Foreword to Feynman Lectures on Gravitation

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During the 1962–63 academic year, Richard Feynman taught a course at Caltech on gravitation. Taking an untraditional approach to the subject, Feynman aimed the course at advanced graduate students and postdoctoral fellows who were familiar with the methods of relativistic quantum field theory—in particular, with Feynman-diagram perturbation theory in quantum electrodynamics. Two of the postdocs, Fernando B. Morinigo and William G. Wagner, wrote up notes for the course. These were typed, and copies were sold in the Caltech bookstore for many years.

The notes were not published, but they were widely distributed, and because of their unique insights into the foundations of physics, they had a significant influence on many of the people who read them. Morinigo and Wagner performed a great service by preserving so well this piece of Feynman's legacy. Now, thanks to the efforts of Brian Hatfield, the lectures are finally being published in this volume, and so are becoming readily available to a broader audience and to posterity. In preparing the notes for publication, Hatfield has corrected minor errors and improved the notation, but otherwise has adhered to the original typescript prepared by Morinigo and Wagner. (Only two brief passages were deleted.[1])

Feynman delivered 27 lectures in all, one each week over the full 1962–63 academic year. The class met in a tiny room with just two rows of seats, on the third floor of Caltech's East Bridge Laboratory; no more than 15 people attended a typical lecture. (At least two of the students attending, James Bardeen and James Hartle, later made significant contributions of their own to gravitation theory.) The lectures were taped, but because they were highly informal, Morinigo and Wagner found it necessary to revise the material substantially in order to produce a readable set of notes. For the most part, Wagner worked on the mathematical presentation and Morinigo worked on the prose. The resulting notes were then reviewed by Feynman, he made various corrections and additions, and the notes were distributed to the students. The notes are pervaded by Feynman's spirit and are sprinkled with his jokes, but inevitably his idiosyncratic use of language has been only partially preserved.

[1]The deleted passages are an incorrect expression for the matter field action at the conclusion of §8.7 (the correct expression appears in §10.2), and some incorrect statements about the Newtonian theory of stellar stability in the third paragraph of §14.3.

Only 16 lectures are included in this book; these correspond, roughly, to the first 16 of the 27 that Feynman gave. Morinigo and Wagner prepared notes for all 27 lectures, but by the end of the academic year Feynman had reviewed and corrected only the first 11. Evidently he then became distracted by other projects and never returned to the task of editing the notes. Thus, only the notes for the first 11 lectures were distributed to the students during 1962–63 and were reproduced for sale by the Caltech bookstore in succeeding years.

In July 1971, a new reproduction of the notes was prepared for bookstore distribution, and Feynman authorized the inclusion of the next 5 lectures. The new lectures were prefaced by a disclaimer:

> The wide interest in these Lectures on Gravitation has led to a third reproduction. While this edition was being prepared, Professor Feynman kindly authorized the inclusion of the following five lectures. These lectures were intended to accompany the previous eleven in the 1962–63 notes, but were never satisfactorially (sic) edited and corrected for Professor Feynman to feel that they should be included.
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> These lectures retain their rough form: except for minor errors corrected in transcription, they remain the same as they were eight years ago: Professor Feynman has not checked them. It is hoped that the reader will bear this in mind and see the following lectures as an account of what Professor Feynman was thinking at that time, rather than as an authoritative exposition of his work.

It seems true, indeed, that Feynman did not check the new lectures in detail. For example, Lecture 14 contains incorrect statements (discussed below) that he would have easily recognized as incorrect in 1971 (or even within a few weeks after giving the lecture) had he checked them. Thus, we urge readers to keep the above disclaimer in mind when reading Lectures 12–16.

Because Feynman never authorized the distribution of the Morinigo-Wagner notes for the last 11 lectures, they are omitted from this volume. These later lectures were mostly concerned with radiative corrections in quantum gravity and in Yang-Mills theory. We assume that Feynman did not want them distributed because he was dissatisfied with their content.

It is remarkable that concurrently with this course on gravitation, Feynman was also creating and teaching an innovative course in sophomore (second-year undergraduate) physics, a course that would become immortalized as the second and third volumes of *The Feynman Lectures on Physics* [Feyn 63a]. Each Monday Feynman would give his sophomore lecture in the morning and the lecture on gravitation after lunch. Later in the week would follow a second sophomore lecture and a lecture for scientists at Hughes Research Laboratories in Malibu. Besides this teaching load and his own research, Feynman was also serving on a panel to review textbooks for the California State Board of Education, itself a consuming task, as is vividly recounted in *Surely You're Joking, Mr. Feynman* [Feyn 85]. Steven Frautschi, who attended the lectures on gravitation as a young Caltech assistant professor, remembers Feynman later saying that he was "utterly exhausted" by the end of the 1962–63 academic year.

Feynman's course was never intended to be a comprehensive introduction to general relativity, and some of the lectures have become seriously dated. Much of the material in Lectures 7–12 is covered more systematically and in greater detail in other books. Why then, should the lectures be published now? There are at least three good reasons. First, nowhere else is there a comparable pedagogical account of an unusual approach to the foundations of general relativity that was pioneered by Feynman (among others). That approach, presented in lectures 3–6, develops the theory of a massless spin-2 field (the graviton) coupled to the energy-momentum tensor of matter, and demonstrates that the effort to make the theory self-consistent leads inevitably to Einstein's general relativity. It is for this demonstration that the notes have become best known in the physics community. Second, the notes contain a number of fascinating digressions and asides on the foundations of physics and other issues that make them enlightening and interesting to read. Third, the notes have historical value. At the time he taught the course, Feynman had been thinking hard for several years about fundamental problems in gravitation, and it is useful to have a record of his insights and viewpoint at that particular time. Some of his opinions seem prescient to us 32 years later, while others, naturally, seem naive or wrong. In some cases, his views were evolving rapidly as he taught the course. This is particularly true of the material in Lecture 14 on relativistic stars, about which we will say more below.

While these lectures are of special value for what they teach us of Feynman's viewpoints on gravitation, they are *not* a good place for a beginning student to learn the modern geometrical formulation of general relativity, or the computational tools and applications of the theory. Books such as Wald [Wald 84], Schutz [Schu 85] (1985), and Misner, Thorne, and Wheeler [MTW 73] do a much better job of this pedagogical task. Even the dogmatically nongeometrical viewpoint preferred by Feynman is presented more systematically and comprehensively by Weinberg [Wein 72]. But no other source contains Feynman's unique insights and approach to the foundations of the subject.

The lecture notes can be read at several different levels by people of varying backgrounds:

- To understand the lectures fully, readers should have advanced training in theoretical physics. Feynman assumes that his audience is familiar with the methods of quantum field theory, to the extent of knowing how to extract Feynman rules from an action and how to use the rules to calculate tree diagrams. However, these field theory methods are used heavily only in Lectures 2–4 and 16, and even in these chapters the key ideas can be grasped without such training. Moreover, the other lectures can be read more or less independently of these.

- Readers with a solid undergraduate training in physics should find the lectures largely intelligible, thanks to Feynman's pedagogical skill. However, such readers will have to be content with a heuristic grasp of some of the more technical portions.

