Underconsideration in Space-time and Particle Physics

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Abstract

Unconceived alternatives could threaten scientific realism. The example of space-time and particle physics indicates a generic heuristic for quantitative sciences for constructing potentially serious cases of underdetermination, involving one-parameter family of rivals T_m (m real and small) that work as a team rather than as a single rival against default theory T_0. In important examples this new parameter has a physical meaning (*e.g.*, particle mass) and makes a crucial *conceptual* difference, shrinking the symmetry group and in some cases putting gauge freedom, formal indeterminism *vs.* determinism, the presence of the hole argument, *etc.*, at risk. It is proposed that the idea of a philosophically "serious rival" involves a theory's plausibility (prior), its fit to data (likelihood), and its making a philosophical (not merely empirical) difference, giving a kind of philosophical expected utility. Methodologies akin to eliminative induction or tempered subjective Bayesianism are more demonstrably reliable than the custom of attending only to "our best theory": they can lead either to a serious rivalry or to improved arguments for the favorite theory. The example of General Relativity (massless spin 2 in particle physics terminology) *vs.* massive spin 2 gravity, a recent topic in the physics literature, is discussed. Arguably the General Relativity and philosophy literatures have ignored the most serious rival to General Relativity.

Contents

1 Introduction

The problem of unconceived alternatives or underconsideration has been proposed as a threat to scientific realism (van Fraassen, 1989, p. 143) (Sklar, 1985; Stanford, 2006; Roush, 2005; Wray, 2008; Khalifa, 2010): scientific theories might be accepted supposedly on the grounds of evidence, but in fact a crucial role was played by a lack of imagination, because there exist other plausible theories that fit that same old evidence at least as well. Arguably this has happened on various occasions in the history of science (Stanford, 2006). The appeal to real science, past or present, is crucial because unconceived alternatives might seem to be a made-up philosophers' worry (Norton, 2008)—especially if one is impressed by how difficult it is to come up with even one good theory. Suffering from unconceived alternatives is an unfortunate consequence of the human condition. But it doesn't enhance the credibility of science. The problem is not merely for the past. Are we merrily detaching consequences from "our best theory" to learn about the world, when the world might well be different and we just haven't tried hard enough to imagine other options? Clearly there is a normative practical point: scientists ought to try to conceive of (some of the better) unconceived alternatives, if possible, and ought to develop them adequately for testing, in order for scientific theories to be as belief-worthy as reasonably possible. Philosophers interested in theory choice should likewise keep tabs on rival theories, as Bayesianism requires.

While such considerations pertain primarily to the philosophy of science, this work is also relevant to the history of science. Historians, wary of whiggishly judging the past by the present, may debate the propriety of rationally reconstructing the past in light of later knowledge. The answer depends on what one is trying to do. If one wishes to learn how events in the past led humans (not Bayesian demigods or Laplacian intelligences) to have solid knowledge (true and justified belief, roughly), then later scientific ideas might be relevant in assessing earlier ones (and *vice versa*) (Chang, 2012). It is difficult to resist the Lakatosian point that the history of science needs normative criteria to ascertain the growth of knowledge, not mere change of opinion (Lakatos, 1971; Lakatos, 1970). Acknowledging this point does not require consigning real history to footnotes; it only requires scouring the actual history—often a *larger* amount of history, including later developments than the usual histories consider, whether of General Relativity (Pitts, 2016b) or perhaps the chemical revolution (Chang's example), collecting those parts that contribute good evidence and argument.

Does one *need* history to make a case for underdetermination? Isn't it enough to produce alternatives and attend to evidence now? I suspect that in principle the history is not needed.

Particle physics generally progresses without attention to history or philosophy, though it could do better in some respects (Pitts, 2016d). Its pragmatic approach to statistics has been discussed in the wake of the Higgs boson discovery (Cousins, 2017). But we moderns are so wedded to the idea of progress (Bury, 1920; Niiniluoto, 1980) that one always faces a strong presumption that knowledge is cumulative, that newer is better (unless somehow the future arrives prophetically, in which case later less revolutionary ideas are worse (Ehlers, 1973)). That is especially true in science, where of course the presumption is often true. But there might be episodes where historical contingency and revolutionary zeal yielded premature dogmatic acceptance of an idea, and later investigation might bring that fact to light. Hence history can be useful to rebut the presumption that the winners won because they were right. Probably they did, but maybe not in every case. Did the Copenhagen interpretation of quantum mechanics achieve hegemony by accident (Cushing, 1994)?

At least three factors have led to the underconsideration of massive spin 2 gravity. Within particle physics, failure to heed a previous exception (Maheshwari, 1972; Pitts, 2016d) to a no-go 'theorem' presented by prestigious physicists played a crucial role. It was thought that every version of massive spin 2 gravity fell prey to one horn or the other of a dilemma, but that claim was (re)discovered in 2010 to be false, leading to a cottage industry. Within General Relativity, ideological factors play a role: general relativists tend to think that background geometry, which (standard) massive spin 2 gravity requires, are bad. A partly resulting longstanding mistake about the cosmological constant Λ, going back to Einstein in 1917 (Schucking, 1991) and long influencing the historiography of General Relativity, has made massive gravity difficult to conceive as the distinct alternative that it is.

Stepping back a bit, one sees that the General Relativity *vs.* particle physics split implies that there have been plenty of experts in geometry, and plenty expert in perturbative expansions, but few adept in both (Brink, 2006). The cosmological constant mistake involves a failure to do a perturbative expansion. The formerly neglected counterexample to the particle physics no-go 'theorem' involves a perturbative expansion that misleads because of non-perturbative effects. Hence each community's strength became a liability when relied upon exclusively.

2 Underdetermination that Makes a Difference

A key feature of some presumably fundamental physical theories is "gauge freedom." The fundamental theories are quantum mechanical (quantum fields), but in line with physicists, let us start with their 'classical' ancestors. Maxwell's electromagnetism can be formulated using the electromagnetic field strength (the "Faraday tensor") $F_{\mu\nu}$. But for many theoretical purposes, including deriving the field equations from the principle of least action and working out the conservation laws for charge, energy, momentum, *etc.*, it is useful to introduce the 4-vector potential A_μ. While A_μ determines $F_{\mu\nu}$ by a 4-dimensional curl (antisymmetric derivative), $F_{\mu\nu}$ doesn't fully determine A_μ. This failure of determination allows spatio-temporal variation: gauge freedom. The principle of least action is best formulated using A_μ, but the resulting Euler-Lagrange equations are too weak to give deterministic time evolution of A_μ. Indeterminism sounds bad, but it is agreed that there is no problem because the real physical content is not A_μ, but the equivalence class of choices of A_μ giving the same electromagnetic field $F_{\mu\nu}$. Analogous but more complicated gauge invariances in the weak and strong nuclear forces use Yang-Mills fields, like several electromagnetisms bound together in a complicated way. Yang-Mills gauge freedom is more interesting because there is no gauge-invariant local field strength; the analog of $F_{\mu\nu}$ is gauge-dependent. These gauge freedoms are both interestingly like and interestingly unlike the gauge (coordinate) freedom of General Relativity, "general covariance."

