great men (sitting at Cushing's apex) was a signal to the rest of the community to steer clear of quantum gravity. To see even such a figure as Pauli both adamant about the necessity of constructing a theory of quantum gravity, yet at the same time daunted by the magnitude of the technical and conceptual hurdles would certainly put the frighteners on many young physicists.[55] This might account for some of the appearance of the history of quantum gravity; however, I think more can be said. I mention two potential lines of inquiry (of an &HPS nature) that can be brought to bear on the history of quantum gravity.

6

THE RIGHT JOB FOR THE TOOLS

Very early on in the history of quantum gravity there emerged distinct paths that were ploughed as a result of the differing backgrounds of those doing to ploughing. With different sets of tools come different points of interest and different research questions. For example, in the canonical (Hamiltonian) quantization approach one is concerned with such global features as the wave-function of the universe, and the domain space of such a wave-function. The construction and understanding of the configuration space is a largely classical problem. By contrast, the covari-

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[55] I use the example of Pauli since DeWitt on mentioning to Pauli that he intended to work on quantum gravity for his PhD received the reply (delivered with a repeated nod and shake of the head): "that's an important problem, but it'll take someone really smart" ([98], p. 57).

ant approach is more ‘local’, focusing on graviton scattering and other typically quantum field theoretic quantities (*cf.* [24], p. 1239). These are entirely different ways of approaching a subject, with distinct commitments, but without empirical distinction.

David Kaiser notes how, in his lectures at Caltech, Feynman introduced general relativity for “physics students ... who know about quantum theory and mesons and the fundamental particles, which were unknown in Einstein’s day” (Feynman’s *Lectures on Gravitation*; cited in [59], p. 329). We have seen that the particle physicist’s approach is conceptually very different from Einstein’s own geometrical approach. Feynman, though not the first to write general relativity in this way, sought to tailor the presentation of the theory to the needs of his students.[56] The approach was designed to mesh with the modern particle physicist’s ‘multi-field’ mindset. With the profusion of new particle types thrown up by the latest generation of particle accelerators, students were used to thinking about large numbers of fields (associated with the particles). Feynman’s approach was to treat “the phenomena of gravitation” as the addition of “another field to the pot” (ibid.); as nothing special, much as many of the very earliest researchers had done. Those trained in the ‘geometrical way’ naturally baulk at this particle physics approach. The divisions between the two ways of approaching gravity are felt strongly by the two sides—one can find the genuine animosity involved in the division in today’s debate over background independent versus background dependent approaches (see [90] for a discussion of this debate). [57]

There are several interesting institutional changes that enter in to this argument, that trigger several phases of research inquiry. The earliest workers on quantum gravity had strong backgrounds in both general relativity and quantum theory, and had made strides in both, and often had written textbooks in both fields. But as quantum theory became more complex, and engaged with experiments more, it occupied more and more of physicists’ attentions. It had a larger share of mathematical and conceptual problems. General relativity was, from very early on, seen

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[56] Recall, as I said earlier, that there is not unlimited freedom in how one can do this: the principles going into the theory, together with the observation data it would need to account for, constrain the form of the theory very tightly indeed.

[57] David Kaiser has argued in a similar way that particle cosmology (roughly: general relativity combined with elementary particle physics) emerged in the 1980s for external reasons. In particular the cold war bubble burst resulting in a cessation of particle physics funding opportunities. The upshot of this is that there are a bunch of physicists who need to get their funding from somewhere.[58] This predicament forces the ploughing of new research avenues. In this case the hybridization of cosmology and particle physics. The joining of these subjects is not entirely accidental: cosmology involves (in certain areas) extreme high-energy phenomena, of just the kind that could function as a laboratory for particle physicists![59] Moreover, the two fields had some history together: the union is not entirely novel. However, it was certainly professionalized in the period Kaiser studies. Background conditions (external factors) can, then, quite clearly be seen to impact institutions which in turn impact scientific thought. I mention this case study since it impacts directly on the development of quantum gravity. Cosmological scenarios could be exploited to generate phenomenology for quantum gravity theorists. Not only this, the interaction between particle physics and cosmology resulted in the training of a generation of new scientists with strong skills in both quantum field theory and general relativity.

to be essentially finished. Hence, the physicists' interests and toolkits shifted more and more into quantum theory, and this shift can be seen in the evolution of approaches to the quantum gravity problem. It wasn't really until John Wheeler and Peter Bergmann began working on quantum gravity in the 1950s, and gathered high quality students around themselves that the geometrical approaches began to make their mark once again. But by this stage there were very strong disciplinary splits, marked out by the existence of distinctive 'schools'.

Of course, as I said above, though these institutional factors contribute a great deal to the nature of the research that is done in quantum gravity, and can go a long way in explaining why certain physicists hold some particular approach, there is not much latitude in the possibilities that theories can take. The available tools are one constraint among many others. I finish by saying something about the notion of constraints, and the role they play in establishing scientific beliefs, and accounting for changes in science. I am particularly concerned with finding some kind of constraint that can play a role functionally similar to that played by experiments in guiding theoretical developments.

7

CONVERGENCE AND CONSTRAINTS

Ian Hacking notes that the convergence on some feature or result (of a theory) can prove very convincing (in evaluative terms) in cases where the convergence comes about through quite different instruments and experiments, using quite distinct physical principles:

> we are convinced because instruments using entirely different physical principles lead us to observe pretty much the same structures in the same specimen. ([49], p. 209)

If we keep finding some similar behaviour in a wide variety of conditions, then we are prone to believe that the behaviour is a universal feature, not some artefact of a model or experiment. Certain results crop up in multiple formalisms and in the context of quite distinct investigations in quantum gravity research. For example, the divergences, or in later work, the existence of quantum geometry.