- For admirers of Feynman who do not have a strong physics background, these lectures may also contain much of value—though to dig it out will require considerable skimming of the technical material that is interspersed with more down to earth insights and passages.

The remainder of this foreword, and a following section by Brian Hatfield, present a synopsis of the lectures and a discussion of how they relate to prior research on gravitation and to subsequent developments.

Derivation of the Einstein field equation:

At the time of these lectures, Feynman was struggling to quantize gravity—that is, to forge a synthesis of general relativity and the fundamental principles of quantum mechanics. Feynman's whole approach to general relativity is shaped by his desire to arrive at a quantum theory of gravitation as straightforwardly as possible. For this purpose, geometrical subtleties seem a distraction; in particular, the conventional geometrical approach to gravitation obscures the telling analogy between gravitation and electrodynamics.

With hindsight, we can arrive at Maxwell's classical electrodynamics by starting with the observation that the photon is a massless spin-1 particle. The form of the quantum theory of a massless spin-1 particle coupled to charged matter is highly constrained by fundamental principles such as Lorentz invariance and conservation of probability. The self-consistent version of the quantum theory—quantum electrodynamics—is governed, in the classical limit, by Maxwell's classical field equations.

Emboldened by this analogy, Feynman views the quantum theory of gravitation as "just another quantum field theory" like quantum electrodynamics. Thus he asks in Lectures 1–6: can we find a sensible quantum field theory describing massless spin-2 quanta (gravitons) coupled to matter, in ordinary flat Minkowski spacetime? The classical limit of such a quantum theory should be governed by Einstein's general relativistic field equation for the classical gravitational field. Therefore, to ascertain the form of the classical theory, Feynman appeals to the features of the quantum theory that must underlie it. Geometrical ideas enter Feynman's discussion only through the "back door," and are developed primarily as technical tools to assist with the task of constructing an acceptable theory. So, for example, the Riemannian curvature tensor (a centerpiece of the conventional formulation of general relativity) is introduced by Feynman initially (§6.4) only as a device for constructing terms in the gravitational action with desired invariance properties. Not until §9.3 does Feynman reveal that the curvature has an interpretation in terms of the parallel transport of a tangent vector on a curved spacetime manifold.

One crucial feature of the quantum theory is that the massless spin-2 graviton has just two helicity states. Thus, the classical gravitational field must also have only two dynamical degrees of freedom. However, the classical field that corresponds to a spin-2 particle is a symmetric tensor $h_{\mu\nu}$ with ten components. Actually, four of these components h_{00}, h_{0i} (with $i = 1, 2, 3$) are nondynamical constrained variables, so we are left with six dynamical components h_{ij} to describe the two physical helicity states. It is because of this mismatch between the number of particle states and the number of field components that the quantum theory of gravitation, and hence also the corresponding classical theory, are highly constrained.

To resolve this counting mismatch, it is necessary for the theory to incorporate a redundancy, so that many different classical field configurations describe the same physical state. In other words, it must be a gauge theory. For the massless spin-2 field, the requisite gauge principle can be shown to be general covariance, which leads to Einstein's theory.

In Lecture 3, Feynman constructs the quadratic action of a massless spin-2 field that is linearly coupled to a conserved energy-momentum tensor. He comments on the gauge invariance of the resulting linear field equation in §3.7, and he remarks in §4.5 that one can infer the nonlinear self-couplings of the field by demanding the gauge invariance of the scattering amplitudes. But he does not carry this program to completion. (He notes that it would be hard to do so.) Instead, he uses a rather different method to arrive at Einstein's nonlinear classical field equation—a method that focuses on *consistency*. Because the linear field equation of the *free* massless spin-2 field necessarily has a gauge invariance (to remove the unwanted helicity states), generic modifications of this field equation (such as the modifications that arise when the spin-2 field is coupled to matter) do not admit any solutions. The new terms in the modified equation must respect a nontrivial *consistency condition*, which is essentially the requirement that the new terms respect the gauge symmetry. This consistency condition is sufficient, when pursued, to point the way toward Einstein's specific set of nonlinear couplings, and his corresponding nonlinear field equation.

In greater detail: the problem, as formulated in §6.2, is to find an action functional $F[h]$ for the spin-2 field $h_{\mu\nu}$ such that the gravitational field equation

$$\frac{\delta F}{\delta h_{\mu\nu}} = T^{\mu\nu} \tag{1}$$

is consistent with the equation of motion satisfied by the matter. Here $T^{\mu\nu}$ is the matter's energy-momentum tensor. Feynman finds a quadratic expression for F in Lecture 3, which yields a consistent linear field equation so long as the matter's energy-momentum is conserved (in the special relativistic sense), ${T^{\mu\nu}}_{,\nu} = 0$. The trouble is that, once the field $h_{\mu\nu}$ is coupled to the matter (so that matter acts as a source for $h_{\mu\nu}$), the equation of motion of the matter is modified by gravitational forces, and ${T^{\mu\nu}}_{,\nu}$ no longer vanishes. Thus, the field equation of $h_{\mu\nu}$ and the equation of motion of the matter are not compatible; the equations admit no simultaneous solutions. This is the consistency problem (of the linear theory).

By demanding that the field equation satisfied by $h_{\mu\nu}$ be compatible with the matter's equation of motion, Feynman infers that higher-order nonlinear corrections must be added to the action, F. The consistency requirement can be cast in the form of an invariance principle satisfied by the action, which is just invariance under general coordinate transformations. After that, Feynman's analysis is fairly conventional, and leads to the conclusion that the most general consistent field equation involving no more than two derivatives is the Einstein equation (with a cosmological constant).

The resulting nonlinear corrections have a pleasing physical interpretation. Without these corrections, gravity does not couple to itself. When nonlinear corrections of the required form are included, the source for the gravitational field (as viewed in flat Minkowski spacetime) is the *total* energy momentum, including the contribution due to the gravitational field itself. In other words, the (strong) principle of equivalence is satisfied. The conservation law obeyed by the energy momentum of the matter becomes Einstein's covariant one, $T^{\mu\nu}{}_{;\nu} = 0$, which in effect allows energy and momentum to be exchanged between matter and gravity.

We know from Feynman's comments at the 1957 Chapel Hill conference [DeWi 57] that by then he had already worked out the calculations described in Lectures 2–6. Murray Gell-Mann reports [Gell 89] that Feynman and he discussed quantum gravity issues during the Christmas break in 1954–55, and that Feynman had already made "considerable progress" by that time.

The claim that the only sensible theory of an interacting massless spin-2 field is essentially general relativity (or is well approximated by general relativity in the limit of low energy) is still often invoked today. (For example, one argues that since superstring theory contains an interacting massless spin-2 particle, it must be a theory of gravity.) In fact, Feynman was not the very first to make such a claim.

The field equation for a free massless spin-2 field was written down by Fierz and Pauli in 1939 [FiPa 39]. Thereafter, the idea of treating Einstein gravity as a theory of a spin-2 field in flat space surfaced occasionally in the literature. As far as we know, however, the first published attempt to *derive* the nonlinear couplings in Einstein's theory in this framework appeared in a 1954 paper by Suraj N. Gupta [Gupt 54]. Gupta noted that the action of the theory must obey a nontrivial consistency condition that is satisfied by general relativity. He did not, however, provide any detailed argument supporting the *uniqueness* of the Einstein field equation.