One thing that the cases of underdetermination discussed below and their analogs show is that we do not know, and perhaps never will know, that gauge freedom is a feature of electromagnetic phenomena. That is because for all we know, Maxwell's theory is wrong and a close cousin developed by de Broglie (de Broglie, 1922; de Broglie, 1923; de Broglie, 1924; de Broglie, 1940; de Broglie, 1942) and Alexandru Proca (Proca, 1936) and later studied by Schrödinger (Schrödinger, 1943b; Schrödinger, 1943a; Bass and Schrödinger, 1955) among others, is correct. In Maxwell's theory, light travels at (naturally) the speed of light, regardless of color (frequency): there is no dispersion. In de Broglie-Proca electromagnetism, light travels at different speeds depending on its color, and the 'speed of light' is the limiting group velocity for high frequencies. The Lagrangian density sprouts a "photon mass" term $-\frac{1}{2}m^2A^{\mu}A_{\mu}$, which *breaks the gauge symmetry* (Jackson, 1975; Sundermeyer, 1982). All of A_{μ} is physical wheat, with no chaff, that is, no gauge freedom. The field equations are deterministic. The true degrees of freedom are available in a local formulation. In some respects de Broglie-Proca electromagnetism is thus *simpler* than Maxwell's theory. The 'photon mass' m distinguishes theories within the de Broglie-Proca family T_m, though 'photon mass' makes sense in terms of classical waves (without light particles) and is a new inverse length scale in the equations. (The speed of light and reduced Planck's constant $\hbar = h/(2\pi)$ are set to 1.) The natural ontologies differ considerably: the massive theory has a deterministic evolution for an observable and physically real potential A_{μ}, whereas Maxwell's theory is deterministic only by rejecting A_{μ} in favor of the electromagnetic field strength $F_{\mu\nu}$ as real. Metaphysics can be radically unstable under small changes in physics, a fact that makes underdetermination and underconsideration serious issues in the philosophy of physics.

In terms of observables, the "massless" limit $m \rightarrow 0$ is smooth, so if m is sufficiently close to 0, then its empirical consequences will likewise be sufficiently negligible (Boulware and Deser, 1972). Empirically, all that we can do is to place ever tighter upper bounds on m, not show that it is 0. Devils in the details can arise in more complicated theories, including grand unified theories that include electromagnetism as a sufficiently integrated part of a larger whole. But there are no devils, classical or even quantum, for electromagnetism by itself. The massless limit of electromagnetism is smooth not only classically (Jackson, 1975), but also in quantum field theory (for references, see (Pitts, 2011b)). Crucially, there is no known difficulty in installing a photon mass term in the electroweak theory, either (Cornwall et al., 1974; Kuzmin and McKeon, 2001); otherwise the appeal to electroweak unification could count strongly against massive photons. For any experimental setup, there are sufficiently small photon masses m that are indistinguishable from 0. It seems that the underdeterminationist wins, because no matter how hard experimentalists try, they cannot refute de Broglie-Proca electromagnetism, but can only squeeze down the upper bound on m. If someone manages to find a plausible objective prior for the photon mass, then it might be possible to show that data make it highly probable that the photon is massless. Thus far the dearth of Bayesian particle physicists and the unfamiliarity of infinitesimal numbers, which might be needed, have been obstacles. Even criteria such as simplicity are deeply ambiguous; depending on one's standards, massive (non-gauge) theories are far simpler than massless (gauge) theories because massive theories allow reduction to the true degrees of freedom while preserving locality, unlike gauge theories (Sundermeyer, 1982; Guay, 2008). There are many theoretical virtues (Keas, 2017), and massive electromagnetism is competitive with Maxwell's theory on most or all of them, and possibly superior in one or two (allowing for differences of judgment). The logical fact $\forall\exists \neq \exists\forall$ is exemplified in that for any photon mass, there is (in principle!) some experiment that would detect it. Thus resolution of the underdetermination problem might not be hopeless if $m \neq 0$, though it is hopeless if $m = 0$ (Maxwell's theory), an amusing asymmetry.

While the continuous massless limit (and assumed same coupling to charge) largely suffice to show that a nonzero photon mass cannot be disproven empirically, one might think that thermodynamic considerations could distinguish between massive and massless electromagnetisms due to the differing number of degrees of freedom (3 polarizations in the massive case, 2 for the massless). However, such considerations pertain to equilibrium. Equilibrium is not reached for the third polarization because the coupling becomes arbitrarily weak in the massless limit (Bass and Schrödinger, 1955; Goldhaber and Nieto, 1971).

3 Godfrey-Smith's Half-Empty/Half-Full Perspectives

This example-driven discussion, long familiar in outline to particle physicists (Goldhaber and Nieto, 1971), resembles some of Peter Godfrey-Smith's discussion of a "Glass Half Full," not just half empty (Godfrey-Smith, 2008), which responded to Stanford's book on unconceived alternatives. Godfrey-Smith, drawing upon Psillos, considers two claims.

> > U: For any particular body of evidence we might have, there will always be more than one scientific theory that can, in principle, accommodate it.
>
> Suppose U is true. How worrying is it? I suggest that its importance is sometimes over-stated because of philosophers' assumption of a particular point of view. The usual situation imagined is one in which we assume we have a body of data and a theory T_1 on the table. Principle U then appears as a kind of epistemic barrier. But so far at least, U is compatible with another principle that might apply to the situation.
>
> > D: For any particular comparison of theories we might want to make, there is some possible body of data that will discriminate the two.
>
> That is, many of the usual underdetermination anxieties are compatible with a kind of symmetry in the situation: for any comparison of theories, we can hope to find discriminating data; for any data, there will be rival theories that are not discriminated. (Godfrey-Smith, 2008)

Thus the glass is half-empty for scientific realists due to U, but also half-full due to D.

For the quantitative cases that I envisage, the fact that the various theories T_m come naturally bundled together as a one-parameter family with philosophical features that they all share, and on some of which they all differ from T_0, gives the family of T_m theories a kind of advantage of unity, strength in numbers. It is interesting whether the photon is massless ($m = 0$) or massive ($m > 0$). It is not so interesting conceptually to know exactly what the value of m is, if it is nonzero. While any specific nonzero value of m is also antecedently implausible—there are too many of them for many to get non-negligible probabilities in isolation, and there is no obvious reason to give a finite probability to any one of them—as a team the massive theories collectively are decently probable antecedently. This judgment reflects practice in particle physics.

4 Particle Physics Terminology and Taxonomy

Despite the quantum mechanical words such as "mass," "particle," and "spin," this paper considers *classical* relativistic field theory, with some quantum words borrowed to apply to the classical antecedents of the presumed quantum results. Particle physics is of interest here due to its *systematic* character: it is the only branch of physics that involves systematic exploration of classical relativistic field theory in the sense of exploring the mass-spin taxonomy. The work has been done primarily by people interested in elementary particles and quantum field theory, often appearing in books with titles such as *Quantum Theory of Fields* or journals such as *Nuclear Physics B*, largely by authors (Wigner, Pauli, Fierz, Dirac, Wentzel, Rosenfeld, Gupta, Feynman, Weinberg, Freund *inter alia*) generally known as "particle physicists."

Often quantum mechanical language is used—"particle," "mass," "spin," *etc.*—reflecting Wigner's and others' work on representations of the group of Lorentz transformations in terms of two parameters, mass and spin (Wigner, 1939; Pauli and Fierz, 1939; Wentzel, 1973). There is no handy alternative language. "Spin" indicates the field's transformation properties under rotations. Spin 0, spin 1 and spin 2 correspond to scalar, vector and (symmetric matrix) tensor transformation properties, respectively, and have consequences for angular momentum. If the quantization of a classical vector field, such as Maxwell's electromagnetism, yields "particles" (photons) with zero rest mass and angular momentum equal to 1 times $\hbar$, a particle physicist will call Maxwell's theory the theory of a massless spin 1 particle or a massless photon; likewise with gravity and "graviton" as spin 2. Massive particles were called "mesons." A field is "massive" if its equation of motion contains an algebraic linear term in the field potential.

