A road map for Feynman’s adventures in the land of gravitation

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Abstract

Richard P. Feynman’s work on gravitation, as can be inferred from several published and unpublished sources, is reviewed. Feynman was involved with this subject at least from late 1954 to the late 1960s, giving several pivotal contributions to it. Even though he published only three papers, much more material is available, beginning with the records of his many interventions at the Chapel Hill conference in 1957, which are here analyzed in detail, and show that he had already considerably developed his ideas on gravity. In addition he expressed deep thoughts about fundamental issues in quantum mechanics which were suggested by the problem of quantum gravity, such as superpositions of the wave functions of macroscopic objects and the role of the observer. Feynman also lectured on gravity several times. Besides the famous lectures given at Caltech in 1962-63, he extensively discussed this subject in a series of lectures delivered at the Hughes Aircraft Company in 1966-67, whose focus was on astronomy and astrophysics. All this material allows to reconstruct a detailed picture of Feynman’s ideas on gravity and of their evolution until the late sixties. According to him, gravity, like electromagnetism, has quantum foundations, therefore general relativity has to be regarded as the classical limit of an underlying quantum theory; this quantum theory should be investigated by computing physical processes, as if they were experimentally accessible. The same attitude is shown with respect to gravitational waves, as is evident also from an unpublished letter addressed to Victor F. Weisskopf. In addition, an original approach to gravity, which closely mimics (and probably was inspired by) the derivation of the Maxwell equations given by Feynman in that period, is sketched in the unpublished Hughes lectures.

\textit{Dedicated to the Memory of Erasmo Recami.}

1 Introduction

Richard P. Feynman’s approach to physics was not a sectorial one. According to his vision, all branches of it are parts of a whole, reflecting the innermost unity of nature itself\footnote{For example, in [1], chap. I-3, he says: “If our small minds, for some convenience, divide this glass of wine, this universe, into parts-physics, biology, geology, astronomy, psychology, and so on-remember that nature does not know it! So let us put it all back together, not forgetting ultimately what it is for”, while he explicitly refers to “the underlying unity of nature” in chap II-12.}. In particular, he was among
the first to be concerned about the relation of gravitation to the rest of physics, in a period (the early 1950s) where general relativity practitioners still tended to be isolated from mainstream research, while the so-called renaissance of general relativity would only take place some years later. Feynman’s interest on gravity dates at least from late 1954 (as recalled in [10, 11]). He gave several fundamental contributions in the following years, until the late sixties, when he apparently lost interest in the subject. The sticky-bead argument, the Feynman rules for general relativity and the associated ghosts, the Caltech Lectures on Gravitation, and the Feynman-Chandrasekhar instability of supermassive stars, are now part of the common lore about classical and quantum gravity.

In this paper we retrace the full development of Feynman’s ideas on this subject. We start from the 1957 Chapel Hill conference, where for the first time Feynman’s thoughts on gravity were publicly expressed and whose written records are widely available. At that conference, which was pivotal in triggering the renaissance of general relativity, the gravitational physics community delineated the tracks along which subsequent work would develop. Besides cosmology, which at the time was still considered “a field on its own, at least at present, not intimately connected with the other aspects of general relativity” (p. 352), the main issues to be addressed at Chapel Hill were classical gravity, quantum gravity, and the classical and quantum theory of measurement (as a link between the previous two topics). The records of Feynman’s interventions at that conference show that he had already deeply thought, and performed computations, about all three topics, focusing on classical gravitational waves, on arguments in favor of quantum gravity from fundamental quantum mechanics, and finally on quantum gravity itself. We shall therefore develop our narrative along these lines, starting with Feynman’s interventions at Chapel Hill and then following the developments of the subsequent years. Some not well-known unpublished material is considered as well, namely the transcriptions of two sets of lectures, which Feynman delivered in the years 1966-67 and 1967-68 at the Hughes Aircraft Company, which have recently been made available on the web. In particular, the 1966-67 lectures, which were devoted to astronomy, astrophysics and cosmology, contain a preliminary discussion of the elements of general relativity. This treatment displays many similarities with the more famous Lectures on Gravitation, delivered at Caltech in 1962-63, but also several differences, probably a consequence of the evolution of the physicist’s ideas in the years between the two courses. In those years, as we reported elsewhere, Feynman developed a new derivation of Maxwell’s equations, with the aim of finding an original way of teaching electromagnetism. Probably inspired by this, in the Hughes lectures he suggested that a similar approach, with suitable modifications, could be adopted also for gravity. This suggestion is scattered in several places in the

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2 “The problem of the relation of gravitation to the rest of physics is one of the outstanding theoretical problems of our age”(2, p. 15); “My subject is the quantum theory of gravitation. My interest in it is primarily in the relation of one part of nature to another” (3, p. 697).

3 This expression was coined by Clifford M. Will in his popular book (see also 5), to denote the period going roughly from the mid-late fifties to the late seventies, in which general relativity gradually switched from being a subject at the margins of physics (as it was from the mid-twenties to the mid-fifties, a period which Jean Eisenstaedt called the low water-mark of general relativity) to being a mainstream subject.

4 Since Murray Gell-Mann wrote that Feynman had “made considerable progress”, it is very likely that he had already been working on gravity for some years in 1954, probably starting shortly after taming quantum electrodynamics, in the early 50s.

5 The story of how Feynman got involved in teaching at the Hughes Aircraft Company is briefly told in 17.
lectures on astrophysics [15], but also in those given in the following year (1967-68), whose focus was on electromagnetism [16]. Feynman limited himself to these hints, without pursuing them further, plausibly because of the considerably higher analytical complexity of developing full general relativity along these lines, in comparison with electromagnetism.

The paper is organized as follows: in Section 2 we introduce the Chapel Hill conference and Feynman’s contributions to it. In Section 3 we focus on gravitational waves and on the sticky-bead argument. In Section 4 we analyze Feynman’s considerations on the foundational issues related to the quantization of gravity. In Section 5 we discuss and put into context his arguments in favor of a field theoretical viewpoint on classical general relativity and on its subsequent quantization. In Section 6 we describe his work on the quantization and renormalization of gravity. In Section 7 we give an overview of the parts of the Hughes lectures of 1966-67 [15] focusing on relativistic gravity issues, and we draw a comparison with the treatment given in the Lectures on Gravitation [12]. Finally, we discuss the approach to gravity sketched both in the 1966-67 [15] and in the 1967-68 Hughes lectures [16]. Section 8 is devoted to our conclusions.

2 The Chapel Hill conference

The renaissance of general relativity was characterized by the establishment of a community of researchers, which was also achieved through the organization of a series of international conferences entirely devoted to the subject [9]. The first one was the Bern Jubilee conference of 1955 [21], celebrating the fiftieth anniversary of the formulation of special relativity. The Chapel Hill conference, organized in 1957 by Bryce S. DeWitt and his wife Cécile DeWitt-Morette [2], was the second one (although it is commonly referred to as GR1, while the Bern conference is called GR0). Unlike the Bern conference, which involved mostly European physicists of the older generation, the Chapel Hill conference involved many younger physicists, in particular many Americans, which would soon become leaders in the field. In fact, it had a bigger impact on the field and contributed a great deal to defining the trends for most of the subsequent research in classical and quantum gravity, and to recognizing general relativity as a genuine physical theory. This recognition, whose need was widely felt among the practitioners, was indeed the main aim of the conference, as its title (The Role of Gravitation in Physics) clearly shows. An excellent account of the events that brought to the organization of the conference can be found in the introduction to the recent republication of the conference records [2].

2.1 Feynman’s contributions

During the Chapel Hill conference, Feynman discussed the reality of gravitational waves, proposing the well-known sticky-bead argument to simply and intuitively argue that gravitational waves carry energy, and therefore are not just coordinate artifacts. Also, he discussed several foundational issues in quantum mechanics, which were linked with the problem of the quantization of gravity; in particular, he explicitly characterized Hugh A. Everett’s novel interpretation of quantum mechanics, which was

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6The full participant list is reported in [2], pp. 39-40.
presented there for the first time\textsuperscript{7}, in terms of “many-worlds”\textsuperscript{8}, in an attempt to criticize it (cf. the quotation reported in Section \ref{sec:4.5}). Lastly, Feynman advocated a quantum field theoretical approach to general relativity, as opposed to the geometric one which had been the standard one since 1915. Such an approach was in fact being pursued and taught by several particle theorists who got involved in gravity in that period, such as Sidney Coleman \cite{29}. Indeed, according to Feynman, the geometric approach prevented the merging of gravity with the rest of physics, although he considered it to be beautiful and elegant. As he later commented:

This great argument\textsuperscript{9} is so perfect it has never been used to solve other physical problems. This is perhaps due to the geometrical interpretation of gravity which no one can unravel into non geometrical arguments (\cite{15}, p. 30).

Feynman did not like rigorous mathematical arguments. In his opinion, in a field like gravity, where experimental input was, at that time, limited (or even absent for the quantum case), physicists could rely either on mathematical rigor or on imagination, that is, on thought experiments (\cite{2}, pp. 271-272). Since he did not believe in rigor, he chose the second way, as is evident in all his work on gravity. In his own words (\cite{2}, p. 272):

I think the best viewpoint is to pretend that there are experiments and calculate. In this field since we are not pushed by experiments we must be pulled by imagination.

He would in fact often try to compute measurable (at least in principle) physical effects, like the Lamb shift of a gravitationally held atom, the Compton scattering of gravitons, or the energy dissipated by the Sun-Earth system through radiation of gravitational waves. His arguments in favor of the quantization of the gravitational field, and of the reality of gravitational waves, relied on thought experiments rather than on sophisticated mathematical analysis. This attitude explains why, unlike most relativists, he gave up the geometrical approach without much regret. A second motivation for him to treat gravity in that way was his belief that quantum mechanics underlies the basic structure of nature, with the forces we observe at the macroscopic level emerging from quantum mechanics in the classical limit:

I shall call conservative forces, those forces which can be deduced from quantum mechanics in the classical limit. As you know, Q.M. is the underpinning of Nature (\cite{16}, p.35).

This is of course true for electromagnetism, as Feynman declared in several places \cite{20}, and it is true for gravity as well. As a matter of fact, even Feynman’s considerations in classical gravity, such as those dealing with gravitational waves, have their roots in quantum physics, as classical general relativity was considered the tree diagram approximation of a quantum theory of gravity.

\textsuperscript{7}This topic was briefly mentioned in a discussion session (\cite{2}, p. 270) by Everett’s Ph.D. advisor John A. Wheeler (Everett himself did not participate to the conference), who also urged DeWitt to include an account of it (which was an abridged version of Everett’s Ph.D. thesis) in the special issue of the Reviews of Modern Physics devoted to the conference (\cite{22}, p. 94).

\textsuperscript{8}This expression was popularized by DeWitt \cite{23}-\cite{28}, who in the late sixties and early seventies undertook the task of making Everett’s interpretation better known among physicists, having realized that it had been substantially unnoticed after its introduction in 1957 (see \cite{23}, pp. 94-97).

\textsuperscript{9}Here Feynman is referring to Albert Einstein’s geometric approach.
3 Do gravitational waves exist?

The general covariance of general relativity makes it difficult – in the absence of an invariant characterization – to distinguish between real physical phenomena and artifacts due to the choice of the coordinate system (which would disappear by changing coordinates). At the time of the Chapel Hill conference, this was not clear yet for gravitational waves, which until then had been studied only in special coordinate systems\[10\]. In 1916 \[31\] and 1918 \[32\], Einstein first found that his gravitational field equations, when linearized, admit wave-like solutions travelling with the speed of light, by adopting a particular coordinate system. He also showed that at least some of these solutions carried energy and provided a formula for the leading-order energy-emission rate, the famous \textit{quadrupole formula}. However, he encountered some difficulties in the definition of such energy, due to the fact, already known to him, that energy is not localizable in general relativity. In 1922, Arthur S. Eddington noted the weakness of Einstein’s derivation:

The potentials $g_{\mu\nu}$ pertain not only to the gravitational influence which has objective reality, but also to the coordinate-system which we select arbitrarily. We can “propagate” coordinate-changes with the \textit{speed of thought}, and these may be mixed up at will with the more dilatory propagation discussed above. There does not seem to be any way of distinguishing a physical and a conventional part in the changes of the $g_{\mu\nu}$ \[33\], p. 130, emphasis in the original\[11\].