Roughly speaking, Gupta's argument proceeds as follows: We wish to construct a theory in which the "source" coupled to the massless spin-2 field $h_{\mu\nu}$ is the energy-momentum tensor, *including* the energy-momentum of the spin-2 field itself. If the source is chosen to be the energy-momentum tensor ${}^2T^{\mu\nu}$ of the free field theory (which is quadratic in h), then coupling this source to $h_{\mu\nu}$ induces a cubic term in the Lagrangian. From this cubic term in the Lagrangian, a corresponding cubic term ${}^3T^{\mu\nu}$ in the energy-momentum tensor can be inferred, which is then included in the source. This generates a quartic term ${}^4T^{\mu\nu}$, and so on. This iterative procedure generates an infinite series that can be summed to yield the full nonlinear Einstein equations. Gupta sketched this procedure, but did not actually carry it to completion. The first complete version of the argument (and an especially elegant one) was published by Deser in 1970 [Dese 70].[2] Deser also noted that Yang-Mills theory can be derived by a similar argument.

Some years before Gupta's work, Robert Kraichnan, then an 18-year-old undergraduate at M.I.T., had also studied the problem of deriving general relativity as a consistent theory of a massless spin-2 field in flat space. He described his results in his unpublished 1946–47 Bachelor's thesis [Krai 47]. Kraichnan continued to pursue this problem at the Institute for Advanced Study in 1949–50. He recalls that, though he received some encouragement from Bryce DeWitt, very few of his colleagues supported his efforts. This certainly included Einstein himself, who was appalled by an approach to gravitation that rejected Einstein's own hard-won geometrical insights. Kraichnan did not publish any of his results until 1955 [Krai 55, Krai 56], when he had finally found a derivation that he was satisfied with. Unlike Gupta, Kraichnan did not *assume* that gravity couples to the total energy momentum tensor. Rather, like Feynman, he derived this result as a consequence of the consistency of the field equations. It seems likely that Feynman was completely unaware of the work of Gupta and Kraichnan.

We should point out that Feynman's analysis was far from the most general possible (and was considerably less general than Kraichnan's). He assumed a particular form for the matter action (that of a relativistic particle), and further assumed a strictly linear coupling of the spin-two field to the matter (which would not have been possible for a more general matter action). In particular, we note that all of the physical predictions of the theory are unchanged if a nonlinear local redefinition of the spin-2 field h is performed; we are free to replace $h_{\mu\nu}(x)$ by $\tilde{h}_{\mu\nu}(h(x)) = h_{\mu\nu}(x)+O(h(x)^2)$. Feynman has implicitly removed the freedom to perform such field redefinitions by requiring the coupling to the matter to be linear in h. (Field redefinitions are treated in detail by Boulware and Deser [BoDe 75].) A considerably more general analysis of the consistency condition for the field equation, performed much later by Wald [Wald 86], leads in the end to conclusions similar to those of Kraichnan and Feynman.

A quite different approach to deducing the form of the gravitational interaction was developed by Weinberg [Wein 64a, Wein 64b]. From very reasonable assumptions about the analyticity properties of graviton-graviton scattering amplitudes, Weinberg showed that the theory of an interacting massless spin-2 particle can be Lorentz invariant only if the particle couples to matter (including itself) with a universal strength; in other words, only if the strong principle of equivalence is satisfied. In a sense, Weinberg's argument is the deepest and most powerful of all, since the property that the graviton couples to the energy-momentum tensor is derived from other, quite general, principles. Once the principle of equivalence is established, one can proceed to the construction of Einstein's theory (see [Wein 72]).

Finally, there is the issue of how terms in the Lagrangian involving more that two derivatives of $h_{\mu\nu}$ are to be excluded. Feynman has little to say about this, except for the remark in §6.2 that including only the terms with two or fewer

[2] See also [BoDe 75], [Dese 87].

derivatives will lead to the "simplest" theory. (See also §10.3, for a related remark in a slightly different context.) He does not seem to anticipate the modern viewpoint [Wein 79]—that terms with more derivatives are bound to be present, but they have a negligible effect on the theory's predictions when the spacetime curvature is weak. The philosophy underlying this viewpoint is that the Einstein theory's Lagrangian is merely an "effective Lagrangian" that describes the low-energy phenomenology of a more fundamental theory—a theory that might involve new degrees of freedom (superstrings?) at length scales of order the Planck length $L_{\rm P} = (G\hbar/c^3)^{1/2} \simeq 10^{-33}$ cm. In the effective Lagrangian, all terms consistent with general principles are allowed, including terms with arbitrary numbers of derivatives. However, on dimensional grounds, a term with more derivatives will have a coefficient proportional to a higher power of L_P. Thus, in a process involving a characteristic radius of curvature of order L, terms in the Lagrangian with four derivatives have effects that are suppressed compared to the effects of terms with two derivatives—suppressed by a factor of order $(L_P/L)^2$, which is exceedingly small for any reasonable process. We can easily understand, then, why a truncated theory involving only terms with two or fewer derivatives would be in excellent agreement with experiment.

On the other hand, this same reasoning also leads one to expect a "cosmological" term (no derivatives) with a coefficient of order one in units of L_P. That the cosmological constant is in fact extraordinarily small compared to this naive expectation remains one of the great unsolved mysteries of gravitation physics [Wein 89].

Geometry:

After conducting a search for a sensible theory that describes the interactions of a massless spin-2 field in flat space, Feynman does not fail to express delight (in §8.4) that the resulting theory has a geometrical interpretation: "...the fact is that a spin two field has this geometrical interpretation; this is not something readily explainable—it is just marvelous." The development of the theory in Lectures 8–10 makes use of geometrical language, and is much more traditional than the approach of the earlier lectures.

In §9.3, Feynman comments that he knows no geometrical interpretation of the Bianchi identity, and he sketches how one might be found. The geometrical interpretation that he envisions was actually implicit in 1928 work of the French mathematician Elie Cartan [Cart 28]; however, it was largely unknown to physicists, even professional relativists, in 1962, and it was couched in the language of differential forms, which Feynman did not speak. Cartan's interpretation, that "the boundary of a boundary is zero," was finally excavated from Cartan's ideas by Charles Misner and John Wheeler in 1971, and has since been made widely accessible by them; see, e.g., chapter 15 of [MTW 73] at the technical level, and chapter 7 of [Whee 90] at the popular level.

Cosmology:

Some of Feynman's ideas about cosmology have a modern ring. A good example is his attitude toward the origin of matter. The idea of continuous matter creation in the steady state cosmology does not seriously offend him (and he notes in §12.2 that the big bang cosmology has a problem just as bad, to explain where all the matter came from in the *beginning*). In §1.2 and again in §13.3, he emphasizes that the total energy of the universe could really be zero, and that matter creation is possible because the rest energy of the matter is actually canceled by its gravitational potential energy. "It is exciting to think that it costs *nothing* to create a new particle, .." This is close to the currently popular view that the universe is a "free lunch," nothing or nearly nothing blown up to cosmological size through the miracle of inflation [Guth 81]. Feynman worries more about the need for baryon number nonconservation, if the universe is to arise from "nothing."