Massive spin 2 gravitational theories might or might not include a spin 0 component: they can be "spin 2-spin 0" or "pure spin 2." By contrast to systematic contemplation of options, even the name "general relativist" privileges one specific theory of one force, gravity. One can tell which theory is the default by looking at the sign on the door or the letterhead. Does institutionally privileging one theory enhance objectivity? If Earman's "Plea for Eliminative Induction" (Earman, 1992, p. 163) is to be heard in application to space-time theory in a way that considers serious alternatives (in a sense to be explained below), particle physics will be required (*c.f.* (Thorne et al., 1973; Lee et al., 1974; Thorne et al., 1971), where massive theories are missing).

5 Serious Quantitative Underdetermination Rivalries

There are gravitational analogs to the electromagnetic rivalry. Their form suggests that *within physics*, prima facie *underdetermination is ubiquitous*. However, sufficiently complicated examples in physics can have devils in the details, as was first noticed around 1970 with quantum spin 1 Yang-Mills and classical spin 2 gravity (Boulware and Deser, 1972) (and references therein). Thus underdetermination is defeasible: adding a mass term and getting underdetermination between massless and low-mass particles/fields is only a heuristic, not an algorithm. One can attempt to construct a rival to any given theory by introducing a new parameter (ideally, with physical meaning, such as particle mass) and making it small; both skill in theory construction and luck are needed for the effort to succeed. The specialness of mechanics (broadly construed), previously noted in discussions of realism and underdetermination (Duhem, 1954; McMullin, 1984; McMullin, 1991; Kitcher, 2001; Stanford, 2006), plays a role: only mathematized theories are so strictly individuated that one can easily write down many candidate theories for a given domain.

Compared to a common strong underdetermination thesis (Kukla, 1998), my thesis of widespread *prima facie* underdetermination in quantitative sciences is weaker in four ways.

- it is restricted to mathematized sciences,
- it is defeasible rather than algorithmic,
- it involves a one-parameter family of rivals that work as a team rather than a single rival theory, and
- it is asymmetric: the family remains viable as long as the 0-parameter theory is, but not *vice versa*.

For example, detecting a non-zero photon mass would falsify Maxwell's theory, but no experimental result would falsify the de Broglie-Proca family unless it also falsified Maxwell's theory. A massive theory (family) can be tuned, by choice of a sufficiently small particle mass, to approximate the massless relative as closely as one wishes, unless some vice appears to spoil the massless limit (but none does for electromagnetism, even when quantized). Thus massive theories with small mass have similar likelihoods $P(E|T)$ to the massless theory's (assuming a smooth massless limit). Being well motivated relativistic field theories, they should have a collective prior probability $P(T)$, obtained by integrating the probability over particle mass, that is also competitive. Just what that prior is, seems to be an unsolved problem, though Harold Jeffreys had a relevant suggestion for the case of a new parameter (Jeffreys, 1961, pp. 245-249). I gesture toward a future Bayesian particle physics. Thus massive theories threaten to cut the posterior probability of their massless relatives substantively, jeopardizing the rational acceptance of their massless relatives, when the massive ones have no vices. Given the similar (but unequal) likelihoods, it is difficult (though not impossible) for evidence to alter much the relative probabilities of the specific theories in question. According to Kitcher, "[t]he underdetermination thesis obtains its bite when permanent underdetermination is taken to be rampant" (Kitcher, 2001, p. 31)—though he thinks that such is not the case. But neglecting particle physics leaves a high-probability, high-likelihood sector in the catch-all hypothesis $\neg T$, an infamous danger (Shimony, 1970, p. 132) (Salmon, 1990). While the feature of being a difference that *makes a philosophical difference* is less generic than the four features above, it is an important feature of many examples of fundamental physics because generally the symmetry group is shrunk by the mass term and often some gauge freedom disappears.

Massive gravities, both spin 0 and (if stable and possessed of a smooth massless limit) spin 2, being alternatives unconceived in the General Relativity tradition, give one reason to wonder whether that tradition's justifications lead toward truth. Einstein's arguments are often not compelling (Norton, 1995). Such theories, once conceived, pose a problem of underdetermination. The table below illustrates the relevant piece of the mass-spin taxonomy, a catalog of the possible relativistic wave equations. Question marks are left for massive scalars and massive spin 2 because they have received so little attention until recently, especially in HPS. While a massive scalar is a common toy theory (the Klein-Gordon equation) (Kragh, 1984), application to gravity has generated less than a dozen papers ever, many of them recent (Pitts, 2011a; Pitts, 2011b; Pitts, 2016c; Pitts, 2017c).

Table 1: Blanks in the Taxonomy of Spin and Mass

	Spin 0	Spin 1	Spin 2
$m = 0$	Nordström	Maxwell	Einstein
$m \neq 0$	**?**	de Broglie-Proca	**?**

6 Unconceived Alternative: Massive Scalar Gravity

Nordström's scalar gravity, a serious competitor to Einstein's program during the middle 1910s, is said to have shown that even the simplest and most conservative relativistic field theory of gravitation burst the bounds of Special Relativity (SR) (Misner et al., 1973, p. 179) (Norton, 2007, p. 414). Nordström's theory has a conformally flat space-time geometry (Einstein and Fokker, 1914). Using the geometry of the 1920s (Thomas, 1925; Pitts, 2016c) (largely forgotten by mathematicians until the 1990s (Branson, 2005, p. 180)), one can factor the metric into separately meaningful conformal and volume pieces: $g_{\mu\nu} = \hat{\eta}_{\mu\nu}\sqrt[4]{-g}$, where $\hat{\eta}_{\mu\nu}$ (with determinant -1), being Weyl-flat, determines the light cones just as in SR. Nordström's theory is invariant under the 15-parameter conformal group, not just the 10-parameter Poincaré group of SR.

Massive scalar gravity has a curious history of early independent partial inventions and retarded completion. In the 1890s Hugo von Seeliger on physical grounds and Carl Neumann on mathematical grounds envisaged long-range modifications of $\frac{1}{r}$ potentials (Neumann, 1896; von Seeliger, 1896; Pauli, 1921; Norton, 1999). While no physical meaning was assignable at the time to Neumann's exponential decay constant in the potential $\frac{1}{r}e^{-mr}$, in retrospect (Yukawa, 1935; Freund et al., 1969) one can see Neumann as having in effect a "massive" variant of Newtonian gravity in the 1890s. When one considers a massless scalar theory such as Nordström's, it is natural to consider a massive variant and to ascertain whether the massless limit is smooth. Scalar fields became standard tools in particle physics in the mid-1930s (Pauli and Weisskopf, 1934) and a systematic exploration of the options of relativistic field theory became important in 1938 (Wentzel, 1973). Despite the prominence of the Klein-Gordon equation, massive variants of Nordström's theory were never entertained prior to the late 1960s or later (Freund and Nambu, 1968; Deser and Halpern, 1970; Dehnen and Frommert, 1990). Massive scalar gravities, if the mass is sufficiently small, fit the data as well as does Nordström's theory, a consequence of the smooth massless limit (Boulware and Deser, 1972) (Weinberg, 1995, p. 246). There is a one-sided permanent underdetermination between Nordström's theory and it massive variants: as long as the former is viable, so are the latter (Pitts, 2011b). Massive Newtonian gravity has a smaller symmetry group (just Galilean transformations) than does Newtonian gravity, so an arbitrarily small empirical difference makes a large conceptual difference. The philosophical significance of massive scalar gravity has finally been explored recently; without being empirically viable, it sheds a striking revisionist light on many standard issues in the philosophy of space-time (Pitts, 2016c; Pitts, 2017c), both old (such as conventionality) and new (such as the logical priority of dynamics or geometry). Massive scalar gravities are not geometrizable: they contain both the conformally flat metric $g_{\mu\nu} = \hat{\eta}_{\mu\nu}\sqrt[4]{-g}$ of Nordström's theory (as geometrized by Einstein and Fokker (Einstein and Fokker, 1914; Beck and Howard, 1996)) and the remainder of the flat metric $\eta_{\mu\nu} = \hat{\eta}_{\mu\nu}\sqrt[4]{-\eta}$ of SR because the graviton mass term contains the volume element $\sqrt{-\eta}$ unaffected by gravity. $\sqrt{-\eta}$ shows itself on cosmic distance scales and only there (Freund et al., 1969). Similar things could be said, at least defeasibly, for the massive spin 2 case, but complications arise.