Further doubts concerned the possibility that gravitational waves were just artifacts of the linearized approximation, which would disappear once the full theory was considered. In fact, Einstein himself \[35\], while attempting, along with Nathan Rosen, to find plane wave solutions of the full nonlinear field equations of gravitation, became convinced that the full theory predicted no gravitational waves\[12\]. However, after a mistake was found\[13\], Einstein and Rosen finally discovered a rigorous solution describing waves radiated off the axis of an infinite cylinder \[37\]. This did not settle the issue yet, since the problem of the physical effects and energy conveying of gravitational waves remained unsolved. Such problems arose as a consequence of the lack (at that time) of a general relativistic theory of measurement, along with the already mentioned difficulties inherent in the definition of energy in general relativity. In particular, the wave solutions found by Einstein and Rosen seemed not to carry energy, as still argued by Rosen himself in 1955 \[21\], pp. 171-174, and it was not known whether this depended on their unbounded nature; also, it was not known whether full general relativity admitted spherical wave solutions, describing energy radiated by a localized center.

\[10\]For an excellent account of the fascinating history of gravitational waves, which adds considerable detail to what follows, see \[30\].

\[11\]In fact, Eddington was not questioning (as is sometimes stated) the existence of gravitational disturbances travelling with a finite speed, which was required by special relativity, but rather Einstein’s procedure, in which the speed of light seemed to be put in by hand by the particular coordinate choice he adopted. In fact, in a subsequent paper \[34\], Eddington showed more rigorously that some of Einstein’s wave solutions were not spurious, i.e. were not just flat space in curvilinear coordinates, and did propagate with the speed of light. In the same paper, he corrected a minor error in Einstein’s quadrupole formula (a factor of 2), and applied it to compute the radiation reaction for a rotator.

\[12\]See also \[39\], letter 71.

\[13\]The singularities that Einstein and Rosen had found were in fact mere coordinate artifacts \[30\].
The dilemma thus persisted that gravitational waves could exist only as mathematical objects, with no actual physical content, and all the above problems remained unsolved until the appearance of Felix A. E. Pirani’s work in 1956 \[38, 39\] and the subsequent discussions which took place at Chapel Hill in the following year \[2\], where this work was presented. While investigating the physical meaning of the Riemann tensor in terms of the geodesic deviation equation, Pirani managed to give an invariant definition of gravitational radiation, in terms of spacetime curvature. Gravitational waves would in fact consist in propagating ripples in spacetime\[14\]. Since curvature modifies the proper distance between test particles, a gravitational wave producing such an effect had to be real\[15\]. As we will see, this was Feynman’s starting point when he proposed his famous sticky-bead argument\[16\].

An additional unsolved issue, related to the former, came to the physicists’ attention. It concerned the energy-loss rate in a binary star system due to the emission of gravitational waves, and whether, in this process, the energy carried by such waves is proportional to the square of their amplitude. In fact, such a gravitationally-bound system cannot be described in the linearized approximation, and it was not known whether Einstein’s quadrupole formula correctly describes the energy loss beyond the linear approximation. The computation was further complicated by the fact that the radiation reaction on the emitting stars has to be taken into account. Indeed, it is very difficult to analytically solve the Einstein equations in the case of a strongly gravitating source, in order to compute the amplitude of gravitational waves. A widely used approximation scheme, suitable to gravitationally-bound systems, is the \textit{post-Newtonian} one (for reviews see \[41, 42\]). It assumes, as small parameters, the magnitude of the metric deviation from the background Minkowski metric and the squared ratio \((\frac{u}{c})^2\) between the typical velocity of the system \(u\) and the speed of light \(c\). For instance, in the case of a binary system, the typical velocity is the average orbital velocity while the Newtonian potential measures deviations from the flat metric at the lowest order. The result is a Newtonian description at the lowest order, while relativistic effects arise at higher orders. The radiative effects emerge at the order \((\frac{u}{c})^5\). The problem was first addressed in 1941 by Lev D. Landau and Evgenij M. Lifshitz, in the first edition of their textbook \[43\], where energy loss was computed by applying the quadrupole formula. But it was not clear whether their calculation was correct. At the time of the Chapel Hill conference, there still was no consensus about the result of such a computation, and not even on the fact that gravitational waves were radiated at all. Differently from the issue of the existence and detectability of gravitational waves, this one was not settled at Chapel Hill, but continued to be controversial for many years \[30\].

\[14\] More precisely, gravitational wave-fronts manifest themselves as propagating discontinuities in the Riemann tensor across null 3-surfaces.

\[15\] This same reasoning was also applied to the cylindrical waves of Einstein and Rosen by Joseph Weber and Wheeler in the special issue of Reviews of Modern Physics devoted to the conference \[40\].

\[16\] The exact words “sticky bead” are usually attributed to Feynman. However, in the proceedings \[2\] there is no record of his use of them at Chapel Hill. We could not retrace the first usage of that expression.

\[17\] This is the lowest nontrivial order with an odd power of \(v\), which signals the dissipation of energy due to radiation.

\[18\] Interestingly, one of the most common objections towards the existence of a gravitational radiation reaction was the lack of any evidence that advanced potentials did not play any role in the theory. Some people believed that maybe gravity worked like a Wheeler-Feynman absorber theory \[44-46\], in which the relevant solution was a combination of retarded plus advanced potential (even though the nonlinearity of the theory prevented a combination of two solutions to be a rigorous solution), but unlike the case of electromagnetism, the weakness of the force prevented the existence of absorbers capable of breaking time-reversal symmetry. Apparently, despite having developed the absorber theory for electromagnetism, Feynman did not believe in such ideas, since, as we shall see, he solved the wave equation within
These issues captured the interest at the Chapel Hill Conference, as clearly recalled by Peter G. Bergmann in his Summary [14] (p. 353):

In view of our interest in the role that gravitation, and particularly its quantum properties, may play in microphysics, the existence and the properties of gravitational waves represent an issue of preeminent physical significance.

Let us now turn to Feynman’s contributions to both the issues of the reality of gravitational waves, and of gravitational radiation by binary star systems.

3.1 The sticky-bead argument

The question of the actual existence of gravitational waves appeared in a discussion following one of Feynman’s thought experiments, in a session devoted to discussing the necessity of gravity quantization (these thought experiments will be described in Section 4). Feynman arrived one day late at the conference and hence had not attended the session on gravitational waves. Although he was trying to argue for the need to quantize the gravitational field even in the absence of gravitational waves, Feynman came out with a simple physical argument in favor of their existence ([2], p. 260 and pp. 279-281). He argued that gravitational waves, if they exist, must carry energy and, along with their existence as solutions of the gravitational equations (albeit in the linear approximation), this was enough for him to be confident in the possibility of their actual generation. In his words: “My instincts are that if you can feel it, you can make it” ([2], p. 260). Feynman reasoned as follows:

Suppose we have a transverse-transverse wave generated by impinging on two masses close together. Let one mass $A$ carry a stick which runs past touching the other $B$. I think I can show that the second in accelerating up and down will rub the stick, and therefore by friction make heat. I use coordinates physically natural to $A$, that is so at $A$ there is flat space and no field ([2], p. 279). Then he recalled the result by Pirani [38, 39] (which was presented earlier at the conference, cf. [2], p. 141), stating that the displacement $\eta$ of the mass $B$ (measured from the origin $A$ of the coordinate system) in the field of a gravitational wave satisfies the following differential equation [20]

$$\ddot{\eta}^a + R^a_{\ 00} \dot{\eta}^b = 0, \quad (a, b = 1, 2, 3)$$

(1)

$R$ being the curvature tensor at $A$. The curvature does not vanish for the transverse-transverse gravity wave but oscillates as the wave goes by, so that Eq. (1) predicts that the particle vibrates a little linearized gravity in full analogy with standard electrodynamics.

19Feynman’s words here are not very precise, since it seems that the same two masses both generate and receive the wave. Presumably, he was considering a wave generated by two colliding masses and another couple of masses (the “beads”) as detector. It may also be that the words “generated by” have been transcribed by mistake, so that it is the wave which impinges on the masses. However, the process which generates the wave is not really relevant for the subsequent argument, which instead is very clear.

20This follows from the geodesic deviation equation ([2], p. 141; [38, 39]).
up and down, hence it rubs the stick, producing heat by friction. This means that the stick absorbs energy from the gravitational wave, leading to the conclusion that gravitational waves carry energy.

Within the discussion ([2], p. 260), Feynman also quoted a result concerning the calculation of the energy radiated by a two-body system (e.g. a binary star) in a circular orbit, showing that he had addressed also the second issue mentioned above, with detailed calculations:

\[
\frac{\text{Energy radiated in one revolution}}{\text{Kinetic energy content}} = \frac{16\pi}{15} \sqrt{mM} \left(\frac{u}{c}\right)^5.
\]

(2)

Here \(m\) and \(M\) are the masses of the two bodies, while \(u\) is the magnitude of their relative velocity. For the Earth-Sun system, this formula describes a tiny effect, leading to a huge order of magnitude (about \(10^{26}\) years) for the lifetime of the motion of the Earth around the Sun. The above argument was based on a detailed analysis performed by Feynman some time before ([2], p. 280), but the full calculation (with some differences, according to Feynman) was reported only four years later in a letter written to Victor F. Weisskopf ([47] (see next Subsection).

Hermann Bondi and Weber also attended the Chapel Hill conference. Bondi likely envisaged an argument similar to Feynman’s, and shortly after the conference he published a variant of the sticky-bead device ([48]), although he did not succeed in relating the intensity of the gravitational wave to the amount of the energy carried by it. The fact that gravitational waves were real and physical was rigorously proved, in the framework of full nonlinear general relativity, not much later, by Bondi himself, Pirani, Ivor Robinson and Andrzej Trautman, who found exact solutions describing plane waves ([49]), and waves radiated from bounded sources ([50]). In the sixties, Bondi, Rainer W. Sachs, Ezra T. Newman and Roger Penrose ([51] - [55], gave a satisfactory definition of the energy radiated at infinity as gravitational radiation by an isolated gravitating system. Weber soon began to study how to experimentally detect gravitational waves ([56], building his famous resonant bar detector in 1966 ([57] and announcing the first experimental results (soon to be disproved) in 1969 [58]. These works triggered all the subsequent research on the detection of gravitational waves.

3.2 Further elaboration

Although Feynman never published anything on gravitational waves, a complete and more systematic description of his Chapel Hill proposal can be found in the already mentioned letter he wrote to Weisskopf in February 1961 ([47], and subsequently included in the material distributed to the students attending his Caltech lectures ([12] in 1962-63. A part of that calculation can in fact also be found in Lecture 16. Feynman indeed claimed that

\[\text{It was this entire argument used in reverse that I made at a conference in North Carolina}\]

\[\text{This is proved by several of his remarks at the conference, such as the following one, made after Pirani’s talk ([2], p.142): “Can one construct in this way an absorber for gravitational energy by inserting a } \frac{\text{d} \eta}{\text{d} \tau} \text{ term, to learn what part of the Riemann tensor would be the energy producing one, because it is that part that we want to isolate to study gravitational waves?”}. \]

\[\text{The absorber of gravitational energy is nothing but the “stickiness” of Feynman’s sticky-bead.}\]

\[\text{In fact, the computations in the letter must have been somewhat simpler, since Feynman admits that “Only as I was writing this letter to you did I find this simpler argument” ([47], p.14).}\]
several years ago to convince people that gravity waves must carry energy. ([37], p. 14).