Feynman also expresses a preference for the "critical" value of the density of the universe in §13.1, a prejudice that is widely held today [LiBr 90]. In §13.2, he gives an interesting (and qualitatively correct) argument to support the density being close to critical: he notes that the existence of clusters and superclusters of galaxies implies that "the gravitational energy is of the same order as the kinetic energy of the expansion—this to me suggests that the average density must be very nearly the critical density everywhere." This was a rather novel argument in 1962.

It is evident that, already in the early 60's, Feynman recognized the need for new fundamental principles of physics that would provide a prescription for the initial conditions of the universe. Early in these lectures, in §2.1, he digresses on the foundations of statistical mechanics, so as to express his conviction that the second law of thermodynamics must have a cosmological origin. Note his statement, "The question is how, in quantum mechanics, to describe the idea that the state of the universe in the past was something special." (A similar insight also appears in *The Feynman Lectures on Physics* [Feyn 63a], and *The Character of Physical Law* [Feyn 67], which date from the same period.) Thus, Feynman seems to anticipate the fascination with quantum cosmology that began to grip a segment of the physics community some twenty years later. He also stresses, in §1.4 and §2.1, the inappropriateness of the Copenhagen interpretation of quantum mechanics in a cosmological context.

Superstars:

In 1962–63, when Feynman was delivering his lectures on gravitation, Caltech was awash in new discoveries about "strong radio sources".

For 30 years, astronomers had been puzzling over the nature of these strongest of all radio-emitting objects. In 1951 Walter Baade [Baad 52] had used Caltech's new 200-inch optical telescope on Palomar Mountain to discover that the brightest of the radio sources, Cygnus A, is not (as astronomers had expected) a star in our own galaxy, but rather is associated with a peculiar, somewhat distant galaxy. Two years later, R. C. Jennison and M. K. Das Gupta [JeDG 53], studying Cygnus A with a new radio interferometer at Jodrell Bank, England, had discovered that most of the radio waves come not from the distant galaxy's interior, but from two giant lobes on opposite sides of the galaxy, about 200,000 light years in size and 200,000 light years apart. Caltech's Owens Valley radio interferometer went into operation in the late 1950s, and by 1962–63, the year of Feynman's gravity lectures, it was being used, together with the Palomar 200 inch, to identify many more double lobed radio sources. Some, like Cyg A, were centered on galaxies; others were centered on star-like point sources of light (which Caltech's Maarten Schmidt on February 5, 1963 would discover to have huge red shifts [Schm 63], and Hong-Yee Chiu later that year would christen *quasars*). In 1962 and early '63, while Caltech's astronomers competed with each other to make new and better observations of these strange objects and interpret their spectra, astrophysicists competed in the construction of models.[3]

One particularly promising model, conceived in summer 1962 by Cambridge's Fred Hoyle and Caltech's William Fowler [HoFo 63], held that the power for each strong radio source comes from a supermassive star in the center of a galaxy. The radio lobes' prodigious energy (estimated by Geoffrey Burbidge as 10^{58} to 10^{60} ergs, i.e. the energy equivalent of 10^4 to 10^6 solar masses) required the powering star to have a mass in the range $\sim 10^6$ to 10^9 suns. By comparison with the ~ 100 solar mass upper limit for normal stars, these Hoyle-Fowler objects were indeed "supermassive." *Superstars* they came to be called in some circles.

Sometime in early 1963 (probably February or March), Fred Hoyle gave a SINS[4] Seminar, in Caltech's Kellogg Radiation Laboratory, on the superstar model for strong radio sources. During the question period, Richard Feynman objected that general relativistic effects would make all superstars unstable—at least if they were nonrotating. They would collapse to form what we now call black holes.

Hoyle and Fowler were dubious, but within a few months, they and independently Icko Iben [Iben 63] (a Senior Research Fellow in Fowler's Kellogg Lab) had verified that Feynman was probably right, and S. Chandrasekhar [Chan 64] at the University of Chicago had independently discovered the general relativistic instability and analyzed it definitively.

To Hoyle and Fowler, Feynman's remark was a "bolt out of the blue," completely unanticipated and with no apparent basis except Feynman's amazing physical intuition. Fowler was so impressed, that he described the seminar and Feynman's insight to many colleagues around the world, adding one more (true) tale to the Feynman legend.

Actually, Feynman's intuition did not come effortlessly. Here, as elsewhere, it was based in large measure on detailed calculations driven by Feynman's curiosity. And in this case, in contrast with others, Feynman has left us a detailed snapshot of one moment in the struggle by which he made his discovery: Lecture 14 of this volume.

We have pieced together the circumstances surrounding Lecture 14, largely from Icko Iben's January 1963 notes and memory, plus a ca. 1971 conversation between Feynman and Thorne, and inputs from James Bardeen, Steven

[3] For further historical detail see, e.g., chapter 9 of [Thor 94] and references therein.

[4] "Stellar Interiors and Nucleosynthesis," a seminar series that Fowler organized and ran.

Frautschi, James Hartle, and William Fowler:

Sometime in late 1962 or early January 1963, it must have occurred to Feynman that the Hoyle-Fowler superstars might be strongly influenced by general relativistic forces. According to Iben's notes, Feynman came to his office in Kellogg Lab sometime before January 18, raised the issue of how general relativity influences superstars, showed Iben the general relativistic equations that govern a superstar's structure, which Feynman had worked out for himself from first principles, and asked how astrophysicists like Iben go about building Newtonian stellar models from the analogous Newtonian equations. After their discussion, Feynman departed, and then returned a few days later, sometime during the week of January 21–25. "Feynman flabbergasted me," Iben recalls, "by coming in and telling me that he had [already] solved the ... equations. He told me that he was doing some consulting for a computer firm and solved the equations in real time on what must have been that generation's version of a workstation."

On Monday January 28, having had only a few days to think about his numerical solutions (and presumably having spent much of that time in other ways, since he also had to prepare a sophomore physics lecture for that same Monday), Feynman delivered Lecture 14 of this volume. (Note that this was just eight days before Maarten Schmidt's discovery of the quasar redshifts.)

Lecture 14 came in the midst of Feynman's struggle to figure out how superstars should behave—and it came *before* he realized that general relativity destabilizes them. As a result, the interpretation portions of Lecture 14 (§14.3 and §14.4) are largely wrong—but wrong in interesting ways that display Feynman's intuitive approach to problem solving.

Feynman never reviewed the Morinigo-Wagner written version of Lecture 14; and in 1971, when he approved Lectures 12–16 for distribution, he presumably had forgotten that the interpretation part of Lecture 14 was just a progress report on ruminations that had not yet borne fruit.

Feynman begins his Lecture 14 by introducing a model for a superstar "which is very simple, yet may possess a great many of the attributes of the real things. After we understand how to go about solving the simple problem, we may worry about refinements in the model." (The refinements—the influence of electron-positron pairs, neutrino emission, nuclear burning, rotation, and instabilities—would be added later in 1963–64 by Iben [Iben 63], Curtis Michael [Mich 63], Fowler [Fowl 64], and Bardeen [Bard 65], with major advice and input from Feynman.)