Boulware and Deser wrote a very influential work (among the particle physics-aware) on the possibility of massive variants of Einstein's theory (Boulware and Deser, 1972). While they had fresh bad news for massive spin 2 gravity, they summarized the happy situation for simpler theories.

> There exists a continuum of consistent finite-mass photon theories whose consequences (both classical and quantum) smoothly approach those of conventional

> electrodynamics in the limit. Specifically, the longitudinal photons decouple from the current, becoming free fields (apart from gravitational interaction) while the helicity 1 quanta become the transverse Maxwell photons in a gauge-invariant way. The same continuity holds for nonrelativistic Newtonian gravitation, the geometric (but scalar) Nordström theory, and generally for exchange of spin-0 or -$\frac{1}{2}$ particles in special relativity. The only effective difference is in the exchange denominators (producing e^{-mr}/r rather than $1/r$ as the long-range potential); this difference disappears in the limit. (Boulware and Deser, 1972)

From the perspective of particle physicists in the 1970s, underdetermination was the *default expectation*. The news was that underdetermination did not *always* arise. The expectation that massive theories are worth exploring, have smaller symmetry groups, lack gauge freedom and can approximate massless theories arbitrarily well was already understood.

7 Massive GR: Unconceived or Underconsidered

Given the analogy between electromagnetism (massless spin 1 gauge theory) and GR (massless spin 2 gauge theory), the precedent of massive electromagnetism motivated some to look for massive variant(s) of GR. (Massive scalar (spin 0) gravity could have had that effect if it had been invented in a timely way.) For Pauli, 1939 was full of mesons, including spin 2 (Pauli and Fierz, 1939; Fierz and Pauli, 1939; von Meyenn et al., 1985; von Meyenn, 1993). Pauli and Markus Fierz found that only a special choice of mass term gave a (positive-energy) pure spin 2 theory, as opposed to a spin 2 with a negative energy spin 0, which looked unstable. Some authors soon considered a graviton mass, especially Marie-Antoinette Tonnelat and Gérard Petiau starting in the 1940s (inspired by de Broglie) (Tonnelat, 1941; Petiau, 1941b; Petiau, 1941a; Tonnelat, 1942a; Tonnelat, 1942b; de Broglie, 1943; Tonnelat, 1943; Petiau, 1943; Tonnelat, 1944a; Tonnelat, 1944b; Tonnelat, 1944c; Petiau, 1944; Petiau, 1945; de Broglie, 1954). In the 1960s some particle physicists developed nonlinear interacting theories which presumably were empirically equivalent to GR in the limit as the graviton mass(es) goes to 0 (Ogievetsky and Polubarinov, 1965; Ogievetskii and Polubarinov, 1966; Freund et al., 1969). These and other massive gravities can be derived using universal coupling (Freund et al., 1969; Pitts and Schieve, 2007; Pitts, 2011c; Pitts, 2016d), much as General Relativity can be derived starting with a free massless spin 2 field in flat space-time (Kraichnan, 1955; Feynman et al., 1995; Deser, 1970; Pitts and Schieve, 2001). The flat metric $\eta_{\mu\nu}$ appears ineliminably and (barely and only indirectly) observably in the graviton mass term only, not in the matter field equations (Freund et al., 1969). The matter action is most of the answer to the question (Brown, 2005) why rods and clocks register the curved geometry. The effective curved metric seen by matter can be viewed (and derived!) as the sum (literally, not just epistemologically or metaphorically) of the flat background metric and the gravitational potential (Kraichnan, 1955; Freund et al., 1969; Deser, 1970; Pitts and Schieve, 2001; Pitts and Schieve, 2007; Pitts, 2016a). Adding a mass term for the graviton suggests theories that approximate Einstein's arbitrarily closely, but completely differing from Einstein's theory in terms of the conceptual novelties (Freund et al., 1969; Pitts, 2017b)—just like de Broglie-Proca massive electromagnetism. Such massive graviton theories, one would expect, are generally straightforwardly special-relativistic, with the Poincaré symmetry group.

Devils in the details can arise for gravity and other fields with spin higher than 1, however (Boulware and Deser, 1972; Berends et al., 1979; Berends and van Reisen, 1980). For spin 1 and higher, one can construct lower-spin part by taking the divergence; then one needs to eliminate that degree of freedom to avoid the expectation of instability. For spin 2 and higher, one can also take a trace and/or a double divergence, giving more opportunities for negative-energy degrees of freedom. That trace is one of the main roots of difficulty in massive spin 2 gravity (Boulware and Deser, 1972). A plausible argument against all massive gravities did not appear before 1970, when the van Dam-Veltman-Zakharov discontinuity for pure spin 2 was discovered (van Dam and Veltman, 1970; van Dam and Veltman, 1972; Zakharov, 1970; Boulware and Deser, 1972; van Nieuwenhuizen, 1973). Whereas the technical novelties of the kinetic term for General Relativity were inevitable because shared between it and its massive relatives, by contrast the *conceptual* novelties of General Relativity involving point individuation and the hole argument, alleged lack of time evolution (but see (Pitts, 2014b; Pitts, 2014a)), difficulty in finding gauge-invariant observables (*c.f.* (Pitts, 2017a)), difficulty in localizing gravitational energy (*c.f.* (Pitts, 2010)) and the like, which are tied to the gauge group (and hence presumably disappear when the gauge freedom is broken by a mass term (Freund et al., 1969), but see (Pitts and Schieve, 2007)), were another matter. Prior to 1970, the conceptual novelty of GR was *optional*, though fascinating and plausible, because one (if informed about particle physics) would expect most aspects of that conceptual novelty to be removed by a graviton mass term and there was no good reason to reject a graviton mass term. Simply failing to entertain such theories was to suffer from unconceived alternatives. One shouldn't bet on it, but *sometimes* what you don't know, won't hurt you; lack of imagination was compensated by good luck in this case. It now appeared that there were objections to *both* types of massive variants of General Relativity in the early 1970s: spin 2-spin 0 was unstable due to negative energies, and pure spin 2 was empirically falsified due to a bad massless limit and didn't exist nonlinearly anyway (Boulware and Deser, 1972). Thus the acceptance of General Relativity itself, and not suspension of judgment between it and a massive variant(s), rightly[1] seemed compelling to most particle physicists who worked on gravity, the community that did not suffer as seriously from unconceived alternatives, but realized that it had such a problem in the 2010s.