Feynman’s reasoning developed in close analogy with electrodynamics, but taking into account the fact that in the case of gravitation the source is a tensor $S_{\mu\nu}$ rather than a vector $A_{\mu}$, so that the following differential equation has to be solved to find classical gravitational waves:

$$\Box^2 \overline{T}_{\mu\nu} = \lambda S_{\mu\nu}.\quad (3)$$

Here as usual $\overline{T}_{\mu\nu}$ represents the metric perturbation (the bar operation is defined in Eq. (9) below), $\Box^2$ is the usual, flat-space, d’Alembertian operator and the de Donder gauge $\partial^{\nu} \overline{T}_{\mu\nu} = 0$ is used. When all quantities are periodic with frequency $\omega$, the solution at a point 1, which is located at a distance much greater from the source (i.e. the region where $S_{\mu\nu}$ is expected to be large) than the dimensions of the source itself, is:

$$\overline{h}_{\mu\nu}(\vec{r}_1) = \frac{-\lambda}{4\pi r_1} e^{i\omega r_1} \int d^3 \vec{r}_2 S_{\mu\nu}(\vec{r}_2) e^{-i \vec{K} \cdot \vec{r}_2},\quad (4)$$

with $|\vec{r}_1| \gg |\vec{r}_2|$. Thus the first non-vanishing term in the sequence of integrals corresponding to an expansion of the exponential is a quadrupole one. This approximation holds for nearly all cases of astronomical interests, where wavelengths are much longer than the system’s dimensions, such as for instance double stars or the earth-sun system. Feynman’s attention then concentrated on computing the power radiated by the above waves in the quadrupole approximation which, for a periodic motion of frequency $\omega$, reads ([37], p.11):

$$\frac{1}{5} G \omega^6 \sum_{ij} \left| Q_{ij}' \right|^2,\quad (5)$$

where $Q_{ij}' = Q_{ij} - \frac{1}{3} \delta_{ij} Q_{kk}$, $Q_{ij} = \sum_a m_a R_i^a R_j^a$ being the quadrupole moment of mass, and the RMS average is taken. In particular, for a circularly rotating double star, the result ([2]) is recovered. In sum, Feynman applied the quadrupole formula to a binary system, as Landau and Lifshitz had earlier. Feynman’s treatment was simpler, however, since he managed by a clever trick to avoid referring to the energy-momentum pseudo-tensor (this is the “simpler argument” referred to above). The question of course arises, whether he took inspiration from the book by Landau and Lifshitz, or rather devised his approach in a completely independent fashion.\[23\]

The above treatment is supplemented with a thorough analysis of the effects of a gravitational wave impinging on a device made of two test particles placed on a rod with friction, in this way providing a more complete description of the working principle of the gravitational wave detector previously introduced at Chapel Hill. A second absorber, made of four moving particles in a quadrupole configuration, is also proposed, and shown to be able to absorb energy from a gravity wave acting on it. The same oscillating device is shown to re-radiate waves with the same energy content. Apparently, at Chapel Hill, Feynman had derived the expression for the radiated energy from the detailed study.\[23\]

\[23\]The issue of whether Feynman took inspiration from Landau-Lifshitz book also arises concerning his formulation of Maxwell’s equations, and was briefly considered by the present authors in [20], without reaching a definite conclusion.
of the detector, while in the letter the opposite route is taken (hence he says that the argument is “used in reverse”).

These results went beyond the treatment carried out in the last section of Lecture 16 of the Caltech Lectures ([12], pp. 218-220), where the treatment stops immediately after the solution of the wave equation. An interesting point is that, both in the letter and in the Lectures, Feynman performed the computation twice, first using the methods of quantum field theory to compute the tree level probability for low-energy graviton emission in several scattering processes, and then by solving the classical linearized wave equation (3) to compute the intensity of the emitted waves, of course with agreeing results (in fact the tree approximation of a quantum theory is equivalent to its classical limit). At the end of Lecture 16 of [12], Feynman commented again on the energy content of gravitational waves:

We can definitely show that they can indeed heat up a wall, so there is no question as to their energy content. The situation is exactly analogous to electrodynamics ([12], p. 219).

Interestingly, in the letter, while remembering the Chapel Hill conference he 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?” ([17], p. 14).

Indeed, from what he wrote in the letter it is clear that he considered the issue of whether binary systems radiate as solved since his calculations clearly showed that the phenomenon occurred. Indeed the fact that relativists continued to argue about the binary star problem without reaching an agreement disturbed Feynman, and may have contributed to his loss of interest in the subject.

4 Should gravity be quantized?

One of the most debated questions at the Chapel Hill conference was whether the gravitational field had to be quantized at all. This was a crucial issue in view of the merging of general relativity with the rest of physics, especially with the then thriving field of elementary particle physics, which was of course dominated by quantum mechanics. As remarked by Bergmann ([2], p. 165):

Physical nature is an organic whole, and various parts of physical theory must not be expected to endure in “peaceful coexistence.” An attempt should be made to force separate branches of theory together to see if they can be made to merge, and if they cannot be united, to try to understand why they clash. Furthermore, a study should be made of the extent to which arguments based on the uncertainty principle force one to the conclusion that the gravitational field must be subject to quantum laws: (a) Can quantized elementary particles serve as sources for a classical field? (b) If the metric is unquantized, would this not in principle allow a precise determination of both the positions and velocities of the Schwarzschild singularities of these particles?
Besides the many technical talks and comments, where the main roads to quantum gravity which would have been pursued in the following decades were delineated, much effort was devoted to fundamental and conceptual questions, in view of the feeling that physicists should attempt to keep physical concepts as much as possible in the foreground in a subject which can otherwise be quickly flooded by masses of detail and which suffers from lack of experimental guideposts ([2], p. 167).

Hence, the problem of quantum measurement was discussed at length during the conference. As stated by Bergmann, the main conceptual question was:

What are the limitations imposed by the quantum theory on the measurements of space-time distances and curvature? ([2], p. 167)

or, equivalently

What are the quantum limitations imposed on the measurement of the gravitational mass of a material body, and, in particular, can the principle of equivalence be extended to elementary particles? ([2], p. 167)

The editors of Ref. [2] pointed out that the answer could not be simply given in terms of dimensional arguments, since the Planck mass does not constitute a lower limit to the mass of a particle whose gravitational field can in principle be measured. A simple argument ([2], pp. 167-8) shows in fact that the gravitational field of any mass can in principle be measured, thanks to the “long tail” of the Newtonian force law.

4.1 Unavoidable quantum effects

Feynman’s views concerning such general matters were very close to those of Bergmann (while they disagreed on more technical ones). Indeed, as his interventions in the many discussion sessions clearly show, Feynman was convinced that nature cannot be half classical and half quantum, as he opposed those proposing that maybe gravity should not be quantized. Feynman’s arguments in favor of the quantization of the gravitational field were quite different from the ones that became popular later among most practitioners, involving the assumption that at the Planck scale gravity has to dominate over all other interactions. He thought that it was not necessary to go to such ridiculously high and unfathomable scales to see quantum effects involving gravity. Specifically, he proposed a thought experiment showing that, assuming the validity of quantum mechanics for objects massive enough to produce a detectable gravitational field (an assumption in which he definitely believed, since the opposite would have required a modification of quantum mechanics [59]), then the only way to avoid contradictions is to quantize the gravitational field itself ([2], pp. 250-252). Alongside with the consideration that a mass point giving rise to a classical Schwarzschild field very unlikely would obey Heisenberg’s uncertainty relations, a more pragmatic motivation was the hope (also expressed earlier by Wolfgang E. Pauli) that quantization of the metric would help resolving the divergencies present in
quantum field theory and, in this way, it would have a key relevance also for the theory of elementary particles. This hope underlies, for example, Feynman’s criticism to Deser’s proposal, where all non-gravitational fields should be quantized in a given geometry, while the quantization of the metric field itself would be the final step of the whole procedure [14]. Quoting Feynman:

If one started to compute the mass correction to, say, an electron, one has two propagators multiplying one another each of which goes as $1/s^2$ and are singular for any value of the $g$’s. Therefore, do the spatial integrations first for a fixed value of the $g$’s and the propagator is singular, giving $\delta m = \infty$. Then the superposition of various values of the $g$’s is still infinite ([2], p. 270).

Feynman’s approach to quantum gravity, as we shall see, was instead fully quantum from the beginning.

### 4.2 Toward quantum gravity

The above-mentioned discussions were one of the few places where Feynman was directly involved in the foundations and interpretation of quantum mechanics [59] or, at least, where his thoughts about the subject were expressed. His arguments, which were based essentially on thought experiments, stimulated a wide debate on the measurement problem in quantum mechanics and on the existence and meaning of macroscopic quantum superpositions [2]. The debate further grew in Session VIII of the conference, where the discussion focused on the contradictions eventually arising in the logical structure of quantum theory if quantization of gravity was not assumed. Indeed, as pointed out by DeWitt ([2], p. 244), the choice of the quantum expectation value of the stress-energy tensor of matter fields as the source of the gravitational field may lead to difficulties, since measurements made on the system may change this expectation value and hence the gravitational field.

The validity of the classical theory of gravitation relies on the smallness of fluctuations at the scale where gravitational effects become sizeable. Feynman’s first argument concerned a two-slit diffraction experiment with a mass indicator behind the two-slit wall, i.e. a gravitational two-slit experiment. If one is working in a space-time region whose linear dimensions are of order $L$ in space and $L/c$ in time, the uncertainty on the gravitational potential (divided by $c^2$ so that it is dimensionless and thus homogeneous to a metric) is in general $\Delta g = \sqrt{\frac{\hbar G}{c^2L^2}} = \frac{L_P}{L}$, where $L_P = \sqrt{\frac{\hbar G}{c^3}}$ is the Planck length (as argued for example by Wheeler in [2], pp. 179-180, on the basis of the path integral for the gravitational field). The order of magnitude of the potential generated by a mass $M$ within the spatial part of the considered region is $g = \frac{MG}{c^2L^2}$, hence in this case $\Delta g = \frac{\Delta M G}{c^2L^2}$, from which by comparison with the general expression one could infer a mass uncertainty $\Delta M = \frac{c^2 L_P G}{G} \approx 10^{-5}$ grams, if the time of observations is less than $L/c$. On the other side, by allowing for an infinite time, one would obtain $M$ with infinite accuracy. Feynman deduced that the last option could not take place for a mass $M$ put into the above two-slit apparatus, so that the apparatus would not be able to uncover this difficulty, unless $M$ was at least of order $10^{-5}$ grams. His conclusion was that:

Either gravity must be quantized because a logical difficulty would arise if one did the experiment with a mass of order $10^{-4}$ grams, or else [...] quantum mechanics fails with
masses as big as \(10^{-5}\) grams ([2], p. 245).

The discussion then briefly dealt with some formal matters and with the meaning of the equivalence principle in quantum gravity, with Helmut Salecker proposing a thought experiment which apparently lead to a violation of the equivalence principle in the quantum realm. Finally, due to a remark by Salecker himself, the discussion again focused on the topical question. Salecker pointed out that charged quantized particles might act as a source of an unquantized Coulomb field within an action-at-a-distance picture, perhaps hinting (as commented by an Editor’s Note, [2], p. 249) to a similar situation in the case of a gravitational field. Subsequent considerations by Frederick J. Belinfante suggested the quantization of the static part of the gravitational field as well as of the transverse part (which describes the gravitational radiation), in order to avoid difficulties arising from the choice of an expectation value (of the stress-energy tensor) as the source of the gravitational field, in full agreement with the previous argument by DeWitt. Here, as noted by H. Dieter Zeh [59], Belinfante’s understanding of the wave function as an epistemic concept clearly emerged, since he said:

“There are two quantities which are involved in the description of any quantized physical system. One of them gives information about the general dynamical behavior of the system, and is represented by a certain operator (or operators). The other gives information about our knowledge of the system; it is the state vector [...] the state vector can undergo a sudden change if one makes an experiment on the system.” ([2], p. 250).

Feynman replied with his second thought experiment, a Stern-Gerlach experiment with a gravitational apparatus, which revealed his completely different ideas on the meaning of quantization and on the role of wave function. Feynman’s argument dealt with a spin-1/2 particle going through the apparatus and then crossing one of two counters (denoted as 1 and 2, respectively), each one connected by means of a rod to an indicator, which is a little ball with a diameter of 1 cm, going up or down depending on whether the object arrives at counter 1 or 2, respectively. A quantum mechanical analysis provides, in principle (before making a measurement), an amplitude for the ball up and an amplitude for the ball down. [25] Thanks to its macroscopic size, the ball is able to produce a gravitational field and such a field may be used to move a probe ball. Thus the gravitational field acts as a channel between the object and the observer. This reasoning leads Feynman to the following conclusion about gravity quantization:

Therefore, there must be an amplitude for the gravitational field, provided that the amplification necessary to reach a mass which can produce a gravitational field big enough to serve as a link in the chain does not destroy the possibility of keeping quantum mechanics all the way. There is a bare possibility (which I shouldn’t mention!) that quantum mechanics fails and becomes classical again when the amplification gets far enough, because

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24 This expression reveals that according to Belinfante “the wave function [...] must change for reasons beyond the system’s physical dynamics. He [Belinfante] does not refer to ensembles of wave functions or a density matrix in order to represent incomplete knowledge” ([59], p. 65).