Because Feynman's goal is to learn about relativity's effects, his superstar model is fully general relativistic—by contrast with the previous Fowler-Hoyle models, which were Newtonian. On the other hand, where Fowler and Hoyle had included the contributions of both gas and radiation to the star's pressure p and internal energy density ϵ, Feynman simplifies by ignoring $p_{\rm gas}$ and $\epsilon_{\rm gas}$. This seems reasonable, since Feynman's principal focus was on a superstar with mass $M = 10^9 M_{\rm sun}$, and Hoyle and Fowler had shown that in the Newtonian

Figure 1: The binding energies of superstars made of hydrogen. Plotted vertically on the left is the negative of the star's fractional binding energy, i.e., $(M - M_{\rm rest})/M_{\rm rest}$, where M is the star's total mass and $M_{\rm rest}$ is the total rest mass of all its nucleons; plotted horizontally on the top is the star's radius R in units of the Schwarzschild radius $2M$ of a black hole of the same mass. The left and top scales are valid in the white region for superstars of any mass, but in the (nearly Newtonian) shaded region only for $M = 10^6 M_{\rm sun}$. Plotted vertically on the right is the negative of the star's binding energy in units of the sun's mass $M_{\rm sun}$; on the bottom is $R/2M$ multiplied by the ratio, β, of gas pressure to total pressure. The right and bottom scales are valid for superstars of any mass in the nearly Newtonian shaded region, but they fail in the fully relativistic white region. The vertical scale is arctangent; i.e., it is nearly linear for $|(M - M_{\rm rest})/M_{\rm sun}| \lesssim 1$ and logarithmic for $|(M - M_{\rm rest})/M_{\rm sun}| \gtrsim 1$. The thick part of the curve is from Feynman's calculations in Lecture 14; the thin parts are due to Fowler [Fowl 64], Iben [Iben 63], Bardeen [Bard 65], and Tooper [Toop 66].

limit superstars are strongly radiation dominated, with

$$\beta \equiv \frac{p_{\rm gas}}{p_{\rm radiation}} = \frac{2\epsilon_{\rm gas}}{\epsilon_{\rm radiation}} = 8.6 \left(\frac{M_{\rm sun}}{M}\right)^{1/2} \simeq 3 \times 10^{-4} \left(\frac{10^9 M_{\rm sun}}{M}\right)^{1/2} . \quad (2)$$

(Here, for simplicity, the gas is assumed to be pure hydrogen.) Because these stars are deeply convective, their entropy per nucleon is independent of radius, which means that β, which is $8\times$(Boltzmann's constant)/(entropy per nucleon), is also radius-independent; and this, as Feynman was aware, remains true in the fully relativistic regime, though Eq. (2) is changed there by a factor of order unity.

Having approximated away $p_{\rm gas}$ and $\epsilon_{\rm gas}$, Feynman proceeds, in §14.1 and §14.2 of Lecture 14, to construct the superstar's general relativistic equations of structure, he reports that he has integrated them numerically, and he presents his results in Table 14.1. This table is to be interpreted with the aid of equations (14.2.1), in which Feynman's parameter τ is

$$\tau = \frac{4/3}{\text{entropy per nucleon}} = \left(\frac{\text{nucleon rest mass}}{\text{Boltzmann's constant}}\right)\frac{\beta}{6} \simeq 1800\beta \, , \quad (3)$$

since Feynman uses units with the nucleon rest mass and 10^9 °K set to unity.

In discussing Feynman's models and his (mis)interpretation of them, Figure 1 will be useful. This figure shows some features of the family of superstar models that Feynman constructed (thick curve), together with their extension into the ultrarelativistic regime (upper thin curve) and into the nearly Newtonian regime (lower thin curve)—extensions that would be computed later by Iben [Iben 63], Fowler [Fowl 64], Bardeen [Bard 65], and Tooper [Toop 66]. Plotted vertically is the negative of the star's gravitational binding energy; plotted horizontally is the star's radius. In the nearly Newtonian regime (shaded region of the binding energy curve), which Feynman does not explore, $p_{\rm gas}$ and $\epsilon_{\rm gas}$ cannot be neglected, and the binding energy is given (as Fowler [Fowl 64] would show in response to Feynman's "bolt out of the blue") by the following delicate balance between gas effects (first term) and general relativistic effects (second term):

$$\frac{M - M_{\rm rest}}{M_{\rm rest}} \simeq -\frac{3\beta}{8}\left(\frac{2M}{R}\right) + 1.3\left(\frac{2M}{R}\right)^2 . \tag{4}$$

Here $2M$ is the Schwarzschild radius of a black hole with the same mass M as the superstar.

In interpreting his models (§14.3), Feynman begins by asking about the evolution of a superstar that contains a fixed number of nucleons (i.e., a fixed nucleon rest mass $M_{\rm rest}$), and that gradually radiates away thermal energy, thereby reducing its total mass M and making itself more tightly bound. He finds a strange evolution: As the star radiates, its radius increases (downward and rightward motion on the thick curve of Fig. 1), and its central temperature goes down. This is opposite to the behavior of most stars, which shrink and heat up as they radiate, if they are not burning fuel. (If, instead of dealing with the fully relativistic region to the left of the binding curve's minimum, Feynman had kept gas effects and computed the nearly Newtonian region to the right, he would have found the opposite behavior: the superstar would shrink and heat up as it radiates.)

Feynman then asks whether his superstar models are stable. "The stability of our star has not been studied [quantitatively]," he emphasizes, and he then goes on to expose his initial ruminations on the issue:

"[Models which have] the same number of nucleons and the same τ [same entropy] may be compared as to radii and central temperature. The fact that there is apparently a minimum in the radius [left-most bend in Fig. 1] ... is very suggestive; the star may have a stable position." Feynman here is groping toward a method of analyzing stability that would be perfected a year or so later by James Bardeen, when Bardeen had become Feynman's Ph.D. thesis student. Bardeen's perfected version of this argument [Bard 65, BTM 66] shows that, as one moves along the binding-energy curve, restricting oneself to stars of fixed $M_{\rm rest}$, the entropy changes from one model to another except in the vicinity of each minimum or maximum of the binding, where it is stationary. This means that the star possesses a zero-frequency mode of deformation at each minimum or maximum—a mode that carries it from one equilibrium model to another with the same entropy, binding energy, and rest mass. This in turn means that one mode of radial oscillation changes stability at each extremum of the binding. By examining the forms that the modes' eigenfunctions must take, Bardeen deduces that, if the binding curve bends clockwise as it moves through an extremum, then the mode is becoming unstable; and if counterclockwise, then the mode is becoming stable. (This statement holds regardless of the direction one traces the curve.) Bardeen's analysis, applied to Fig. 1, shows that the nearly Newtonian models in the lower right (which shrink as they radiate) are stable, and they must lose stability and collapse to form a black hole when they reach the minimum of the binding curve; the models beyond the minimum (including all of Feynman's models) possess one unstable mode of radial pulsation; the models beyond the first peak in the binding curve (upper left part of Fig. 1) posses two unstable modes; etc.