If one knows what to look for, one can see from Klaus Hentschel's exhaustive work that neither the historical actors nor (in this case) their more recent historians have contemplated massive (spin 0 or spin 2) gravity as an alternative theory in the context of interpreting General Relativity (or addressing it more generally) (Hentschel, 1990). Another work that aimed for comprehensiveness and that usefully illustrates the neglect of the possibility of massive gravity during a formative era (1921-1937) is Combridge's bibliography (Combridge, 1965). Hentschel helpfully gives a list of alternative theories considered (section 1.5, pp. 46-54), in which nothing like massive spin 0 or massive spin 2 gravity appears. Neither does the book refer to the relevant 1890s papers by Seeliger or Neumann, or to the key Pauli-Fierz or Fierz-Pauli papers (Pauli and Fierz, 1939; Fierz and Pauli, 1939), or mention Fierz at all; presumably the historical actors didn't do so either. Thus a treatment that stops in the 1910s or 1920s or even 1938 is seriously limited for assessing why *we today* should accept GR. The problem is not necessarily with histories of General Relativity; much depends on why one writes such

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[1] Apart from a completely ignored work that foreshadowed a 2010 breakthrough proving nonlinear existence of pure spin 2, that is (Maheshwari, 1972; de Rham et al., 2011; Pitts, 2016d), as well as a small hint for avoiding the discontinuity (Vainshtein, 1972) that would be developed in the 2000s. Particle physicists' lack of zeal for dotting i's and crossing t's and doing exhaustive literature searches contributed to a mistaken belief that all types of massive gravity were excluded by the van Dam-Veltman-Zakharov discontinuity and/or the Boulware-Deser ghost for 30-40 years.

histories. But if one makes the common tacit assumption that such histories have some direct applicability to us today, showing how we came to know what we take ourselves to know, yet without addressing the massive amount of relevant work from particle physics from the 1920s onward, then there is a problem. Thanks to the particle physics tradition, we have both a larger collection of potentially interesting competitor theories and remarkably strict criteria for eliminating many naive possibilities, namely, excluding negative-energy degrees of freedom. This criterion would exclude, for example, Einstein's *Entwurf* theory (Pitts, 2016b). Thus the problem of unconceived alternatives also affects the historiography of GR.

7.1 Einstein's Faulty Analogy

One reason that massive gravity has been underconsidered is a long-lasting faulty analogy made by Einstein in 1917 (Pitts, 2018). In 1917, not yet aware of Seeliger's work, Einstein reinvented Neumann's mathematics on Seeliger-like grounds, giving what one might now call massive Newtonian gravity (Einstein, 1923). But he promptly drew an analogy between this massive Newtonian gravity and his new cosmological constant Λ term. Unfortunately this analogy is false (Heckmann, 1942; Treder, 1963; Trautman, 1965; DeWitt, 1965; Freund et al., 1969; Treder, 1968; Schucking, 1991; Faraoni and Cooperstock, 1998; Norton, 1999; Harvey and Schucking, 2000; Earman, 2001)—an error that would resurface often and still does occasionally. The cosmological constant introduces a zeroth order term, not just a first order term, into the field equations, and does not give exponential decay. This mistake evidently was diagnosed first by Otto Heckmann in 1942 (Heckmann, 1942; Harvey and Schucking, 2000), but with little effect. This false analogy—Einstein's 'other' blunder with the cosmological constant—tends to obscure the possibility of a genuine spin 2 analog to massive scalar gravity. Unfortunately Einstein's false analogy has continued to bewitch serious historians (North, 1965, p. 179) (Jammer, 1993, pp. 194, 195) (Pais, 1982, p. 286) (Kragh, 2004, p. 28). Engelbert Schücking, who was influenced by Heckmann, mounted a vigorous assault on that error (Schucking, 1991; Harvey and Schucking, 2000).

7.2 Relevance to Kant and Schlick; Mittelstaedt

The existence of unconceived alternatives in the form of the rival 'massive' theories of gravity to be considered here has a dramatic effect on the robustness of the lessons typically inferred from General Relativity, whether about space-time theory or even, more broadly, about Kant's constitutive *a priori* (Pitts, 2017b). In the actual contingent history (as opposed to what should have happened), the years around 1920 were crucial for a rejection of even a broadly Kantian *a priori* philosophy of geometry, especially due to Moritz Schlick's influence (Schlick, 1920; Schlick, 1921; Coffa, 1991; Reichenbach, 1965; Ryckman, 2005; Friedman, 1999; Bitbol et al., 2009; Domski et al., 2010). That is because physics, that is, General Relativity, had shown that the most plausible example of synthetic *a priori* knowledge, geometry, was no example after all, and there apparently being none, one should drop the idea, Schlick argued. If anyone had considered this issue in light of particle physics, then massive gravity would have shown that Schlick's argument didn't work (Pitts, 2017b). Then finally the early 1970s, with the (apparent) failure of massive gravity, would have been the right time to say what Schlick said prematurely in the late 1910s.[2]

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[2] Given the neglected counterexample (Maheshwari, 1972), even the 1970s were too early. Schlick's claims might finally be justified in the 2010s.

The nearest miss is the work of Mittelstaedt, who discussed Kant (Mittelstaedt, 1976) and massive gravitons (Mittelstaedt, 1970, p. 15) in related contexts at similar times. Unfortunately he did not recognize the *conceptual* significance of massive gravity due to its removing the gauge freedom (general covariance), from which most of the conceptual novelty of GR arises. Mittelstaedt's eliminative induction approach to space-time theory was inspired by Gupta (Gupta, 1952; Gupta, 1954; Gupta, 1957). Mittelstaedt's discussion, brief as it is, is worth noting, being perhaps unique within the philosophical literature prior to this millennium in its perspective. (It will also be useful as a foil for Jürgen Ehlers' approach below.) Mittelstaedt writes:

> Requiring that the gravitational field be describable in the framework of a Lorentz-invariant field theory, one can immediately draw some important conclusions from these four empirical findings. The fact that the gravitational force is attractive means that it is a scalar field ϕ or a tensor field $\psi_{\mu\nu}$. Vector fields lead to repulsive forces, as electrodynamics shows. Furthermore, since the range of the field is very large, the rest mass of a corresponding elementary particle, the graviton, is very small. We will assume that the rest mass of the graviton $m_0 = 0$ vanishes, so that the remaining choice is a scalar or tensor field of rest mass $m_0 = 0$. The deflection of light, however, can be described only by tensor. As already mentioned in the discussion of Nordström's theory, a scalar gravitational field gives no bending of light and must therefore be excluded. One will therefore attempt to describe gravity by a massless tensor field. Since the source of this field is the (symmetric) energy-momentum tensor of matter, it further follows from the field equations that also the tensor itself is symmetrical. Finally, requiring that the gravitons as described by this symmetric tensor field have a well-defined spin, namely $s = 2$, the Lagrangian of the gravitational field is uniquely determined if we restrict ourselves for the time being to Lagrangians that lead to linear field equations. (Mittelstaedt, 1970, p. 15, translated with help from ...)[3]

Unfortunately Mittelstaedt apparently did not relate Kant and massive gravitons, treated the graviton mass as a merely empirical parameter with no conceptual significance, and wrote a bit too early to encounter the discontinuous limit problem (van Dam and Veltman, 1970; Zakharov, 1970). The 1976 English translation was based on the 1972 German fourth edition (Mittelstaedt, 1976), leaving little time to absorb the new results on massive gravity (van Dam and Veltman, 1972; Boulware and Deser, 1972). But the seventh German edition (1989) does