25 As pointed out by Zeh [59], Feynman’s description of the measurement process is very close to the standard measurement and registration device proposal by von Neumann [20].
of some minimum amplification which you can get across such a chain. But aside from that possibility, if you believe in quantum mechanics up to any level then you have to believe in gravitational quantization in order to describe this experiment ([2], p. 251).

To a question by Bondi, who asks ([2], p. 252): “What is the difference between this and people playing dice, so that the ball goes one way or the other according to whether they throw a six or not?”, Feynman answers:

I don’t really have to measure whether the particle is here or there. I can do something else: I can put an inverse Stern-Gerlach experiment on and bring the beams back together again. And if I do it with great precision, then I arrive at a situation which is not derivable simply from the information that there is a 50 percent probability of being here and a 50 percent probability of being there. In other words, the situation at this stage is not 50-50 that the die is up or down, but there is an amplitude that it is up and an amplitude that it is down – a complex amplitude – and as long as it is still possible to put those amplitudes together for interference you have to keep quantum mechanics in the picture ([2], p. 252, our emphasis).

As Zeh notices ([59] p. 67), this is a standard argument against an epistemic interpretation of the wave function.

### 4.3 Wave packet reduction

In the last sentence of the above quote, Feynman focused on the problem of wave function collapse and hints to decoherence as a possible solution. He argued that the wave packet reduction acts somewhere in his experimental apparatus thanks to the amplifying mechanism, so that amplitudes become probabilities in the presence of a huge amount of amplification (via the macroscopic gravitational field of the ball). Then he wondered whether it might be possible to design an experiment in which the wave packet reduction due to the amplification process could be avoided. Subsequent critical remarks by Leon Rosenfeld and Bondi lead Feynman to envision a kind of quantum interference in his experiment, by allowing gravitational interaction between macroscopic balls to be described by means of a quantum field with suitable amplitudes taking a value or another value, or to propagate here and there. Clearly, as suggested by Bondi, one has to remove any irreversible element such as, for instance, the possibility that gravitational links radiate. This is probably another hint to the possible role of dissipation or decoherence in destroying quantum interference, but Feynman and Bondi here only speak about classical irreversibility [59]. The concept of decoherence as source of smearing off phase relations, as well as the transition to the classical picture and its environmental origin, was in fact still unclear at the time (for a historical and research account see Ref. [61] and references therein). By arguing again on gravity quantization in relation to wave function collapse, Feynman claimed that:

There would be a new principle! It would be fundamental! The principle would be: – roughly: **Any piece of equipment able to amplify by such and such a factor** (10⁻⁵ grams or
whatever it is) necessarily must be of such a nature that it is irreversible. It might be true! But at least it would be fundamental because it would be a new principle. There are two possibilities. Either this principle – this missing principle – is right, or you can amplify to any level and still maintain interference, in which case it’s absolutely imperative that the gravitational field be quantized... I believe! or there’s another possibility which I haven’t thought of ([2], pp. 254-255, emphasis in original).

The discussion on possible sources of decoherence went on further, with Feynman assuming that quantum interference might eventually take place with a mass of macroscopic size, i.e. about $10^{-5}$ grams or even 1 gram, and hinting to the possible role of gravity in destroying quantum superpositions. The same argument would be later developed by Feynman in his Lectures on Gravitation [12], where he dealt with “philosophical problems in quantizing macroscopic objects” and commented about a possible gravity-induced failure of quantum mechanics:

I would like to suggest that it is possible that quantum mechanics fails at large distances and for large objects. Now, mind you, I do not say that I think that quantum mechanics does fail at large distances, I only say that it is not inconsistent with what we do know. If this failure of quantum mechanics is connected with gravity, we might speculatively expect this to happen for masses such that $\frac{GM^2}{\hbar c} = 1$, of $M$ near $10^{-5}$ grams, which corresponds to some $10^{18}$ particles ([12], pp. 12-13).

The same problem was pointed out again later ([12], p. 14), where the possibility was put forward that amplitudes may reduce to probabilities for a sufficiently complex object, as a consequence of a smearing effect on the evolution of the phases of all parts of the object. Such a smearing effect could be related to the existence of gravitation. A similar idea was expressed in the end of the letter to Weisskopf [47], where he wrote:

How can we experimentally verify that these waves are quantized? Maybe they are not. Maybe gravity is a way that quantum mechanics fails at large distances.

It is remarkable that, although Feynman often expressed his belief in the quantumness of nature, including gravity, he was nevertheless open to the possibility that gravity may not be quantized, in the absence of experiments able to clear the question.

### 4.4 Feynman-inspired later results

The possibility of a gravity-induced collapse of the wave function, as a solution to the measurement problem in quantum mechanics, can thus be traced back to the broad debate at Chapel Hill about gravity quantization and Feynman’s insights. This is an attractive idea, because gravity is ubiquitous in all existing interactions, and gravitational effects depend on the size of objects, and it triggered a huge amount of investigation (see for example [62], [70]). In particular, the Penrose proposal relies strongly on the conflict between the general covariance of general relativity and the quantum mechanical superposition principle [67], [68]: such a conflict emerges when considering a balanced superposition of
two separate wave packets representing two different positions of a massive object. If the mass $M$ of this object is large enough, the two wave packets represent two very different mass distributions. By assuming that in each space-time one can use the notions of stationarity and energy, while the difference between the time-translation operators gives a measure of the ill-definiteness or uncertainty of the final superposition’s energy, the decay time for the balanced superposition of two mass distributions is:

$$t_D = \frac{\hbar}{\Delta E_{\text{grav}}}.$$  

(6)

Here $\Delta E_{\text{grav}}$ is the gravitational self-energy of the difference between the mass distributions of each of the two locations of the object. Thus, massive superpositions cannot form, because they would decay immediately. Later, Penrose suggested that the basic stationary states into which a superposition of such states decays are stationary solutions of the so-called Newton-Schrödinger equation [69]. More recently, other collapse models (called dynamical collapse models) have been also suggested [71]-[74], where the collapse of the wave function is induced by the interaction with a random source, for instance an external noise source. In the gravitational context, a lot of experimental proposals have been put forward as well, attempting to explore a parameter regime where both quantum mechanics and gravity are significant. Currently, it has been possible to build up a quantum superposition state with complex organic molecules with masses of the order $m = 10^{-22}$ kg [75, 76]. Today prospective typical experiments, revealing gravity-induced decoherence, involve matter-wave interferometers [77]-[79], quantum optomechanics [80]-[83] and magnetomechanics [84]. However, despite the huge theoretical and experimental effort, there is still no consensus on a definitive solution of the fundamental dichotomy between unitary deterministic quantum dynamics and the discontinuous irreversible state collapse following a measurement process, as well as of the problem of the emergence of classical from the quantum world.

4.5 Observers in a closed Universe

Another foundational topic which emerged from discussions on quantum gravity is the role of the observer in a closed Universe. As pointed out by Wheeler:

> General relativity, however, includes the space as an integral part of the physics and it is impossible to get outside of space to observe the physics. Another important thought is that the concept of eigenstates of the total energy is meaningless for a closed Universe. However, there exists the proposal that there is one “universal wave function”. This function has already been discussed by Everett, and it might be easier to look for this “universal wave function” than to look for all the propagators ([2], p. 270).

As already stated in Section 2.1, Wheeler was giving the first presentation of Everett’s relative-state interpretation of quantum mechanics [22] [85], to which Feynman promptly replied with a “many-worlds” characterization:

> The concept of a “universal wave function” has serious conceptual difficulties. This is so since this function must contain amplitudes for all possible worlds depending on all
quantum-mechanical possibilities in the past and thus one is forced to believe in the equal reality of an infinity of possible worlds ([2], p. 270).

The same idea would have been expressed by Feynman some years later in his Lectures on Gravitation ([12], pp. 13-14), where the role of the observer in quantum mechanics was also discussed by making explicit reference to the Schrödinger cat paradox. In particular, an external observer is in a peculiar position because he always describes the result of a measurement by an amplitude, while the system collapses into a well-defined final state after the measurement. On the other hand, according to an internal observer, the result of the same measurement is given by a probability. Thus, a paradoxical situation emerges in the absence of an external observer, most notably in considering the whole Universe as being described by a complete wave function without an outside observer. The Universe wave function obeys a Schrödinger equation and implies the presence of an infinite number of amplitudes, which bifurcate from each atomic event. This implies that an inside observer knows which branch the world has taken, so that he can follow the track of his past. Feynman concluded the argument by raising a conceptual problem:

Now, the philosophical question before us is, when we make an observation of our track in the past, does the result of our observation become real in the same sense that the final state would be defined if an outside observer were to make the observation ([12], p. 14)?

Later in the Lectures on Gravitation ([12], pp. 21-22), Feynman returned briefly to the meaning of the wave function of the Universe and confirmed his “many-worlds” characterization of Everett’s approach by resorting to a “cat paradox on a large scale”, from which our world could be obtained by a “reduction of the wave packet”. He questioned the mechanism of this reduction, the crucial issue being how to relate Everett’s approach and collapse mechanisms of whatever origin. In this connection, it is interesting to quote a comment made by John Preskill in a recent talk about the Feynman legacy (see [86], slide 29):

When pressed, Feynman would support the Everett viewpoint, that all phenomena (including measurement) are encompassed by unitary evolution alone. According to Gell-Mann, both he and Feynman already held this view by the early 1960s, without being aware of Everett’s work. However, in 1981 Feynman says of the many-worlds picture: “It’s possible, but I am not very happy with it”.

Everett’s analysis ([22] is, indeed, the first attempt to go beyond the Copenhagen interpretation in order to apply quantum mechanics to the Universe as a whole. This requires to overcome the sharp separation of the world into “observer” and “observed”, showing how an observer could become part of the system while measuring, recording or doing whatever operation according to the usual quantum rules. Quantum fluctuations of space-time in the very early Universe have also to be properly taken

\[\text{Here Feynman probably hints to a vision of the Universe as constantly splitting into an infinite number of branches, which result from the interactions between its components. Indeed such interactions act as measurements.}\]

\[\text{We do not agree that Feynman was not aware of Everett’s work in the 1960s, since he commented on it at Chapel Hill in 1957.}\]
into account. On the other hand, this approach lacks an adequate description of the origin of the quasi-classical realm as well as a clear explanation of the meaning of the branching of the wave function. A further extension and completion of Everett’s work has been developed by a number of authors [87]-[93] and is today known as *decoherent histories approach to quantum mechanics of closed systems*. Within this formulation neither observers nor their measurements play a prominent role, and the so called *retrodiction*, namely the ability to construct a history of the evolution of the Universe toward its actual state by using today’s data and an initial quantum state, is allowed. The process of prediction requires to select out decoherent sets of histories of the Universe as a closed system, while decoherence in this context plays the same role of a measurement within the Copenhagen interpretation. Decoherence is a much more observer-independent concept and gives a clear meaning to Everett’s branches, the main issue being the identification of mechanisms responsible for it.

5  Gravity as a quantum field theory

In a set of critical comments at Chapel Hill ([2], pp. 272-276), Feynman advocated a non-geometric and field theoretical approach to gravity, which would later be developed in the first part of his Caltech lectures [12]. He imagined a parallel development of history[28], in which the general theory of relativity had not been discovered yet, while the principles of Lorentzian quantum field theory were known. Then, he wondered, how would people deal with the discovery of a new force, namely gravitation? The roots of such an approach can be traced back to Robert H. Kraichnan [94] and Suraj N. Gupta [95] (more details about this fascinating story can be found below, in Section 5.1, and in [96]). Basically, one can use general arguments from field theory and from experiment to infer that gravity (assumed to be mediated by virtual particle exchanges as any other interaction) has to be carried by a massless neutral spin-2 quantum, called the graviton. Then, full general relativity should follow from the quantum theory of a massless neutral spin-2 field[29], as well as from consistency requirements[30]. The same procedure, for the spin-1 case, yields the Maxwell equations. Such an approach is also coherent with Feynman’s views about fundamental interactions [20].

5.1  Quantum gravity research before 1957: a brief account

Before describing Feynman’s work, let us briefly outline the history of quantum gravity prior to the Chapel Hill conference. Although the first embryonic ideas about the interaction of quantum theory and gravity can be traced back at least to Einstein’s first paper on gravitational waves [31], we here focus on the problem of quantizing the gravitational field itself, which began to be seriously tackled only after quantum mechanics was completed and the first papers on field quantization were written. The interested reader can refer to the book [100], as well as to the source book [101] for a comprehensive

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28 In [2], p. 273, Feynman said: “Instead of trying to explain the rest of physics in terms of gravity I propose to reverse the problem by changing history. Suppose Einstein never existed [...]”.