This is especially important in the absence of experiments. One can usefully view this through the lens of Peter Galison's notion of "constraints" [41]. Constraints are very much the life-blood of science. They minimize the latitude one has in theory construction. The satisfaction constraints can in itself act as an evaluative measure. In the absence of experiments and observation, new kinds of constraints must come to the fore, to guide theorizing.[60] The black hole entropy value I mentioned earlier functions, in some sense, as a constraint almost (but not quite)

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[60] The classic tests of GR functioned as constraints on the early approaches in a more standard way. What is more interesting is where we have approaches that match up on all pre-existing knowledge, and don't make any novel testable predictions either.

like an experimental constraint. The new approaches are tested against this constraint. It thus provides new material for constructing theories, or working out the possibilities of old theories. Renormalizability too acted as a crucial constraint in the post-WWII years. Weinberg describes the importance of this constraint:

> [I]t seemed to me to be a wonderful thing that very few quantum field theories are renormalizable. Limitations of this sort are, after all, what we most *want*; not mathematical methods which can make sense out of an infinite variety of physically irrelevant theories, but methods which carry constraints, because these constraints may point the way towards the one true theory. In particular, I was very impressed by the fact that [QED] could in a sense be *derived* from symmetry principles and the constraint of renormalizability; the only Lorentz invariant and gauge invariant renormalizable Lagrangian for photons and electrons is precisely the original Dirac Lagrangian. ([104], p. 1213; cited in [41], p. 22)

The original perturbative approach was rejected because it conflicted with this constraint. By contrast, string theory was given credence because it offered the prospect of a finite theory. However, it was then found to violate the constraint that there be no quantum anomalies in the theory (i.e. symmetries that are in the classical theory but broken at the quantum level). The subsequent evasion of this constraint provided almost as significant a degree of motivation for renewed interest in the theory as a successful experiment.

However, as with experiments, we shouldn't place too much weight on them: they are rarely decisive. As I mentioned in the string theory case; these constraints can lead one to drop a theory prematurely, only to be found at a later date to satisfy it. I mentioned earlier in fn.9 that the lessons of the Bohr-Rosenfeld analysis of measurability of the electromagnetic field were taken to transfer over to the gravitational case (yet another example of the analogical reasoning so prevalent in quantum gravity research).[61] The idea was that the gravitational field would *necessarily* have to be quantized if it were coupled to another quantized field, or to quantized matter. This belief spurred on physicists in the early days. However, in the 1957 Chapel Hill conference (*On the Role of Gravitation in Physics*), Rosenfeld argues that the analysis he performed with Bohr does not translate into the gravitational case. The crucial dis-analogy is that one cannot (even theoretically) find a measuring instrument that would not generate perturbations in the measurement result: this is due to the equivalence principle. In the electromagnetic case the fact that there are both positive and negative charges allows one to control the perturbations. The electromagnetic field can be shielded.

This supposed necessity (suggested by the thought experiment) previously functioned as a constraint on quantum gravitational theorizing. However, John Wheeler was willing to suggest (following Rosenfeld's remarks) that perhaps the measurement problem for quantum gravity could be ignored for the present and than one place more emphasis on "the organic unity of nature" as a key constraint ([105], p. 83). Hence, though the development of theory demands constraints to guide it at any one time; the constraints it makes use of don't have to be constant.

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[61] Note that such analogies can themselves be interpreted as constraints, for one is essentially making a claim that two systems are sufficiently similar so that the (well-known) constraints that apply to one will most likely apply to the other.

In the very earliest approaches, there was no quantum field theory available, so what would become an important constraint (renormalizability) was absent. Once QED was constructed, however, one could get a handle on computable aspects of quantum gravity, and compare them to what had been done in QED. Rosenfeld's computation of the self-energy of the graviton was such an example. The development of renormalisability led to an easily applicable criterion to decide whether a theory was worth pursuing. Interestingly, the constraint of renormalizability played a lesser role once the tools of renormalization *group* theory had been assimilated.

General invariance became a constraint itself. Other important constraints include unitarity (probability conservation), Lorentz invariance, and causality. These combined in an interesting way (with known resonance data from particle collision experiments) to lead to string theory in the late 1960s. Taken together, these principles can home in on a very small number of possible candidate theories. Indeed, for a long time it was believed that they could work in tandem to produce a *unique* theory, though this view is less popular today. Whether the constraints can force uniqueness or not, it is true that they reduce the freedom one has in theory construction, and this is crucial, for without them one would have infinite freedom! A new field known as quantum gravity phenomenology, currently in the early stages, is developing in order to provide additional data to further constraint the possible theories of quantum gravity.

The notion of constraints seems to offer some promise in exposing the innards of the black box I began with, in a way that might be conducive to philosophers. The constraints are at work on both constructive and evaluative levels. The interest to sociologists enters through the fact that different communities are determined by their different trainings (with different toolkits), and this difference spills over into a difference over what constraints ought to be respected (renormalizability versus general invariance, for example). Only a close investigation of the historical details can reveal which constraints guided some particular theory choice. The constraints will often be sociological as well as mathematical and empirical.

8
CONCLUSION

Quantum gravity research constitutes an ideal and novel historical episode that should appeal to historians, philosophers, and sociologists of science alike. The absence of possible experiments and experimental anomalies that usually drive the development of the field expose an entirely different set of inner workings than we are used to seeing in science. One can see how a range of virtues (such as unification, beauty, and so on) beyond 'the usual suspects' can guide both the construction and justification of theories. One also sees the strong role played by analogies, which continued to be pursued despite the knowledge that the analogy was far from perfect. Methodologically, what the development of quantum gravity reveals is that what is deemed appropriate will depend upon what constraints are available at the time, and this is prone to changes of a great variety of sorts.

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