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[3] "Verlangt man, daß das Gravitationsfeld sich im Rahmen einer lorentzinvarianten Feldtheorie beschreiben läßt, so kann man aus diesen vier empirischen Feststellungen sofort einige wichtige Konsequenzen ziehen. Die Tatsache, daß die Gravitationskraft anziehend ist, bedeutet, daß es sich um ein skalares Feld ϕ oder um ein Tensorfeld $\psi_{\mu\nu}$ handelt. Vektorfelder führen zu abstoßenden Kräften, wie die Elektrodynamik lehrt. Da weiter die Reichweite des Feldes sehr groß ist, ist die Ruhemasse eines entsprechenden Elementarteilchens, des Gravitons, sehr klein. Wir wollen annehmen, daß die Ruhemasse des Gravitons $m_0 = 0$ verschwindet, so daß noch ein skalares oder ein tensorielles Feld der Ruhemasse $m_0 = 0$ zur Wahl stehen. Die Lichtablenkung kann aber nur durch Tensorfeld beschreiben werden. Ein skalares Gravitationsfeld liefert, wie schon bei der Diskussion der Nordströmschen Theorie erwähnt wurde, keine Lichtablenkung und muß daher ausgeschlossen werden. Man wird daher versuchen, die Gravitation durch ein masseloses Tensorfeld zu beschreiben. Da die Quelle dieses Feldes der (symmetrische) Energie-Impuls-Tensor der Materie ist, ergibt sich weiter aus den Feldgleichungen, daß auch das Tensorfeld selbst symmetrisch ist. Verlangt man schließlich noch, daß die durch dieses symmetrische Tensorfeld beschriebenen Gravitationen einen eindeutig definierten Spin, nämlich $s = 2$, haben, so ist durch die Gesamheit dieser Forderungen die Lagrangefunktion des Gravitationsfeldes eindeutig bestimmt, wenn wir uns zunächst auf Lagrangefunktion beschränken, die auf lineare Feldgleichungen führen." [footnote to (Thirring, 1959; Thirring, 1961)]

not differ.

8 Recent Breakthroughs in Massive Gravity?

With few exceptions, matters stood still from the early 1970s until the late 1990s, when the question started slowly to reopen due to the "dark energy" phenomenon indicating that the cosmic expansion is accelerating (Riess et al., 1998), casting doubt on the long-distance behavior of General Relativity—just the regime where a graviton mass term should be most evident. Note that the *kind* of difference that one might expect from a mass term is not obviously what dark energy showed. Many other long-distance modifications of General Relativity were also attempted, some far more speculative than a graviton mass. A viable massive gravity theory must, somehow, achieve a smooth massless limit in order to approximate General Relativity, and be stable (or at least not catastrophically unstable). That such an outcome is possible has been widely and seriously discussed in recent years. In the 2000s a flood of work on massive gravities (*e.g.*, (Hamamoto, 1996; Visser, 1998; Dvali et al., 2000; Scharf, 2001; Damour and Kogan, 2002; Damour et al., 2003; Babak and Grishchuk, 2003; Petrov, 2004; Gabadadze and Shifman, 2004; Creminelli et al., 2005; Dvali et al., 2008; Rubakov and Tinyakov, 2008; Grigore and Scharf, 2008; de Rham et al., 2008; Izumi and Tanaka, 2009; de Rham, 2014; Scargill et al., 2014) made massive gravity now a "small industry" (Hinterbichler, 2012, p. 673), one in which physicists also get jobs.

In particular, recently a number of physicists have doubted the finality of the van Dam-Veltman-Zakharov discontinuity argument against the pure spin 2 theory (Vainshtein, 1972; Gruzinov, 2001; Porrati, 2002; Deffayet et al., 2002; Arkani-Hamed et al., 2003; Deffayet and Rombouts, 2005; Vainshtein, 2006; Babichev et al., 2009; Babichev et al., 2010; de Rham et al., 2011; Hinterbichler, 2012). Perhaps the linear approximation used by van Dam *et al.* breaks down and something better happens non-perturbatively? A key recent question was whether one could secure a smooth massless limit without converting a pure spin 2 theory into a spin 2-spin 0 theory and so acquiring its presumed stability problem (Deffayet and Rombouts, 2005; Creminelli et al., 2005; Babichev et al., 2010; de Rham et al., 2011; Hinterbichler, 2012). It had long and reasonably been believed (Boulware and Deser, 1972) that the negative energy spin 0 could not be avoided past linear order, on account of special relativistic constraints on whether a certain quantity (the "lapse" in the ADM split of space-time into space and time) could appear linearly in a graviton mass term. The obvious answer is "no," because the metric is quadratic in the lapse, so functions of the metric, even infinite series expansions, will be even in the lapse. But a breakthrough in 2010 and following shows that, by redefining some other fields (the ADM shift vector) in a highly nonlinear way, one can make the lapse appear linearly and thus avoid the negative energy spin 0 after all (de Rham et al., 2011; Hassan and Rosen, 2012)! The result involves a square root of the metric tensor in the sense of a binomial series expansion; adding sufficiently many even powers of the lapse N with the right coefficients can give a term $|N|$ that is reflection-symmetric (even in one sense) but *odd* in the crucial sense that its derivative with respect to N is constant for $N > 0$. Thus some theories that are pure spin 2 to linear order can remain that way exactly to all orders, securing positive energy and hence stability (avoiding the curse of spin 2-spin 0 theories). As noted above, this result was in a sense anticipated (Maheshwari, 1972) prior to the discovery that there is (usually) a Boulware-Deser ghost, but no one noticed (a counterexample before the no-go 'theorem').

The other issue, the discontinuity of the massless limit for pure spin 2 theories, arguably could be adequately handled by the "Vainshtein mechanism" (Vainshtein, 1972; Deffayet et al., 2002; Vainshtein, 2006; Babichev et al., 2010). Up to some large distance scale determined by the graviton mass and the Schwarzschild radius of a heavy source, the linear approximation is invalid and a more accurate nonlinear treatment (involving exact solutions or numerical simulation by computer) removes the discontinuity. Thus recent careful work and technical-conceptual breakthroughs show that a massive variant of General Relativity can have both a smooth massless limit and positive energy. This dramatic vindication of massive gravity that made it a serious contender in space-time and gravitation theory since 2010. While underdetermination by approximate but arbitrarily close empirical equivalence has long been clear in electromagnetism (Pitts, 2011b), it is now (back) in business for gravitation as well. At least it was briefly, before new problems arose (Deser and Waldron, 2013; Deser et al., 2015); these problems are disputed, however (de Rham, 2014). A review of the bounds on the graviton mass has appeared recently (de Rham et al., 2017a). While some authors criticize massive gravity, authors reject these criticisms (de Rham et al., 2017b). The situation is highly dynamic.

9 Ignoring the Most Serious Rival(s) to General Relativity?

In order for the conceptual lessons of General Relativity to be taken with appropriate seriousness, rather than too little or too much, one needs to know whether there are or were serious rivals considered along the way. I take a rival theory to be a *serious rival* when all three of the following criteria are substantially satisfied:

1. **P**lausible (fairly high prior probability?) in terms of local relativistic field theory.
2. **D**ifferent from GR in terms of its philosophical lessons.
3. **C**onfirmed by the data, which in effect mean. not easily distinguished from GR *empirically* (similar likelihoods $P(E|T)$) where GR does well).