29 As we discuss in more detail below, the linear theory for such a field and its massive counterpart was completely worked out by Markus Fierz and Pauli in Ref. [97], following a previous work by Paul A. M. Dirac [98].

30 See, however, [99].
survey of early work on quantum gravity, including both pioneering work preceding 1930, and the part of the story which we summarize here.

The first attempts to quantize the gravitational field date back to the early 1930s, when the bases for the so-called covariant and canonical approaches were laid. While the idea behind the covariant approach is to build up a quantum field theory of the perturbations $h_{\mu\nu}$ of the metric over a flat Minkowski space, the canonical approach aims at recasting general relativity in the Hamiltonian form (which requires singling out the time direction, thus breaking explicit covariance), and then quantizing it along the usual lines.

A mention of the quantization of the gravitational field in analogy with the electromagnetic field can be found in the first paper on quantum electrodynamics by Werner Heisenberg and Pauli. Their remarks prompted a subsequent attempt by Rosenfeld, who applied a novel method for the quantization of fields with gauge groups, the Hamiltonian formulation of which involves constraints, to the linearized Einstein field equations. In the process, he showed that the gravitational field has to be quantized by using commutators, hence its quanta obey Bose statistics. Despite not completing the program of quantizing linearized general relativity, in his second paper Rosenfeld calculated the gravitational self-energy of a light quantum (finding a divergence) and studied various possible transition processes involving both light quanta and gravitational quanta. But it is worth mentioning that, at the beginning of the 1930s, Rosenfeld did not realize yet that certain peculiar features of gravity would have posed fundamental problems in quantizing the gravitational field. The idea of a gravitational quantum analogous to the photon emerged in the same years (the 1930s), and the name graviton came out for the first time in a 1934 paper by Dmitri I. Blokhintsev and Fëdor M. Gal’perin, although those authors supposed that the gravitational quantum was related to the neutrino. Later, in 1939, Fierz and Pauli, while studying the quantization of fields with arbitrary spin and mass (and of their coupling with the electromagnetic field), realized that a massless spin-2 quantum field obeys equations which formally coincide with the linearized Einstein equations, unveiling an interesting link with Rosenfeld’s work, and at the same time showing that linearized quantum gravity can be developed from purely quantum field theoretical considerations, independently of general relativity. Meanwhile, in 1935, the quantization of the linearized theory had been completely carried out independently by Matvei P. Bronstein by means of...
Fermi’s quantization technique [116] but, unlike Rosenfeld, he soon realized the deep difference between quantum electrodynamics and a quantum theory of gravitation. Bronstein carried out an analysis of the measurements of the linearized Christoffel symbols (which he identified with the components of the gravitational field), modeled on the analogous one carried out by Niels Bohr and Rosenfeld himself for the electromagnetic case [117]. The result was an expression for the minimum uncertainty in a measurement which is a function of the inverse mass density of a test body, so that this uncertainty cannot be made arbitrarily small, since general relativity does not allow the existence of arbitrarily massive bodies in a given volume. He speculated that the validity of such a result would be preserved also in the full nonlinear theory, and that the quantization of the latter would imply a rejection of the usual concepts of space and time. Bronstein’s latter argument was criticized by Jacques Solomon [118], who however put forward his own argument for the same conclusion, by questioning that a quantization method based on the superposition principle would work when the gravitational field is not weak. Interestingly, Solomon’s argument was based on a recent “proof” by Rosen [119] of the nonexistence of non-singular plane gravitational waves in full general relativity (the standard quantization methods that were used at the time were in fact based on expansions of the fields in plane waves). Of course, Rosen’s proof was flawed, since what he found were mere coordinate singularities (there is an interesting analogy with the Einstein-Rosen cylindrical waves [37]). In fact, exact plane wave solutions were found in the late 1950s [49], as we discussed in Section 3.2.

The quantum gravity landscape began to change in the late 1940s [102]. After the great successes of renormalized quantum electrodynamics, the idea that full quantum general relativity could be treated by means of a perturbative expansion around flat space, with nonlinear terms seen as self-interactions of the gravitational field with itself, gained momentum. DeWitt was the first who pursued such an approach in his Ph.D. thesis [120], applying the powerful covariant machinery developed by his advisor Julian S. Schwinger [121] to the computation of the photon gravitational self-energy. Unlike Rosenfeld, he found a vanishing result, in agreement with the requirements of gauge invariance. The already mentioned program of constructing the full theory of general relativity by starting from a free, massless spin-2 field in Minkowski space, which is closely related to this approach to quantization, and which was embraced also by Feynman, started being developed by Kraichnan [94] (who actually began pursuing it back in 1947) as well as Gupta [95] (for further details see [96, 102] and a comment in the following subsection). In the same years, another line of research became popular among relativists, who were just starting to recognize nonlinearity as the essential feature of general relativity (this was in fact one of the aspects that characterized the renaissance of general relativity): in 1949 Bergmann launched the program of canonically quantizing the full nonlinear theory of general relativity (promoting in particular the full metric $g_{\mu\nu}$ to a quantum operator, rather than the deviations from the flat spacetime metric), starting with the development of a Hamiltonian formulation of constrained systems [122, 123] (for historical studies see [124, 125]). The problem of constrained Hamiltonian dynamics was also pursued by Dirac [126] (who was initially motivated by the relation between Hamiltonian dynamics and special relativity [127]); his formalism was then applied to general relativity by Pirani and Alfred Schild [128].
5.2 Nonlinearity and curved spacetime

Let us now focus on Feynman’s approach to the derivation of general relativity from the theory of a spin-2 graviton field. The starting point is the linearized theory for such a field; nonlinearity then arises from the fact that the graviton has to couple universally with anything carrying energy-momentum, including itself. General covariance, along with the usual geometric interpretation of general relativity, comes about merely as interesting and useful (albeit somewhat mysterious) byproducts, which can be seen as related to gauge invariance. In Feynman’s words:

The fact is that a spin-two field has this geometric interpretation; this is not something readily explainable – it is just marvelous. The geometric interpretation is not really necessary or essential to physics. It might be that the whole coincidence might be understood as representing some kind of gauge invariance ([12], p. 113).

Besides the understanding of another part of nature in his own and original way, Feynman sought a fast track toward the quantization of gravity, which he just regarded as the quantization of another field. In particular, once general relativity had been understood as the result of the interactions of spin-2 quanta, quantum gravity effects would be taken into account by including diagrams with closed loops. By treating gravity in this way, difficult conceptual and technical issues concerning the meaning of quantum geometry would not show up ([129], p. 377).

Feynman’s justification for his starting point was the fact that gravity behaves as a $1/r^2$ force, through which like charges (i.e. masses) attract. By trial and error, one would then arrive at the hypothesis that such a force is mediated by a new massless spin-2 field (see Section 5.5 for Feynman’s later arguments for this statement). Then the action would be

$$\int \left( \frac{\partial A_\mu}{\partial x_\nu} - \frac{\partial A_\nu}{\partial x_\mu} \right)^2 d^4x + \int A_\mu j^\mu d^4x + \frac{m}{2} \int z^2 ds + \frac{1}{2} \int T_{\mu\nu} h^{\mu\nu} d^4x + \int (\text{second power of first derivatives of } h),$$

(7)

where $h_{\mu\nu}$ is the new field, satisfying second order equations of the kind:

$$h^{\mu\nu,\sigma}_\sigma - 2\bar{h}^{\mu\nu}_\sigma \eta^{\sigma\sigma} = T^{\mu\nu}. \quad (8)$$

Here the bar operation on a general second rank tensor $X_{\mu\nu}$ is defined as:

$$\bar{X}_{\mu\nu} = \frac{1}{2} (X_{\mu\nu} + X_{\nu\mu}) - \frac{1}{2} \eta_{\mu\nu} X^{\sigma\sigma}. \quad (9)$$

The corresponding equation of motion for particles moving in this field would be:

$$g_{\mu\nu} \ddot{z}^\nu = - [\rho_\sigma, \ddot{z}],$$

(10)

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36 The original equations appearing in [2] are schematic and present several index and sign mistakes; however, this fact does not affect the reasoning we are reporting; in the following we write the correct versions of such equations.
where $g_{\mu \nu} = \eta_{\mu \nu} + h_{\mu \nu}$, and $[\rho \sigma, \mu]$ are the Christoffel symbols of the first kind. Eqs. (5) are nothing but the linearized Einstein equations. Further pursuing the analogy with electromagnetism (for which, as said, the same approach can be adopted), a crucial fact to be noticed is that, in that case, the Maxwell equations automatically imply that the electric current $j$ is conserved. Then, also in the gravitational case, a suitable $T_{\mu \nu}$, such that the condition $\partial_\nu T^\mu_\nu = 0$ is satisfied, must be found. However, a problem arises if particles move according to Eq. (10) or, what is the same, if the field $h_{\mu \nu}$ is coupled to the matter, since the corresponding $T_{\mu \nu}$ does not obey to a conservation law. Thus, the linear theory leads to a consistency problem. The addition of a further contribution $t_{\mu \nu}$ to the stress-energy tensor due to the gravitational field itself, thus replacing the term $\int T_{\mu \nu} h^{\mu \nu} d^4x$ in the action (7) with $\int (T_{\mu \nu} + t_{\mu \nu}) h^{\mu \nu} d^4x$, does not solve the problem, since a variation of $h$ would give new terms. According to Feynman (see [2], p. 274 and, for a more detailed derivation [12], pp. 78-79), a working solution could be obtained only by adding to the action a nonlinear third order term in $h_{\mu \nu}$, which gives for $T_{\mu \nu}$ the following equation:

$$g_{\mu \lambda} T^{\mu \nu, \nu} = - [\rho \nu, \lambda] T^{\rho \nu}.$$  

(11)

One could then proceed to the next order approximation, and so on. However, as a matter of fact, finding the general solution of Eq. (11) is a very difficult task, which may be pursued by finding an expression that is invariant under the following infinitesimal transformation of the tensor field $g_{\mu \nu}$:

$$g'_{\mu \nu} = g_{\mu \nu} + g_{\mu \lambda} \frac{\partial A^\lambda}{\partial x^\nu} + g_{\nu \lambda} \frac{\partial A^\lambda}{\partial x^\mu} + A^\lambda \frac{\partial g_{\mu \nu}}{\partial x^\lambda}.$$  

(12)

Here the 4-vector $A^\lambda$ is the generator of the transformation. This is a geometric transformation on a Riemannian manifold, hence one might finally argue that geometry gives the metric. However, as noticed by Feynman,

this would be a marvelous suggestion but it would be made at the end of the work and not at the beginning. What does one gain by looking at the problem in this manner? Obviously, one loses the beauty of geometry but this is not primary. What is primary is that one had a new field and tried his very best to get a spin-two field as consistent as possible ([2], p. 275).

Feynman would later develop the above sketched procedure in great detail, managing in the end to obtain Einstein’s full nonlinear gravitational field equations. He presented the full derivation in his graduate course on gravitation ([12], lectures 3-6). In that course, after completing the task and before switching to applications,[38] he devoted some lectures to the usual geometric approach to gravity ([12], Lectures 7-10). He had already shown that his formalism was able to reproduce physical effects that

[37] In fact, the stress-energy tensor $T_{\mu \nu}$ has been specified only in terms of matter. As such it does not include the energy of the gravitational field itself. This can be taken into account only as a nonlinear effect (gravity has to couple with itself), which would be important also in explaining the precession of the perihelion of Mercury (see the discussion in [12], p. 75).

[38] These include the Schwarzschild solution and wormholes (Lecture 11), cosmology (Lectures 12-13), supermassive stars (Lecture 14) and black holes (Lecture 15), closing with the already cited Lecture 16 on gravitational radiation.
in the standard picture are ascribed to curved spacetime geometry. For example ([12], pp. 66-69) Feynman showed, by studying the action of a scalar field, that a constant weak gravitational field, described by the metric component $g_{44} = 1 + \epsilon$, $g_{ii} = -1$, $i = 1, 2, 3$, is exactly reproduced by substituting $t \rightarrow t' = t\sqrt{1 + \epsilon}$, i.e. by time dilation. He also noticed that this effect is pivotal in getting the right precession of Mercury’s perihelion, which he had computed previously ([12], pp. 63-65) without taking it into account and hence getting a result which was $4/3$ of the right one.