Feynman, of course, was not aware of this on January 28, 1963; so he goes on in Lecture 14 to seek insight into his models' stability by other methods. He imagines taking one of his superstar models with baryonic rest mass $M_{\rm rest}$, and breaking it up into two superstars, each with rest mass $M_{\rm rest}/2$, while holding the entropy per nucleon fixed. "Do we get work out of the process, or did we have to do work to get [the star] broken up?" From his Table 1 and equations (14.2.1) he deduces that "the two objects ... would be more massive; work is required to break up the system. This suggests that the star might not throw off material, but keep together in one lump," i.e., the star might be stable.

At first sight this seems a compelling argument. However, it actually is specious (as Feynman presumably realized sometime between this lecture and Hoyle's seminar). The two new stars that Feynman makes by breaking up his original star are further up the thick binding-energy curve of Fig. 1, i.e. they are more relativistic than the original star. However, there are also two stellar models, with the same rest masses and entropy per nucleon, on the stable, nearly Newtonian branch of the binding curve in the lower right corner of Fig. 1. If the original star were broken up into those two stars, energy would be released, which suggests correctly that the original star is actually unstable. Feynman missed this point because neither he, nor anyone else on January 28 1963, knew the form of the binding energy curve in the shaded region. It is reasonable to surmise, however, that he guessed enough about it before Hoyle's seminar to recognize his error.

Having misdiagnosed his stars' stability, Feynman goes on in §14.4 to propose directions for future superstar research. He begins by proposing a variational principle by which one might construct equilibrium models of fully relativistic, isentropic superstars: "compute the configuration of least mass starting with a fixed number of nucleons" (and fixed entropy per nucleon). Two years later John Cocke [Cock 65], working in Paris and presumably unaware of Feynman's proposal, would develop a detailed variational principle equivalent to Feynman's (holding the mass and nucleon number fixed and maximizing the entropy), and would use it to construct relativistic stellar models.

Feynman continues in §14.4 with, "After we have investigated static solutions, we may turn our attention to the full dynamical problem. The differential equations are horrifying." Feynman, in fact, had worked out some of the equations for himself. They would later be derived independently and solved numerically by Mikhail Podurets [Podu 64] in the USSR and Michael May and Richard White [MaWh 66] in the United States, using descendents of computer codes that were developed for the design of thermonuclear weapons. The result is well known: stars that experience the Feynman/Chandrasekhar relativistic instability implode to form black holes.

For about 10 years after Feynman's Lecture 14, rapidly rotating superstars remained a strong competitor in the marketplace of powerhouse models for quasars and strong radio sources. But gradually, in the 1970s, models based on rapidly spinning, supermassive black holes gained ascendency; and today superstars are generally regarded as fascinating but transient objects along a galaxy core's route toward formation of the supermassive black hole that will later come to dominate it; cf. [Thor 94].

Black holes:

The concept of a black hole was just beginning to emerge in the early 60's, and Feynman's views may have been slightly behind the times. Thus, the most seriously out-of-date lectures are probably 11 and 15, which deal with the Schwarzschild solution and its implications.

In one sense, what we now call a black hole already became known in 1916, when Karl Schwarzschild discovered his solution to the Einstein field equation [Schw 16]. But for decades most physicists stubbornly resisted the outrageous implications of Schwarzschild's solution. (This included Einstein himself, who wrote a regrettable paper in 1939 arguing that black holes cannot exist [Eins 39].) Even the beautiful and definitive analysis (also in 1939) of gravitational collapse by Oppenheimer and Snyder [OpSn 39] had surprisingly little impact for many years. Oppenheimer and Snyder studied the collapse of a spherically symmetric pressureless "star" of uniform density, and noted that the star's implosion, as seen by a stationary observer who remains outside, would slow and ultimately freeze as the star's surface approached the critical Schwarzschild circumference. Yet they also clearly explained that no such freezing of the implosion would be seen by observers riding in with the collapsing matter—these observers would cross inside the critical circumference in a finite proper time, and thereafter would be unable to send a light signal to observers on the outside. This extreme difference between the descriptions in the two reference frames proved exceptionally difficult to grasp. The two descriptions were not clearly reconciled until 1958, when David Finkelstein [Fink 58] analyzed the Schwarzschild solution using a coordinate system that made it easy to visualize simultaneously the worldlines of dust particles that fall inward through the critical circumference and the worldlines of outgoing photons that freeze there. This analysis revealed the unusual "causal structure" of the Schwarzschild spacetime—nothing inside the "horizon" can avoid being drawn toward spheres of smaller and smaller area. The emerging picture indicated (to some), that once the star fell through its critical circumference, its compression to form a spacetime singularity was inevitable. That this, indeed, is true, independent of any idealizing assumptions such as spherical symmetry and zero pressure, would be proved by Roger Penrose in 1964 [Penr 65].

Thus, the timing of Feynman's lectures on gravitation is unfortunate. A "golden age" of black hole research was about to begin, which would produce many remarkable insights over the next decade. These insights, which could not have been anticipated in 1962–63, would completely transform the study of general relativity, and help usher in the new discipline of relativistic astrophysics.

Feynman's 1962–63 view of the Schwarzschild solution was heavily influenced by John Wheeler. Wheeler had felt for many years that the conclusions of Oppenheimer and Snyder could not be trusted; he thought them physically unreasonable. As late as 1958, he advocated that, if a more realistic equation of state were used in the analysis of gravitational collapse, qualitatively different results would be obtained [HWWh 58]. (This view would become less tenable as the causal structure of the black hole geometry became properly understood.) Gradually, however, Wheeler came to accept the inevitability of gravitational collapse to form a black hole, in agreement with the conclusions of Oppenheimer-Snyder. (This shift of viewpoint was facilitated by the insights of Martin Kruskal [Krus 60], who, independently of Finkelstein, had also clarified the black hole causal structure; in fact, Kruskal's influential paper was largely written by Wheeler, though the insights and calculations were Kruskal's.) But during the years that he remained skeptical, Wheeler reacted in a characteristic way—he rarely mentioned the Oppenheimer-Snyder results in his published papers. It is revealing that in §11.6 Feynman remarks that it would be interesting to study the collapse of dust. He seems unaware that Oppenheimer and Snyder had studied dust collapse in detail 23 years earlier! In §15.1 he speculates, based on the (incorrect!) ruminations of Lecture 14, that a star composed of "real matter" cannot collapse inside its critical circumference.

Feynman makes several references to the "geometrodynamics" program that Wheeler had been pushing since the mid 50's, and was still pushing (if less vigorously) in 1962; cf. [Whee 62]. Wheeler and coworkers hoped to interpret elementary particles as geometric entities arising from (quantum versions of) classical solutions to the matter-free gravitational field equations. Wheeler was especially attracted to the concept of "charge without charge;" he noted that if electrical lines of force were trapped by the nontrivial topology of a "wormhole" in space, then each wormhole mouth would appear to be a pointlike charged object to an observer whose resolution is insufficient to perceive the tiny mouth [MiWh 57]. Wheeler emphasized that the Schwarzschild solution possesses spatial slices in which two asymptotically flat regions are connected by a narrow neck, and so provides a model of the wormhole geometry that he envisioned.