Hence a serious rival will be **P**lausible, **D**ifferent, and **C**onfirmed. These criteria are patterned after expected utilities, with utility construed in terms of making a philosophical difference compared to the default theory. The considerations that motivate standard rational decision theory, rendered specific to philosophers' interest in space-time theory choice, thus serve to motivate these criteria. Without these three conditions, either a rival is most likely false because initially implausible ($\neg P$) or disconfirmed ($\neg C$), or its being true instead of General Relativity would make little difference philosophically ($\neg D$). The category of making little philosophical difference would include, for example, higher-derivative theories of gravity with curvature-squared terms. Such theories might in principle be plausible and confirmed, but entertaining them does not put to the test the conceptual lessons of General Relativity, because such theories' conceptual lessons would be largely the same as General Relativity's. It is helpful to categorize many of the rival theories entertained in the General Relativity tradition and/or noticed by philosophers in the last 90-odd years (Whitrow and Morduch, 1965; Whitrow and Morduch, 1960; Thorne and Will, 1971; Will, 1993; Earman, 1992). None satisfies all three conditions in my judgment, so none is a serious rival in the specified sense. (It is of course possible that I have missed something, and judgments are required.) Theories only proposed in the last decade are also less relevant to assessing whether serious rivals have been entertained, because space-time philosophy hasn't taken alternatives to General Relativity with real serious in many decades (though (Brown, 2005; Lehmkuhl, 2008) are exceptional). Whitrow and Morduch produced a conceptually insightful and thorough evaluation of various special relativistic gravitation theories for the older phase through the early 1960s (Whitrow and Morduch, 1965; Whitrow and Morduch, 1960). Some were not local field theories, while others had dangerous negative-energy degrees of freedom. All were more or less *ad hoc* or motivated by naive mathematical simplicity as opposed to serious first-principles motivation in special relativistic field theory. Thus these rivals had either a low prior probability in comparison to General Relativity or a low likelihood given the evidence. Despite paying attention to "Lorentz invariant" theories, unfortunately Whitrow and Morduch omitted massive variants of General Relativity. (They could have cited Tonnelat, Petiau and more contemporaneously, Droz-Vincent (Droz-Vincent, 1959).) One could easily enough draw an inductive lesson that "Lorentz invariant" gravitation theories fall short on plausibility, and in some cases also on confirmation, without noticing that the most serious candidate was omitted. North's survey of cosmology from the early 1960s attends significantly to Milne, Birkhoff, and Whitehead (North, 1965), reflecting cosmology as it then was but implicitly showing how out of touch cosmology was with theoretical physics.

Work associated with or based upon the early 1970s project of a "theory of gravity theories" by Thorne, Lee, Lightman, Ni, Will, *etc.* (*e.g.*, (Thorne and Will, 1971; Will, 1993)), noted by Earman as akin to eliminative induction (Earman, 1992), is more sophisticated. It is, however, ultimately not relevantly different, in that it still fails to introduce a serious rival for General Relativity. The newer rivals at least are local field theories, but are not necessarily compatible even with special relativity or otherwise at all plausible (*e.g.*, Ni's theory (Ni, 1972)). Though a serious rival(s) to General Relativity existed in the particle physics literature in the 1960s (Ogievetsky and Polubarinov, 1965; Freund et al., 1969; Maheshwari, 1972)—including pure spin 2 massive gravity reinvented in 2010!—the literature on experimental testing of General Relativity apparently did not consider massive gravity until almost 2000 (Will, 1998; Finn and Sutton, 2002). A few relevant references that did not fit in the table are worth mentioning (Belinfante and Swihart, 1957; Lightman and Lee, 1973; Whitehead, 1922).

Rivals in GR Literature: Plausible? Different? Confirmed?

Theory	Bugs/Features	Scorecard
Newton	Not local field theory, violates SR	$\neg P$, D, $\neg C$
Poincaré 1906	Not local field theory	$\neg P$, D, $\neg C$
Nordström’s second (1914)	Mercury wrong, doesn’t bend light	P, D, $\neg C$
Whitehead (Synge, 1952; Schild, 1956)	Not local field theory	$\neg P$, D, $\neg C$
Birkhoff (Weyl, 1944; Birkhoff, 1943)	Speed of sound = c	$\neg P$, D, $\neg C$
Belinfante-Swihart (Lee and Lightman, 1973)	Simple math, negative energy	$\neg P$,D, $\neg C$?
Rosen’s bimetric (Will, 1993)	Simple math, negative energy	$\neg P$, D, $\neg C$?
Ni (Ni, 1972)	Absolute time, conform. flat space	$\neg P$, D, $\neg C$
Brans-Dicke-Jordan scalar-tensor	Funny kinetic term	P?, $\neg D$,C
Will-Nordtvedt (Will and Nordtvedt, Jr., 1972)	Odd kinetic term, preferred frame	$\neg P, D/P, \neg D, C$?
Einstein-Aether (Jacobson and Mattingly, 2001)	Violates Special Relativity?	$\neg P, D/P, \neg D, C$
TeVeS (Bekenstein, 2005)	MOND, contrived, violates SR?	$\neg P, D/P, \neg D, \neg C$?

Whether the last three theories in the table (Will-Nordtvedt, Einstein-Aether, and TeVeS) are generally covariant or violate *special* relativity depends on whether one thinks that having every field varied in the principle of least action suffices for substantive general covariance, or if having a field that is the same (neighborhood-by-neighborhood (Hiskes, 1984)) in all models (an “absolute object”) implies a violation (Anderson, 1967). Awkwardly, these two notions of substantive general covariance turn out not to be coextensive (Pitts, 2006; Giulini, 2007; Zajtz, 1988; Brading and Castellani, 2007; Pooley, 2010), *pace* traditional belief ((Anderson, 1967)). One can adapt the coordinates so that the non-vanishing (tangent) vector has components $(1,0,0,0)$ in a neighborhood, defining absolute rest. Einstein-Aether and TeVeS, being more serious rivals than most of those previously entertained, also date from the 2000s, after dark energy broke the near-consensus that only General Relativity was worth exploring. Further possibilities in this vicinity are being explored (Jacobson and Speranza, 2015). TeVeS evidently is encountering new empirical problems (Boran et al., 2017), so one might regard it as disconfirmed.

By contrast the massive gravities entertained here all have at least the special relativistic (Poincaré) symmetry group, which makes them a more conservative option. Massive gravity, missing from the table because it has generally been ignored in the General Relativity literature despite being available in a mature form since 1965 (Ogievetsky and Polubarinov, 1965; Freund et al., 1969), would pass on all three criteria, a score of P, D, C, at least if no devils in the details are envisaged. Thus the most serious rival to General Relativity has long been almost entirely omitted in the General Relativity literature, living instead only in the particle physics literature.

One should perhaps mention some other theories also, such as Kaluza-Klein extra dimensional unified field theories, metric-affine theories of Weyl, Eddington, Einstein, *inter alia*, and other unified field theories (Goenner, 2004). This is a large subject, albeit one of more relevance to mathematicians than to astrophysicists and cosmologists. If one were to add such theories to the table, it seems that one would find that most or all unified field theories were not particularly plausible *a priori*, fit the data about was well as GR (as long as the unification didn’t accomplish much) or worse, and typically did not differ from General Relativity philosophically *in the respects that I have in mind*, which involve general covariance. Unified theories might have differed from GR in other, fascinating philosophical respects, such as metric-affine geometry readily permits (Reichenbach, 1928, Appendix) (Grünbaum, 1970), but usually they were intended to preserve and extend the 'spirit' of General Relativity. Thus no serious rival to the spirit of GR is to be found here, unless (as in the work of Tonnelat) a unified field theory makes gravity massive.

10 Two Communities and Two Methodologies

We have seen a contrast between two approaches to gravitational physics: one that considers a wide array of potential theories and then finds empirical and theoretical criteria to eliminate most of the options, suspending judgment among those that remain, and one that is primarily interested in "our best theory", what it means, and where it came from in the actual contingent history. As Lakatos urged, any history of science that aims to display genuine rational progress, not mere change, must compare the contingent actual history to normative criteria for what should have happened (Lakatos, 1970; Lakatos, 1971). Noretta Koertge took pains to highlight strands in Lakatos's thought that suggest the importance of rival scientific theories in the progress of science (Koertge, 1971). Bayesianism shows that how strongly a body of evidence supports a theory depends on what other theories exist, not simply on the theory of interest (Earman, 1992; Shimony, 1970).