Feynman was of course intrigued by the double nature of gravitation, and tried to link his approach to the usual one:

Let us try to discuss what it is that we are learning in finding out that these various approaches give the same results ([12], p. 112).

In particular, he discussed the issue of flatness of space, since

The point of view we had before was that space is describable as the space of Special Relativity [...] there might be gravity fields $h_{\mu \nu}$ which have the effect that rulers are changed in length, and clocks go at faster or slower rates. So that in speaking of the results of experiments we are forced to make distinctions between the scales of actual measurements, physical scales, and the scales in which the theory is written [...] It may be convenient in order to write a theory in the beginning to assume that measurements are made in a space that is in principle Galilean, but after we get through predicting real effects, we see that the Galilean space has no significance. ([12], p. 112).

To better explain the situation, he considered the analogy with an observer making length measurements on a hot plate, since a ruler can be affected by temperature. However, in this situation the observer can resort to a different instrument, such as light, which is not affected by temperature. On the other hand, gravity is universal, so

we know of no scale that would be unaffected–there is no “light” unaffected by gravity with which we might define a Galilean coordinate system. Thus, all coordinate systems are equivalent, and they differ only in that different values for the fields are necessary for the description of clock rates or length scales ([12], p. 112-113).

In Section 8.3 of [12], Feynman explored possible directions to understand how gravity can be both geometry and a field, and argued (see the quote at the beginning of this Section) that such a link is provided by gauge invariance. In order to connect gravity and gauge invariance, one may look for a procedure to obtain the invariance of the equations of physics under spacetime dependent coordinate displacements. This amounts to adding to the Lagrangian new terms involving a gravitational field. The latter thus emerges as the gauge field enforcing invariance with respect to local displacements.

5.3 Relation of Feynman’s approach to analogous work

Unlike previous partial attempts which addressed only the linearized quantum theory [97, 104, 105, 112] or iterative arguments, able in principle to generate infinite nonlinear terms in the Lagrangian as well
as in the stress-energy tensor, but still incomplete [95]. Feynman succeeded in obtaining the full nonlinear Einstein equations by means of a consistency argument. According to Preskill and Kip S. Thorne [96], it is likely that Feynman was completely unaware of Kraichnan’s work as well as Gupta’s, thus he devised this method independently, besides getting more complete results.

Other people who pursued analogous approaches to gravity include Steven Weinberg, whose approach was quite different from Feynman’s one, being based on analyticity properties of graviton-graviton scattering amplitudes (Weinberg also derived Maxwell’s equations along the same lines) [130]-[131], and Deser [132]-[135]. Unlike Weinberg’s, Deser’s approach was closely analogous to Feynman’s one, but more elegant, and it was in fact the first published completion of the program started with Kraichnan and Gupta. Moreover, Deser applied his method also to Yang-Mills theory. The relation of Lorentzian spin-2 field theories with general covariance was later investigated in a more rigorous and general way by Robert M. Wald [136].

5.4 A neutrino-induced gravity?

Feynman concluded his comments at Chapel Hill with some speculations on a possible theory of gravitation built on already known fields, i.e. not relying on a brand new spin-2 field, a promising candidate being the neutrino, described by a weakly coupled field with (at that time) zero rest mass. But how could a gravitational-like force be obtained by exchange of neutrinos? The exchange of a single neutrino is ruled out by its half-integer spin, which requires orthogonal initial and final states; similarly, exchange of two neutrinos has to be discarded, because the resulting potential would fall off faster than $1/r$. A possible solution emerges from exchanging one neutrino between two bodies while, in turn, each body exchanges one neutrino with the rest of the Universe, located at a fixed distance. This situation, however, gives rise to a logarithmic divergence in the potential, and a higher order divergence would be obtained if one considered, for instance, four neutrino processes. Thus, Feynman was led to the conclusion that a theory built of neutrinos would not be viable: “This is obviously no serious theory and is not to be believed” ([2], p. 276). The inadequacy of a theory of gravitation involving neutrinos would be later discussed in much more detail in the Lectures on Gravitation ([12], Lecture 2, pp. 23-28).

5.5 Further work

After 1957, Feynman’s investigations concentrated on the quantum theory of gravity, addressing in particular the issues of loop diagrams, renormalization and unitarity, and also on the applications of relativistic gravity to astrophysics and cosmology [39]. These applications are described, as we saw, in the Caltech lectures [12], and further discussed in the Hughes lectures [15]. Also in this case, Feynman obtained some original results. However, we do not address applicative topics in this paper, leaving them to a future publication.

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[39] Such applications, indeed, had meanwhile begun to flourish, with the discovery of a wealth of phenomena, such as the cosmic microwave background, pulsars and quasars (see e.g. [137] and references therein).
Concerning quantum gravity, Feynman gave a preliminary assessment of his progress at the La Jolla conference\footnote{The International Conference on the Theory of Weak and Strong Interactions was held in June 14-16 1961 at the University of California, San Diego, in La Jolla. We recall that, here, Geoffrey F. Chew gave his celebrated talk on the S-matrix \cite{chew1961}, while an afternoon session was devoted to the theory of gravitation, with Feynman reporting on his work on the renormalization of the gravitational field and recognizing non-unitarity as the main difficulty, shared also by Yang-Mills theory.} in 1961 \cite{feynman1961} and in the already mentioned letter to Weisskopf \cite{weisskopf1961} where, consistently with his views, classical gravitational radiation is mainly discussed in a quantum field theoretical framework, but within the tree approximation, noting that neglecting radiative corrections, the related problems with divergencies and unitarity are not present or, in his words (\cite{weisskopf1961}, p.1), “without the radiation correction there is no difficulty”. A fuller account was given in the famous 1963 Warsaw talk (see next Section). Interestingly enough, the written version of that talk was the first paper that he published on gravity, despite an interest which at the time had already been going on for a decade. After that, Feynman only published two more papers on the subject \cite{feynman1963, feynman1965}, in the Festschrift for Wheeler’s 60th birthday \cite{wheeler1963}, in which he completed the discussion of his Warsaw paper, giving many details. Apparently, the Wheeler festschrift papers present research performed several years before their publication, which Feynman had been reluctant to publish \cite{feynman1966} due to its presumed incompleteness\footnote{Also, starting from the late 1960s, Feynman became more and more absorbed in studying partons and strong interactions, which were his main interest in the 1970s.}. For the same reason, he did not authorize the distribution of the notes for the last 11 lectures, addressing the quantization of gravity, which are therefore not included in Ref. \cite{feynman1962}. As already remarked, the 1966-67 Hughes lectures on astronomy, astrophysics and cosmology \cite{feynman1966} include a presentation of general relativity. Notably, an elementary discussion of this topic is given also in the undergraduate Caltech lectures \cite{feynman1955}.

Both in the Caltech and in the Hughes lectures, Feynman gave some further justification for the choice of a spin-2 field, based on the following observation. Unlike the electric charge, which is the source of the electromagnetic force, the source of the gravitational force – energy-momentum – is not a relativistic invariant, but rather grows with the velocity. Feynman noted that the charge associated with a spin-0 field would instead decrease with the velocity. This result can be traced back to an old argument by Einstein (which he never published, but was recalled by him in \cite{einstein1912}), according to which, in a Lorentz-covariant scalar theory of gravity (which is what must be expected to descend from a Lorentzian quantum field theory of spin-0 particles), the vertical acceleration of a body would depend on its horizontal velocity (and also on its internal and/or rotational energy\footnote{This was the reason for Einstein’s initial rejection of Lorentz-invariant theories of gravity based on a scalar field, and for his criticism toward the first theory proposed by Gunnar Nordström \cite{nordstrom1913} (see \cite{nordstrom1913} for details).}). Moreover, a scalar theory of gravity cannot account for the observed phenomenon of light deflection. Hence the possibility of a spin-0 field is ruled out. Feynman thus singled out the spin-2 case.

6 Quantum corrections

The issue of loop corrections and renormalization in quantum gravity is publicly addressed by Feynman for the first time (to the best of our knowledge) in 1961, at the already mentioned Conference in La
Here he recognized nonlinearity as a source of difficulty for both gravitation and Yang-Mills theory. Basically, the sources of the gravitational field are energy and momentum, which are locally conserved, and the gravitational field carries energy and momentum itself, so it is self-coupled. Similarly, the source of a Yang-Mills field is the isotopic spin current, which is locally conserved, and the Yang-Mills field carries isotopic spin itself, and thus it is also self-coupled. In both cases, the result is a nonlinear field theory.

6.1 Attacking the problem

As anticipated, all of Feynman’s results on quantum gravity are essentially contained in the already mentioned Ref. [3], a report of the talk given at the International Conference General Relativity and Gravitation (GR3), held in Warsaw in 1962 and in the two Wheeler festschrift papers [140, 129]. According to his original strategy, in [3] Feynman did not dwell on the problem of the quantization of space-time geometry, but rather constructed a quantum field theory for a massless spin-2 field – the graviton – and then worked out the results at different perturbative orders, with quantum corrections being taken into account by including loop diagrams. Since the focus was on the quantum theory, the Einstein equations and the corresponding action were assumed from the beginning, rather than derived as in the Lectures [12], and then quantized by adopting a standard procedure.

In the introduction of [3], Feynman outlined his aims and approach once again:

My subject is the quantum theory of gravitation. My interest in it is primarily in the relation of one part of nature to another. There’s a certain irrationality to any work in gravitation, so it’s hard to explain why you do any of it; for example, as far as quantum effects are concerned let us consider the effect of the gravitational attraction between an electron and a proton in a hydrogen atom; it changes the energy a little bit. Changing the energy of a quantum system means that the phase of the wave function is slowly shifted relative to what it would have been were no perturbation present. The effect of gravitation on the hydrogen atom is to shift the phase by 43 seconds of phase in every hundred times the lifetime of the Universe! [...] I am limiting myself to not discussing the questions of quantum geometry nor what happens when the fields are of very short wave length. [...] I suppose that no wave lengths are shorter than one-millionth of the Compton wave length of a proton, and therefore it is legitimate to analyze everything in perturbation approximation; and I will carry out the perturbation approximation as far as I can in every direction, so that we can have as many terms as we want ([3], p. 697).

Again, Feynman advocated a substantial unity of nature, which required to reconcile gravity and quantum mechanics, although he admitted that the work had some irrationality in it due to the
smallness of the effects. Consistently with what he said at Chapel Hill concerning the choice between mathematical rigor and thought experiments, he chose the latter, pursuing a perturbative approach, solving simple specific problems and later switching to more complex ones:

So please appreciate that the plan of the attack is a succession of increasingly complex physical problems; if I could do one, then I was finished, and I went to a harder one imagining the experimenters were getting into more and more complicated situations (3, p. 698).

6.2 A perturbative approach

As said, the starting point is the Einstein-Hilbert Lagrangian for gravity coupled to scalar matter:

I started with the Lagrangian of Einstein for the interacting field of gravity and I had to make some definition for the matter since I’m dealing with real bodies and make up my mind what the matter was made of; and then later I would check whether the results that I have depend on the specific choice or they are more powerful (3, p. 698). I can do only one example at a time. I took spin zero matter.