Feynman is clearly enamored of the wormhole concept, and he describes these ideas briefly in §11.5, and then again in §15.1 and §15.3. Note that Feynman calls a star confined inside its gravitational radius a "wormhole;" the term "black hole" would not be coined (by Wheeler) until 1967. For what we now call the "horizon" of a black hole, Feynman uses the older term "Schwarzschild singularity." This is an especially unfortunate locution, as it risks confusion with the actual singularity, the region of infinite spacetime curvature at the hole's center. Feynman never explicitly discusses this genuine singularity.

By 1962, the causal structure of the Schwarzschild solution was fairly well understood. It is well explained by Fuller and Wheeler [FuWh 62], a paper that Feynman mentions, and on which §15.1 is based. (This paper, one of the very few references cited in Feynman's lectures, employs Kruskal's coordinates to construct the complete analytically extended Schwarzschild geometry, and presents a "Kruskal diagram" that succinctly exhibits the properties of the timelike and null geodesics.) Feynman quotes the main conclusion: the Schwarzschild solution is not really a wormhole of the sort that Wheeler is interested in, because the wormhole throat is actually dynamical, and pinches off before any particle can traverse it. However, the Fuller-Wheeler paper does not mention any of the broader implications of this causal structure for the problem of gravitational collapse, and Feynman gives no indication of having appreciated those implications.

One can also see from Feynman's comments in §15.2 and §15.3 that he did not understand the causal structure of the ("Reissner-Nordström") charged black hole solution, which had been worked out in 1960 by Graves and Brill [GrBr 60]. Note the remark "... it is not inconceivable that it might turn out that a reflected particle comes out earlier than it went in!" In fact, on the analytically extended geometry, the geodesics pass into a "new universe" in finite proper time, rather than reemerging from the black hole (see, e.g., [HaEl 73]). However, the interior of this solution is known to be unstable to generic perturbations [ChHa 82]; for the "realistic" case of a charged black hole formed in gravitational collapse, the situation is qualitatively different, and still not fully understood—though it seems highly likely that the hole's core is so singular that nothing can pass through it to a "new universe," at least within the realm of classical general relativity [BBIP 91].

Gravitational waves:

As late as 1957, at the Chapel Hill conference, it was still possible to have a serious discussion about whether Einstein's theory really predicted the existence of gravitational radiation [DeWi 57]. This confusion arose in large measure because it is a rather subtle matter to define rigorously the energy transmitted by a gravitational wave—the trouble is that the gravitational energy cannot be expressed as the integral of a locally measurable density.

At Chapel Hill, Feynman addressed this issue in a pragmatic way, describing how a gravitational wave antenna could in principle be designed that would absorb the energy "carried" by the wave [DeWi 57, Feyn 57]. In Lecture 16, he is clearly leading up to a description of a variant of this device, when the notes abruptly end: "We shall therefore show that they can indeed heat up a wall, so there is no question as to their energy content." A variant of Feynman's antenna was published by Bondi [Bond 57] shortly after Chapel Hill (ironically, as Bondi had once been skeptical about the reality of gravitational waves), but Feynman never published anything about it. The best surviving description of this work is in a letter to Victor Weisskopf completed in February, 1961 [Feyn 61]. This letter contains some of the same material as Lecture 16, but then goes a bit further, and derives the formula for the power radiated in the quadrupole approximation (a result also quoted at Chapel Hill). Then the letter describes Feynman's gravitational wave detector: It is simply two beads sliding freely (but with a small amount of friction) on a rigid rod. As the wave passes over the rod, atomic forces hold the length of the rod fixed, but the proper distance between the two beads oscillates. Thus, the beads rub against the rod, dissipating heat. (Feynman included the letter to Weisskopf in the material that he distributed to the the students taking the course.)

However controversial they may have seemed to some of the participants at the Chapel Hill meeting, Feynman's conclusions about gravitational waves were hardly new. Indeed, a classic textbook by Landau and Lifshitz, which was completed in Russian in 1939 and appeared in English translation in 1951 [LaLi 51], contains several sections devoted to the theory of gravitational waves. Their account is clear and correct, if characteristically terse. In the letter to Weisskopf, Feynman recalls the 1957 conference and comments, "I was surprised to find a whole day at the conference devoted to this question, and that 'experts' were confused. That is what comes from looking for conserved energy tensors, etc. instead of asking 'can the waves do work?' "

Indeed, in spite of his deep respect for John Wheeler, Feynman felt an undisguised contempt for much of the relativity community in the late 50's and early 60's. This is perhaps expressed most bluntly in a letter to his wife Gweneth that he wrote from the Warsaw conference in 1962 [Feyn 88]:

> I am not getting anything out of the meeting. I am learning nothing. Because there are no experiments this field is not an active one, so few of the best men are doing work in it. The result is that there are hosts of dopes here and it is not good for my blood pressure: such inane things are said and seriously discussed that I get into arguments outside the formal sessions (say, at lunch) whenever anyone asks me a question or starts to tell me about his "work." The "work" is always: (1) completely un-understandable, (2) vague and indefinite, (3) something correct that is obvious and self-evident, but worked out by a long and difficult analysis, and presented as an important discovery, or (4) a claim based on the stupidity of the author that some obvious and correct fact, accepted and checked for years, is, in fact, false (these are the worst: no argument will convince the idiot), (5) an attempt to do something probably impossible, but certainly of no utility, which, it is finally revealed at the end, fails, or (6) just plain wrong. There is a great deal of "activity in the field" these days, but this "activity" is mainly in showing that the previous "activity" of somebody else resulted in an error or in nothing useful or in something promising. It is like a lot of worms trying to get out of a bottle by crawling all over each other. It is not that the subject is hard; it is that the good men are occupied elsewhere. Remind me not to come to any more gravity conferences!

So extreme an assessment could not have been completely justified even in 1962, nor does it seem likely that the letter reflected Feynman's true feelings with 100 per cent accuracy. Bryce DeWitt, who attended both the Chapel Hill and Warsaw conferences, offers this comment [DeWi 94]:

> I can surely sympathize with Feynman's reaction to the Warsaw conference because I had similar feelings. (I have a vivid memory of

> him venting his frustrations there by giving Ivanenko one of the most thorough tongue lashings I have ever heard.) But those who publish his private letter without giving the whole picture do a disservice to historical truth. Although he felt that some of the discussion at the Chapel Hill conference was nonsensical (as did I), I think he had a reasonably good time there. I remember him being very interested when I showed that his path integral for a curved configuration space leads to a Schroödinger equation with a Ricci scalar term in it. The people at that conference (such as Bondi, Hoyle, Sciama, Møller, Rosenfeld, Wheeler) were not stupid and talked with him intelligently. (I had chosen the participants myself—it was a closed conference.) Feynman's experience at Chapel Hill surely had something to do with his willingness to accept the invitation to Warsaw (which was an open conference). Even at Warsaw he and I had discussions outside the formal session that I try not to believe he could honestly have put into one of the six categories in his letter.

However apt Feynman's comments may or may not have been in 1962, they would soon cease to be so. The "golden age" of black hole research was just beginning to dawn.