Lakatos's sense of assessing the rationality of history can be compared with one that is often present tacitly in the history and philosophy of space-time theory as well as among general relativists. The latter attitude was articulated with unusual clarity by Jürgen Ehlers (Ehlers, 1973), a dominating figure in the revived (West) German community of general relativists. This attitude helps to *explain* why serious rivals (in the sense defined above), in particular those making a philosophical difference (condition **D**), would tend to be neglected. Ehlers was commenting on what were fairly recent (1950s-60s) results showing that one could derive Einstein's equations, their effective curved space-time, nonlinearity, *etc*., starting with the mundane assumption of a massless spin 2 field in a flat space-time, as shown by Kraichnan, Feynman, Thirring, Wyss, Deser, and the like. A natural and difficult question is what these results mean (recently addressed (Pitts, 2016a)).

> "In the opinion of the author these remarkable results indicate strongly that there is no satisfactory flat space theory of gravity.... To interpret these results as showing that Einstein's theory may as well be considered as a somewhat peculiar Poincaré invariant theory with a complicated gauge group seems (to me) inappropriate and misleading.... *The physicist's conception of spacetime has been changed profoundly in the transition from special relativity to general relativity, and a return to the earlier, narrower scheme is as improbable as a return from quantum to classical mechanics.*" (Ehlers, 1973, p. 85) (emphasis added)

Ehlers' interpretive remark has some justice to it: even if one starts with flat space-time, one arrives at curved space-time, so curved space-time seems like the right place to be.

What is of special interest at the moment, however, is the degree of certainty at which Ehlers feels able to arrive without attention to rival theories. Ehlers wrote at a time when the results showing that massive spin 2 gravity was problematic were very new; certainly he does not cite the van Dam-Veltman-Zakharov discontinuity or the Boulware-Deser ghost. Such negative results would have provided a powerful—indeed far more convincing—way to make a point in favor of General Relativity (Boulware and Deser, 1972). One can compare the approach to Mittelstaedt's work (Mittelstaedt, 1970) above; Mittelstaedt eschews discerning the trajectory of historical progress in favor of nuts-and-bolts facts and logical-mathematical reasoning. What is striking is that Ehlers felt able to claim irreversible progress and certainty while feeling no need to entertain massive spin 2 gravity at all, not even to refute it. And yet flat space-time plays an essential (albeit not directly observable) quantitative role (Ogievetsky and Polubarinov, 1965; Freund et al., 1969); those papers contained highly sophisticated and novel calculations and deep conceptual insights, many of which have been rediscovered in the last 7 years. In the same time period, others were placing empirical limits on the photon and graviton masses (Goldhaber and Nieto, 1971; Goldhaber and Nieto, 1974). Such efforts continue (Arun and Will, 2009; Abbott et al., 2016). Mass and spin being the two parameters for classifying relativistic wave equations, simply ignoring one of them is unjustified.

Presumably Ehlers was making not merely a descriptive sociological prediction, but also a normative claim that something has happened historically that makes it *rational* to be so committed to curved space-time. This is Progress (Bury, 1920; Niiniluoto, 1980). Whatever happened to bring this about, surely had happened several decades before he wrote, and probably in the 1910s. Thus Ehlers was at least implicitly committed to the view that what proponents of General Relativity did not know, could not hurt them, concerning some seemingly relevant future or possible results. How is that possible? I find it difficult to understand Ehlers' epistemology except in terms of a broadly Hegelian methodology: Absolute Spirit is progressively manifesting Itself in history (though not necessary dialectically). Here Ehlers's method fits nicely with the prophetic role that Pais's standard scientific biography unashamedly ascribes to Einstein (Pais, 1982, pp. 305, 306). In the early days, the rapturous reception of General Relativity by Schlick (Schlick, 1920, pp. 70-75) and aggressive advocacy by Reichenbach (Reichenbach et al., 2006) (not leaving space for the idea that less revolutionary concepts than Einstein's might have worked) may not have been wholly unrelated to the Vienna Circle's political agenda (discussed, *e.g.*, by (Galison, 1996; Okruhlik, 2004) and references therein).

Much as Ehlers the practitioner felt surprisingly free to draw confident conclusions without entertaining relevant rival theories, and physicist-historian Pais has cast Einstein in a prophetic role, philosopher-historian John Norton as a commentator has called attention to lacunae in reasoning in favor of Einstein's theory and to a tendency towards mysticism (Norton, 1995; Norton, 2016). Norton, however, writes as a skeptic rather than an advocate.

> In this [20th] century, there seems to be a strong temptation to represent the generation of scientific discoveries, especially those of the caliber of general relativity, as somehow miraculously transcending reason and analysis. Perhaps the fear is that we would respect Einstein less if we realized that his toolbag was filled with the same instruments as are used in the common reasoning of science. Such a fear is surely unwarranted. We ought to respect an Einstein all the more when we find that he wrought his miracles with tools and materials available to everyone, day to day. (Norton, 1995, pp. 62, 63)

Unlike Ehlers and Pais, Norton has normative doubts. Fortunately he proposed a remedy, eliminative induction. Such a remedy finds its fullest fulfillment in the particle physics literature, according to Pitts (Pitts, 2016a).

Norton concludes:

> Finally we might well wonder just how plausible it is for the process of discovery of a theory such a general relativity to be dominated by arational maneuvers. What faces any such process is an enormous number of candidate theories, the bulk of them essentially unarticulated. What kind of a process could select and articulate from this overwhelming flood a theory as able as general relativity to stand up to extensive later rational testing — both as to its internal logical structure and its

> foundation in experience? Could it be that a set of canons of rationality that cannot embrace such a process is in need of revision? Or are we prepared to entertain the possibility of mysterious processes realized in the human mind that achieve eminently rational ends by predominantly arational means? (Norton, 1995, pp. 63, 64)

I share Norton's hesitation on that point, especially because particle physics clearly shows how unnecessary such a methodology akin to inspiration is for space-time theory. Something like eliminative induction or Bayesianism will suffice: try all the serious possibilities and eliminate those that do not work. The result is that it is difficult to differ much from General Relativity; but whether one allowed way of differing involves a graviton mass has long been a subtle and crucial question.

11 Conclusion

It is evident that a particle physics-aware view of 20th century gravity differs from the default view among historians and philosophers of space-time that General Relativity has been so clearly the best theory almost since it appeared as to warrant acceptance, and that nothing much has happened in physics relevant to space-time theory in 95+ years except further derivations from the closed canon of works by Einstein or about his theory. In light of particle physics, the answer to deep questions of theory choice and conceptual lessons about space-time theory depends on surprises found in sorting out fine technical details, with the years around 1970 and since 2010 being key. Philosophers should not feel professionally bound to avoid such matters. Neither should we assume that all the relevant physics has already been worked out long ago and diffused in textbooks. Much of it can hardly be found in books at all (except some in (Zee, 2013)). General Relativity was the best theory for most or all of that 95+-year period, plausibly better even than massive gravities collectively. But especially during some years prior to 1970 General Relativity (a massless spin 2 theory) was not so clearly superior to massive spin 2 gravity to warranted wholehearted acceptance among the particle physics-aware. In the 2010s the fortunes of massive spin 2 gravity improved dramatically, with the prospect of evading both horns of the early 1970s dilemma. Subsequent developments indicated more subtle difficulties (Deser and Waldron, 2013; Deser et al., 2015), though they are contested. The subject is highly dynamical and difficult to predict long-term. In any case it is risky to continue ignoring the literature after the 1910s as though it contained just more realization of how right Einstein was. While one probably will arrive at the right answer anyway, it will not be for the right reasons. Thus one has, if perhaps not quite a textbook problem of unconceived alternatives, at least something close enough to merit attention, in real contemporary science.

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