The metric is split in the following way:

\[ g_{\mu\nu} = \delta_{\mu\nu} + \kappa h_{\mu\nu}, \]  

which allows, upon substitution and subsequent expansion, to cast the Lagrangian in the form:

\[ L = \int \left( h_{\mu\sigma,\nu} h_{\mu\sigma,\nu} - 2 \overline{h}_{\mu\sigma,\nu} \overline{h}_{\mu\sigma,\nu} \right) \, d\tau + \frac{1}{2} \int \left( \phi_{,\mu} h_{,\mu} - m^2 \phi^2 \right) \, d\tau + \kappa \int \left( h_{,\mu} \phi_{,\mu} - m^2 \phi^2 \right) \, d\tau + \kappa^2 \int \left( h \phi \phi \right) \, d\tau + \ldots \]  

(14)

Here again the bar operation (9) is used, and a schematic notation is adopted for the highly complex higher order terms. The first two terms are the free Lagrangians of the gravitational field and of matter, respectively. The first step is to solve the problem classically, which is carried out by varying the Lagrangian (14) with respect to \( h \) and, then, to \( \phi \). The following equations of motion with a source term are obtained:

\[ h_{\mu\sigma,\nu} - \overline{h}_{\sigma,\mu,\nu} = \bar{\Sigma}_{\mu\nu} (h, \phi), \]  

\[ \phi_{,\sigma} - m^2 \phi = \chi (\phi, h). \]  

(15)

(16)

The next step consists on obtaining the propagators, by following a procedure analogous to that used in electromagnetism. But soon Feynman realized that Eq. (15) is singular, so he resorted to the invariance of the Lagrangian under the transformation:

\[ h'_{\mu\nu} = h_{\mu\nu} + 2\xi_{,\mu,\nu} + 2 h_{,\mu} \xi_{,\nu} + \xi_{,\sigma} h_{\mu\nu,\sigma}, \]  

(17)
where $\xi_\mu$ is arbitrary. As a consequence, the consistency of Eq. (15) requires the source $S_{\mu\nu}$ to have zero divergence, since the symmetry (17) implies the identical vanishing of the divergence of the barred left hand side of Eq. (15). By making the simple gauge choice $T_{\mu\sigma, \sigma} = 0$, finally the law describing the gravitational interaction of two systems by means of the exchange of a virtual graviton can be obtained. In order to achieve a higher accuracy, one has then to work out radiative corrections. Besides working out the propagator, Feynman gave other examples of calculations with diagrams, explicitly computing the amplitude for the coupling of two particles to a graviton, namely an interaction vertex, and finally considering the gravitational analogue of the Compton effect, where the photon is replaced by a graviton.

6.3 One-loop corrections

Feynman then switched to more complex situations, which required to go beyond the tree-diagram approximation:

However the next step is to take situations in which we have what we call closed loops, or rings, or circuits, in which not all momenta of the problem are defined ([3], pp. 703-704).

Unfortunately, the inclusion of loops brings in several new conceptual issues, as clearly expressed by Feynman’s words:

This made me investigate the entire subject in great detail to find out what the trouble is. I discovered in the process two things. First, I discovered a number of theorems, which as far as I know are new, which relate closed loop diagrams and diagrams without closed loop diagrams (sic) (I shall call the latter diagrams “trees”). The unitarity relation which I have just been describing, is one connection between a closed loop diagram and a tree; but I found a whole lot of other ones, and this gives me more tests on my machinery. So let me just tell you a little bit about this theorem, which gives other rules. It is rather interesting. As a matter of fact, I proved that if you have a diagram with rings in it there are enough theorems altogether, so that you can express any diagram with circuits completely in terms of diagrams with trees and with all momenta for tree diagrams in physically attainable regions and on the mass shell. The demonstration is remarkably easy ([3], p. 705).

In the discussion section of [3] (pp. 714-717), in answering DeWitt’s questions about the statement of the tree theorem and the nature of the proof for the one-loop case, Feynman gave more details about his arguments. The first seed of his idea was related to the computation of the self-energy in quantum electrodynamics and, in particular, of the Lamb shift of the hydrogen atom, but he really worked out the whole machinery when dealing with the quantum theory of gravitation.

By working out in detail one-loop calculations, Feynman soon realized that unitarity was lost because some contributions arising from the unphysical longitudinal polarization states of the graviton

\footnote{In Feynman’s approach to quantum field theory propagators are just Green’s functions for the classical field equations, with suitable boundary conditions.}
did not cancel. As suggested by Gell-Mann, he considered a similar problem in the simpler context of Yang-Mills theory, and found the same pathologic behavior:

But this disease which I discovered here is a disease which exist in other theories. So at least there is one good thing: gravity isn’t alone in this difficulty. This observation that Yang-Mills was also in trouble was of very great advantage to me. [...] the Yang-Mills theory is enormously easier to compute with than the gravity theory, and therefore I continued most of my investigations on the Yang-Mills theory, with the idea, if I ever cure that one, I’ll turn around and cure the other (3, p. 707).

In order to solve this tricky issue, Feynman succeeded in showing that any diagram with closed loops can be expressed in terms of sums of on shell tree diagrams, notwithstanding the fact that the process of opening a loop by cutting a graviton line implies replacing a virtual graviton with a real transverse one. This is the very content of his tree theorem (which was fully treated in [140]). In order to guarantee gauge invariance, one has to sum the whole set of tree diagrams corresponding to a given process. Feynman then hinted to the general problem, and commented that his novel procedure works fine for the one-loop case:

The question is: Can we express the closed ring diagrams for some process into a sum over various other processes of tree diagrams for these processes? Well, in the case with one ring only, I am sure it can be done, I proved it can be done and I have done it and it’s all fine. And therefore the problem with one ring is fundamentally solved; because we say, you express it in terms of open parts, you find the processes that they correspond to, compute each process and add them together (3, p. 709).

It is of interest to notice that Feynman’s tree theorem has recently been revived in the context of advanced perturbative calculations and generalized unitarity (see e.g. [147]-[150]).

Finally, Feynman asked himself how to get the same result by integrating the closed loop directly, promptly giving the answer. On the one hand, a further term (like a mass term) has to be added to the Lagrangian in order to make it non-singular, but such a term breaks gauge invariance; on the other hand a contribution, obtained by taking a ghost particle (with spin-1 and Fermi statistics) going around the ring and artificially coupled to the external field, has to be subtracted. In this way unitarity and gauge invariance would be recovered. The same calculation can be carried out for the Yang-Mills theory, but in this case the ghost particle must have spin-0 (all the results on Yang-Mills theory would be written up in Ref. [129]). This discovery would have been of seminal importance for gauge theories. Indeed, Feynman’s formalism was later fully developed by DeWitt who, inspired by Feynman’s talk in Warsaw and by the discussion that followed, solved the problem of extending it to two [151] and arbitrarily many [152, 153] loops, while Ludvig D. Faddeev and Victor N. Popov [154] derived the extra terms to be added to the Lagrangian in a much simpler way, by means of a functional integral quantization rather than Feynman diagrams.

46This circumstance is also mentioned in Ref. [10].
6.4 Renormalizability

Feynman’s conclusive remarks about quantum corrections deal with the possibility of extending the above results to higher orders (e.g., two or more loops):

Now, the next question is, what happens when there are two or more loops? Since I only got this completely straightened out a week before I came here, I haven’t had time to investigate the case of 2 or more loops to my own satisfaction. The preliminary investigations that I have made do not indicate that it’s going to be possible so easily gather the things into the right barrels. It’s surprising, I can’t understand it; when you gather the trees into processes, there seems to be some loose trees, extra trees (3, p. 710).

Clearly, he did not know how to manage the problem. The same feeling was expressed in the Lectures on Gravitation:

I do not know whether it will be possible to develop a cure for treating the multi-ring diagrams. I suspect not – in other words, I suspect that the theory is not renormalizable. Whether it is a truly significant objection to a theory, to say that it is not renormalizable, I don’t know (12, Lecture 16, pp. 211-212).

In the same lecture, Feynman pointed out that the Yang-Mills case constituted a simpler context in which to tackle the same difficulties, whose source is mainly the lack of unitarity of some sums of diagrams. Then he argued for a substantial non-renormalizability of the theory, probably as a consequence of these difficulties.47 In fact, Yang-Mills theory was later shown to be renormalizable 157–160, while the divergences of gravity proved to be too strong to be tamed by renormalization 161–164, confirming Feynman’s suspect in that case. It is noteworthy that, against the common lore of the time, Feynman was not convinced that non-renormalizability meant that a theory was inconsistent.48 As he himself declared in one of his last interviews, given in January 1988 (quoted in 165, p. 507):

The fact that the theory has infinities never bothered me quite so much as it bother others, because I always thought that it just meant that we’ve gone too far: that when we go to very short distances the world is very different; geometry, or whatever it is, is different, and it’s all very subtle.

Modern views on quantum field theory, which emerged in the 1970s, indeed see non-renormalizability to be not a significant objection against a theory, but rather as a signal that the theory loses validity at energies higher than a certain scale, i.e., it is an effective field theory (see for example 166, 167).

We point out that, to the best of our knowledge, it is not clear if here Feynman hinted to a link between non-unitarity and non-renormalizability issues. We now know that there is no such link, since while unitarity at arbitrarily many loops would have been shown later by DeWitt 152, 153, in the case of gravity this does not imply renormalizability. Conversely, modified theories of gravity whose action is quadratic in the curvature have been shown to be renormalizable, but not unitary 155 (see however 156 for a proposal of a quantum theory of gravity which is both unitary and renormalizable).

This is also recalled by Gell-Mann in Ref. 10, where he states that “he was always very suspicious of unrenormalizability as a criterion for rejecting theories”.

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for historical discussions), which can however be used to make predictions below that scale. As it is now well understood, this is just the case of gravity (see e.g. [168] and references therein). However, Feynman apparently never embraced this view. In [86] (slide 37), Preskill remembers:

I spoke to Feynman a number of times about renormalization theory during the mid-80s (I arrived at Caltech in 1981 and he died in 1988). I was surprised on a few occasions how the effective field theory viewpoint did not come naturally to him". [...] Feynman briefly discusses in his lectures on gravitation (1962) why there are no higher derivative terms in the Einstein action, saying this is the “simplest” theory, not mentioning that higher derivative terms would be suppressed by more powers of the Planck length.

7 The Hughes lectures

The 1966-67 Hughes lectures[49] on Astronomy and Astrophysics[15] are of great interest not only as a further example of Feynman’s curiosity in action in a different field, but also because they contribute to our understanding of Feynman’s ideas about gravity. Indeed, an important part of them is devoted to introducing the elements of general relativity, with the aim of applying it to astrophysics and cosmology. In the following we shall therefore focus on the third chapter, which is the relevant one[50].

The remaining chapters concern the internal structure of stars, their evolution, active galactic nuclei, and the solar system.

The lectures begin with an overview of the subject matter, organized by growing scale, i.e. from the solar system to quasars. After that, Feynman deals with the Universe as a whole, namely cosmology. This is the subject of the last part of Chapter 1 and of Chapter 2 of the notes, in which a model Universe treated using Newtonian gravity is discussed. Then, in Chapter 3, Feynman started discussing general relativity, by criticizing the geometric approach (cf. the quotation we reported in Section 2.1) and giving some motivation about why gravity should be described by a spin-2 field, along the lines of the Caltech lectures[12] ([15], pp. 30-32). After that he switched to the usual, Einsteinian views. His choice was probably motivated by the fact that the field theoretical derivation would be both too advanced for the audience, and too long for a course whose main focus was on applications. For each step he properly credited Einstein, but, interestingly, he often gave a quite original motivation, different from Einstein’s one (this is especially intriguing for the case of light deflection, as we discuss below). He explained the different notions of mass, the equivalence principle (stated by the usual elevator thought-experiment), and gravitational redshift ([15], pp. 32-35). Then ([15], p. 36) he included an interesting comment on the well-known problem of the consistency of the principle of equivalence with...
the fact that accelerated electric charges radiate. This problem had been solved a few years before Feynman’s lectures, by theorists like Fritz Rohrlich and DeWitt, who showed that the equivalence principle is not violated by realizing that the usual laws of electrodynamics hold in inertial frames, while when transformations between accelerating frames are involved, whether a charge radiates or not is actually an observer-dependent statement. Feynman’s aim was to use these results to argue on physical grounds in favor of the modification of electromagnetism in a gravitational field. This had been discussed more explicitly in the Lectures on Gravitation, where Feynman declared: “Clearly, some interaction between gravity and electrodynamics must be included in a better statement of the laws of electricity, to make them consistent with the principle of equivalence”. Indeed, such a phenomenon is peculiar to electrodynamics in the presence of a gravitational field (another one, he noticed, are the curved trajectories of light rays). In fact, Feynman was just motivating the standard modification of the inhomogeneous Maxwell’s equations on curved spacetime, which is induced by the substitution of the ordinary derivatives with covariant ones (this is the minimal coupling with the gravitational field), but not in the purely geometric and formal way which is commonly found in textbooks.