Philosophy:

A striking feature of these lectures is that Feynman is frequently drawn to philosophical issues. (He often showed disdain for philosophers of science and for the word "philosophical," which he liked to mockingly pronounce "philo-ZAW-phical;" nevertheless, he is revered, at least by physicists, for his philosophical insights.) For example, he ruminates in §1.4 on whether quantum mechanics need actually apply to macroscopic objects. (The argument he sketches there to support the claim that quantization of gravity is really necessary was presented at the 1957 Chapel Hill conference, where it provoked a great deal of discussion.) Another example is his preoccupation with Mach's principle. Mach's idea—that inertia arises from the interactions of a body with distant bodies—bears a vague resemblance to the interpretation of electrodynamics proposed by Feynman and Wheeler when Feynman was in graduate school [WhFe 45, WhFe 49]: that the radiation reaction force on an accelerated charge arises from interactions with distant charges, rather than with the local electromagnetic field. So perhaps it should not be surprising that in §5.3 and §5.4 Feynman seems sympathetic to Mach's views. He gropes for a quantum mechanical formulation of Mach's principle in §5.4, and revisits it in a cosmological context in §13.4. Feynman's reluctance in §9.4 and §15.3 to accept the idea of curvature without a matter source also smacks of Mach.

Philosophical reflections come to the fore in a number of brief asides. In §8.3 Feynman assesses the meaning of the statements that space is "really" curved or flat. In §7.1 he explains why Newton's second law is not a mere tautology (a definition of "force"). He takes some slaps at rigor in §10.1 ("it is the facts that matter, and not the proofs") and in §13.3 ("there is no way of showing mathematically that a physical conclusion is wrong or inconsistent"). And in §13.4 he questions the notion that simplicity should be a guiding principle in the search for the truth about Nature: "...the simplest solution by far would be nothing, that there should be *nothing* at all in the universe. Nature is more inventive than that, so I refuse to go along thinking that it always has to be simple."

It is also revealing when Feynman bullheadedly pursues an unpromising idea in §2.3 and §2.4—that gravitation is due to neutrino exchange. This is an object lesson on how Feynman believes a scientist should react to a new experimental phenomenon: one should always look carefully for an explanation in terms of known principles before indulging in speculations about new laws. Yet, at the same time, Feynman emphasizes over and over again the importance of retaining skepticism about accepted ideas, and keeping an open mind about ideas that appear flaky. Quantum mechanics might fail (§1.4 and §2.1), the universe might not be homogeneous on large scales (§12.2 and §13.2), the steady state cosmology could turn out to be right (§13.3), Wheeler's intuition concerning wormholes might be vindicated (§15.3), etc.

One-loop quantum gravity:

Feynman's investigations of quantum gravity eventually led him to a seminal discovery (which, however, is not described in this book, except for a brief mention in §16.2). This is his discovery that a "ghost" field must be introduced into the covariantly quantized theory to maintain unitarity at one-loop order of perturbation theory. The timing of this discovery can be dated fairly precisely. Feynman reported the result in a talk at a conference in Warsaw in July, 1962 [Feyn 63b], and commented that the problem of unitarity at one-loop order had only been "completely straightened out a week before I came here." Thus, he had it all worked out before he gave these lectures.

By computing one-loop amplitudes using the naive covariant Feynman rules, Feynman had found that the contributions of the unphysical polarization states of the graviton fail to cancel completely, resulting in a violation of unitarity. For awhile, he was unable to resolve this puzzle. Then Murray Gell-Mann suggested to Feynman that he try to analyze the simpler case of massless Yang-Mills theory. (Gell-Mann recalls making this suggestion in 1960 [Gell 89].) Feynman found that he could fix up the problem in Yang-Mills at one loop, and then could use a similar method for gravity. In his Warsaw talk, Feynman reports that he has solved the unitarity problem at one loop order, but that he is now stuck again—he doesn't know how to generalize the method to two or more loops. Yet he protests that, "I've only had a week, gentlemen." He never solved this problem.

There is an interesting exchange in the question period following Feynman's talk at Warsaw [Feyn 63b], in which Bryce DeWitt presses Feynman for more detail about how unitarity is achieved at one-loop order, and Feynman resists. But DeWitt is persistent, and Feynman finally relents, and offers a rather lengthy explanation, prefaced by the comment, "Now I will show you that I too can write equations that nobody can understand." This is amusing because it was eventually DeWitt [DeWi 67a, DeWi 67b] (and also Faddeev and Popov [FaPo 67], independently) who worked out how to generalize the covariant quantization of Yang-Mills theory and gravitation to arbitrary loop order. It is worth noting that Feynman's own path integral techniques were crucial to the general formulation, and that the more complete results of DeWitt and Faddeev-Popov were clearly inspired by Feynman's one-loop construction.

Feynman's lectures late in the gravitation course (the ones that are not reproduced in this book) concerned loop corrections in quantum gravity and Yang-Mills theory. He described the one-loop results that had been reported in Warsaw, and his attempts to extend the results to higher order. By all accounts, these lectures were complicated and difficult to follow, and sometimes flavored by a palpable sense of frustration. We presume that Feynman felt embarrassment at having failed to find a satisfactory formulation of the perturbation expansion, and that this is why he never authorized the distribution of the notes for these lectures.

Feynman wrote up a detailed account of his results only much later [Feyn 72], in two articles for a volume in honor of John Wheeler's 60th birthday. These articles would never have been written had it not been for persistent badgering by one of us (Thorne). By mutual agreement, Thorne telephoned Feynman at home once a week, at a prearranged time, to remind him to work on his contributions to the Wheeler Festschrift. This continued until the articles were finally completed. It was clear that Feynman returned to quantum gravity only with some pain and regret.

One of Feynman's goals was to settle the issue of the renormalizability of the theory. At the Warsaw meeting, he says that he's not sure if it is renormalizable, but in §16.2 he puts it more strongly: "I suspect that the theory is not renormalizable." Even this statement sounds surprisingly cautious to us today. In any case, Feynman was always reluctant to use renormalizability as a criterion for judging a theory, and he professes in §16.2 not to know whether nonrenormalizability is "a truly significant objection."

Conclusion:

Any book about gravitation prepared over 30 years ago is inevitably out of date today, at least in some respects. This book is certainly no exception. Moreover, we believe that the lectures did not meet Feynman's own expectations, even at the time they were given. He had hoped that teaching this course would help bring his work on quantum gravity to a coherent conclusion, but it did not do so. Toward the end of the year, it was evident to the students that Feynman felt discouraged and frustrated. Thus, in accord with Feynman's own wishes, the lectures (17 through 27) that focused on issues in quantum gravity are not included in this book.

Still, we feel that there is much in this volume's Lectures 1–16 that will be valued by physicists, students, historians, and admirers of Feynman. Moreover, the lectures are fun. Many passages offer a glimpse of a great mind approaching deep and challenging questions from an original perspective. Feynman thought long and hard about gravitation for several years, yet he published remarkably little on the subject. These characteristically clear and clever lectures are an addition to his published oeuvre that should surely be welcomed.

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John Preskill and Kip S. Thorne
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