In a very interesting discussion, Feynman used a variational approach to show that if a traveling clock maximizes the time elapsed, then it satisfies the usual Newtonian equation of motion of a particle subject to the gravitational acceleration, i.e. if it is freely falling. This is of course a “flat space” version of the geodesic principle. The same discussion is present in the Caltech lectures, but without any calculation. This simple computation provided a motivation for the relativity of time in a gravitational field. Then Feynman argued that if time intervals are relative, then “because of the space-time relationship”, space distances must be relative as well, hence he introduced position dependent coefficients in the expression of the usual proper time interval. This further required a “replacement of Newton’s simple law”, which is the geodesic equation (this equation is not written down in its general form, though). These steps had been given in more detail in the Lectures (pp. 137-139). This discussion was followed by the interpretation of the position dependent coefficients as the components of the metric tensor of curved spacetime, a lengthy discussion of curved two-dimensional geometry, and of Einstein’s equations as a generalization of the Poisson equation. The latter material was presented in considerable less detail than in the Lectures. After that, Neer included some additional material on spacetime, curved metrics and the relation with accelerated frames taken from Chapter 2 of Weber’s book. This book had not been recommended by Feynman since, as Neer writes in the preface of the Lectures, he “never called out a reading reference”.

51 This topic is the source of apparent paradoxes (see e.g. for a review). For example, a charge sitting at rest in a gravitational field is not seen to radiate, despite being indistinguishable from a uniformly accelerated charge according to the equivalence principle, thus allowing an observer at rest to distinguish gravitational pull from other kinds of acceleration.

52 It was later shown that the radiation from a uniformly accelerated charge is entirely emitted beyond the Rindler horizon of a co-accelerating observer, which therefore cannot observe it.

53 “So we have a real mess on our hand with 10 potentials all coupled together. So we must try to put some order to them. [...] We need some geometrical interpretation” (p. 41).
The next topic is a description of the Schwarzschild solution of the field equations ([15], pp. 47-49). There is no full derivation of it, since Feynman had not developed all the necessary tools in the previous lectures, therefore Neer refers again to Weber’s book [176], (pp. 56-60), for the missing steps. After a computation of gravitational redshift in the Schwarzschild metric ([15], pp. 50-51), there was a lengthier description of the maximum proper time principle to find trajectories in a gravitational field, which is then written down explicitly for the Schwarzschild metric case and applied first to radial motion of light ([15], p. 52), and then to a quite detailed discussion of the orbits ([15], pp. 53-54b). By contrast, in the Caltech lectures, such a computation had been performed in the perturbative approach ([12], pp. 59-61), and repeated for the full Schwarzschild metric only for radial geodesics ([12], p. 201). Then, Feynman switched to light bending by the Sun. However, instead of adapting the previous discussion to unbound orbits of a massless particle, he computes the deflection of a particle in Newtonian gravity, and then puts $v = c$ ([12], p. 54c). The result here obtained, which is:

$$\theta = \frac{2GM_S}{ac^2}$$

where $M_S$ is the solar mass and $a$ the impact parameter (which is the radius of the Sun for a grazing light ray), is thus half of the correct one. Feynman then claimed that the correct result had been obtained by Einstein by assimilating space with a varying gravitational potential to a medium with a varying index of refraction. This statement is actually wrong. While it is true that Einstein, back in 1911, discussed light deflection in this way [177], he actually obtained a result coinciding with the Newtonian one [18]. In fact, at that time he still had to realize that space was curved, and it is just space curvature which gives the “other half” of the result. Later, in 1916 [178], Einstein got the correct result by studying null geodesics. Quite interestingly, Feynman asserted that Einstein got the following result for a finite mass particle:

$$\theta = \frac{2GM_S}{av^2} \left(1 + \frac{v^2}{c^2}\right)$$

which of course for $v = c$ gives the correct deflection angle. The reference to Einstein is not correct, since as far as we know Eq. (19) does not appear in any of Einstein’s writings. Instead, this statement is quite likely connected to a discussion contained in the Caltech lectures ([12], p. 41), where Feynman heuristically explained the factor of 2 as due to magnetic-like gravitational interactions, which become equally important as the static ones for an object moving with the speed of light.

The last topic is a qualitative discussion of the Schwarzschild radius and of black holes (which are called “black stars”, as appropriate for those times) ([15], pp. 55-58).

### 7.1 An unpublished approach to the gravitational interaction

The Hughes lectures on Astronomy and Astrophysics [15] contain some statements that may hint to the fact that Feynman had been thinking at gravity in a way analogous to his treatment of electromag-
netism, recently uncovered in [16] and [179] and discussed in detail in [19] and [20]. Indeed, Feynman
developed a formulation of electromagnetism that was relativistic from scratch, and again having its roots in
his belief that nature is fundamentally quantum, with classical fundamental interactions emerging from
quantum theories. His treatment starts with the statement that, for electromagnetism, the relevant charge is unchanged by motion; prominence is given to the electromagnetic potentials, since they (and not the fields) are the basic objects in the quantum theory. We have already seen (in Section 5.5) how a similar reasoning led Feynman to the conclusion that the relevant potential for gravity had to be a tensor. At variance with electromagnetism, general relativity is typically formulated by starting from the potentials (i.e. the metric), while the analogs of the electric and magnetic fields (the connection coefficients) are considered as derived objects.\footnote{We should mention, however, that, since the 1970s, a few “pure connection” formulations of general relativity, which are the basis of modern approaches to quantum gravity, have been developed (see e.g. \cite{180} and references therein).} Thus, it makes sense to develop an approach to gravity analogous to Feynman’s approach to electromagnetism.

Let us briefly describe Feynman’s approach to electromagnetism, before turning to gravity. As described in [19, 179], Feynman used relativity to argue that the force felt by an electric charge $q$ in the presence of other charges has to be linear in the velocity, that is, of the form:

$$F_i(x, v) = q(E_i + v_j B_{ij}), \quad i, j = 1, 2, 3,$$

(20)

where $E_i$ and $B_{ij}$ are coefficients depending on the other charges. Then, relativistic covariance allowed him to restrict the force (20) to assume the form of the Lorentz force, and to derive the transformation properties of the coefficients, which are identified with the usual electric and magnetic fields (in particular, the magnetic field is obtained as $B_i = \frac{1}{2} \epsilon_{ijk} B_{jk}$). For gravity, Feynman argued that the force acting on a massive particle has to be quadratic in the velocity, rather than linear, due to the higher tensor character of gravitation with respect to electromagnetism. Thus the general expression is ([15], p. 33):

$$F_i = m_0(C_i + v_j \beta_{ij} + v_j v_k \delta_{ijk}), \quad i, j, k = 1, 2, 3,$$

(21)

where $C_i$ is the usual Newtonian field, and $m_0$ is the (gravitational) mass of the given particle. Presumably, this discussion should motivate – in Feynman’s aims – why the geodesic equation is quadratic in the velocity of the moving particle, as he states:

What Einstein did was then to set out to find the laws of motion and the laws determining the coefficients.

For electromagnetism, after deriving the Lorentz force, Feynman switched to Maxwell’s equations. The whole derivation is developed in the 1967-68 Hughes lectures on electromagnetism [16]. In particular, the homogeneous Maxwell equations are obtained from the observation that an invariant action for a relativistic particle in a field can be written by adding to the free action an invariant 4-potential term
\[
S = \int_{\tau_i}^{\tau_f} (-m_0 c^2 \, d\tau - q A_\mu \, dx^\mu),
\]
(22)

where \( d\tau = \sqrt{1 - v^2/c^2} \, dt \) is the proper time element, \([\tau_i, \tau_f]\) is the proper time range and \( A^\mu = (\phi/c, A) \). The corresponding equations of motion are:

\[
\frac{d}{dt} \left[ m_0 \sqrt{1 - \frac{v^2}{c^2}} \right] = q \left[ -\nabla \phi - \frac{\partial}{\partial t} A + v \times (\nabla \times A) \right].
\]
(23)

A comparison of the right-hand side of this equation with the Lorentz force leads to the identifications \( E = -\nabla \phi - \frac{\partial}{\partial t} A \) and \( B = \nabla \times A \), from which the homogeneous Maxwell equations follow immediately, by using standard vector calculus identities.

Remarkably, in \([16]\) (p. 42) there is also a hint to a possible extension of this derivation to gravity, by noticing that a term containing what Feynman calls a “10-potential” (which is a symmetric tensor) could be added as well. In such a case, the action should read:

\[
S = -m_0 c^2 \int_{\tau_i}^{\tau_f} \left( d\tau + \frac{1}{2c^2} h_{\mu\nu} \frac{dx^\mu}{d\tau} \frac{dx^\nu}{d\tau} \right),
\]
(24)

and Feynman commented: “An example of a force derivable from that action is gravity.” Clearly, the 10-potential \( h \) is the gravitational potential, which in general relativity is given by the deviation of the metric from flat space, i.e. \( g_{\mu\nu} = \eta_{\mu\nu} + h_{\mu\nu} \), where \( \eta_{\mu\nu} \) is the Minkowski metric; however, Feynman wrote generically \( g \) in place of \( h \) in the above action. In fact, we found that the correct interaction of the particle with the gravitational field in the weak field approximation \((|h_{\mu\nu}| \ll 1)\) is given by the action in Eq. (24), since it can be easily obtained from that for a particle in a curved spacetime in that approximation:

\[
S_{\text{curved}} = -m_0 c \int_{\tau_i}^{\tau_f} ds = -m_0 c \int \sqrt{g_{\mu\nu} \, dx^\mu \, dx^\nu} = -m_0 c \int_{\tau_i}^{\tau_f} \sqrt{(\eta_{\mu\nu} + h_{\mu\nu}) \, dx^\mu \, dx^\nu}
\]
\[
= -m_0 c \int_{\tau_i}^{\tau_f} \sqrt{(\eta_{\mu\nu} + h_{\mu\nu})} \frac{dx^\mu}{d\tau} \frac{dx^\nu}{d\tau} d\tau = -m_0 c^2 \int_{\tau_i}^{\tau_f} \sqrt{1 + \frac{1}{c^2} h_{\mu\nu} \frac{dx^\mu}{d\tau} \frac{dx^\nu}{d\tau}} d\tau
\]
\[
\approx -m_0 c^2 \int_{\tau_i}^{\tau_f} \left( d\tau + \frac{1}{2c^2} h_{\mu\nu} \frac{dx^\mu}{d\tau} \frac{dx^\nu}{d\tau} \right) = S,
\]
(25)

where in the fifth equality the fact that \( \eta_{\mu\nu} \frac{dx^\mu}{d\tau} \frac{dx^\nu}{d\tau} = c^2 \) has been used. Furthermore the factor \( 1/2 \) has been included in (24) in order to allow the identification of the 10-potential components \( h_{\mu\nu} \) with the usual perturbations of the metric. We also notice that the \( m_0 \) factor appearing in front of the action is the inertial mass, which coincides with the gravitational mass appearing in (21). Feynman, however, did not develop this approach, likely because it would have been very cumbersome.

\textsuperscript{56}He also briefly entertains the possibility of a spacetime scalar potential, which is however immediately discarded in view of the fact that “there are no known laws which are derivable from this action” (cf. the discussion in Section 5.4).
8 Conclusions

In this paper we have reviewed Feynman’s work on gravity by considering all known published sources, as well as a few unpublished ones, including the Hughes lectures [16], which were made available only recently. An emerging feature is his belief in the innermost unity of nature, which, at its deepest level, has to be quantum. This view is reflected in the statement that the fundamental interactions that we experience at the macroscopic level are manifestations of underlying quantum theories, and general relativity can indeed be obtained (as the Maxwell theory) from the fundamental principles of Lorentzian quantum field theory. In this approach, quantum corrections are included as loop diagrams, with several emerging difficulties, which Feynman tackled and partly solved. He usually started from the field theoretical viewpoint to make even classical computations in gravity, such as the orbits of planets in Ref. [12] and the radiation of gravitational waves in Ref. [47], but the Hughes lectures [16] (and also the final chapter of volume II of Ref. [1]) prove that he could also put aside that approach in favor of a more conventional one, if the audience required it. Even then, however, his originality emerges clearly. On the flip side, his strategies were not always the simplest possible, and sometimes tended to be quite cumbersome. An example is his derivation of the equations of general relativity, which was quite long, complicated, and not completely general, and was later surpassed by Deser’s more elegant one. And along with Deser’s and other analogous approaches, Feynman’s one has some shortcomings such as hidden assumptions (as discussed e.g. in [99]). Also his route to the quantization of gravity and Yang-Mills theory was much more difficult than the Faddeev-Popov one, even if some tools he developed, such as the tree theorem, have been recently found to be of wide use. Finally, the approach to the gravitational interaction presented in Section 7.1, while being very original as its electromagnetic counterpart, is not very useful in practice, hence Feynman did not pursue it. Despite these drawbacks, it can be certainly said that Feynman’s work on gravity constitutes a further demonstration of his versatility and ability to contribute substantially to almost every branch of theoretical physics.

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