

Underconsideration in Space-time and Particle Physics

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Abstract

The idea that a serious threat to scientific realism comes from unconceived alternatives has been proposed by van Fraassen, Sklar, Stanford and Wray among others. Peter Lipton's critique of this threat from underconsideration is examined briefly in terms of its logic and its applicability to the case of space-time and particle physics. 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 case putting gauge freedom, formal indeterminism *vs.* determinism, the presence of the hole argument, *etc.*, at risk. 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.

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1 Introduction

One might wonder whether there exist interesting moderate forms of skepticism between radical Humean skepticism, which is perhaps too serious to matter because it is everyone’s problem, and (non-skeptical) scientific realism. The problem of unconceived alternatives or underconsideration has been proposed as an example (van Fraassen, 1989, p. 143) (Sklar, 1985; Stanford, 2006; Roush, 2005; Wray, 2008; Khalifa, 2010). That problem is roughly that scientific theories might be accepted supposedly on the grounds of evidence in their favor, 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, whether long past or (as in this paper) more current, 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, in part, an unfortunate consequence of the human condition. But it is not the sort of phenomenon that enhances the credibility of science. The problem is not merely one that past scientists faced, for why not think that the same sort of situation arises today? Are we merrily detaching consequences from “our best theory” to learn about the world, when in fact the world might well be different from how we think it is, 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. Scientific theories’ predictions don’t spring up full-grown like Athena from the head of Zeus, even if the logic isn’t complicated by auxiliary hypotheses.

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 seems to depend on what one is trying to do. If one wishes to learn how events in the past led humans (not Bayesian demigods, Laplacian intelligences, or similar exotica) to have solid knowledge (true and justified belief, more or less), 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 some normative criterion if one is interested in ascertaining the growth of knowledge, as opposed to mere variation of belief over time (Lakatos, 1971; Lakatos, 1970). This does not require fabrication or consigning real history to footnotes, of course; 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 the chemical revolution (Chang’s example), collecting those parts that contribute good evidence and argument to the human heritage.

Does one truly *need* history to make a case for underdetermination? Isn’t it enough simply to produce alternative theories and attend to evidence in the present? I suspect that in principle the history is not needed. Particle physics does pretty well without careful attention to history or philosophy, though it could do better in some respects. (Its pragmatic approach to statistics has been discussed in the wake of the Higgs boson discovery (Cousins, 2015).) But in fact we modern people 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, ahead of schedule, in which case one shouldn’t cautiously wait for the evidence—General Relativity being a key example, as will appear below). That is especially true in science, where of course the presumption is very often demonstrably correct. But there might be pockets here and there where, say, historical contingency and revolutionary zeal combined in a way led to premature dogmatic acceptance of an idea, and later investigation might bring that fact to light. Hence history is useful to rebut the presumption that the winners won because they were right. Probably they did, but maybe in some cases specific investigation could show otherwise. Is the Copenhagen interpretation of quantum mechanics hegemonic by accident? So it has been argued (Cushing, 1994). Whatever one’s take on that issue, there are other examples that will be discussed below.

Peter Lipton tried to show that the problem of underconsideration or unconceived alternatives is unreal, because it is based upon contradictory assumptions (Lipton, 1993). Much of what one might want to say in reply has been said by Wray (Wray, 2008; Wray, 2012). I would add that Lipton’s attempt to derive a contradiction between the premises of the underconsideration argument gives the appearance of working only by his obtruding onto his anti-realist opponent a premise that the anti-realist has no reason to accept (Pitts, 2016e).

2 Underdetermination in Electromagnetism: A Difference that Make a Difference

To build up some momentum for making the effort to encounter some particular physics, it is worthwhile to recall a key feature of some fundamental physics theories, namely “gauge freedom.” The fundamental theories are quantum mechanical (quantum field theories), but in line with how even physicists tend to approach them, let us start with their non-quantum ‘classical’ ancestors. (Unlike some physicists, we’ll stop there.) Maxwell’s electromagnetism is generally first encountered expressed in terms of electric and magnetic fields \vec{E} and \vec{B} , or perhaps in a more manifestly relativistic form using the electromagnetic field strength (the “Faraday tensor”) $F_{\mu\nu}$, which combines \vec{E} and \vec{B} into a manifestly relativistically covariant entity. 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 scalar and vector potentials ϕ and \vec{A} , or their relativistic combination 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 : “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 generally agreed that there is no problem because the real

physical content is not (all of) A_μ , but the equivalence class of choices of A_μ giving the same electromagnetic field $F_{\mu\nu}$. There are global subtleties (such as the Aharonov-Bohm effect), but let us ignore those. There are analogous but more complicated gauge invariances involved in the weak and strong nuclear forces, which use Yang-Mills fields, like several electromagnetisms bound together in a complicated way (Morrison, 2000). This gauge freedom is more interesting because there is no gauge-invariant local field strength; the analog of $F_{\mu\nu}$, though less gauge-dependent than the analog of A_μ , is still gauge-dependent. These gauge freedoms are both interestingly like and interestingly unlike the coordinate freedom of General Relativity, “general covariance.” Metaphysics can be radically unstable under small changes in physics, a fact that makes underdetermination and underconsidered/unconceived alternatives serious issues in the philosophy of physics.

What the cases of underdetermination discussed below and their analogs show is, among other things, that *we do not know and perhaps never will know that gauge freedom is a feature of real electromagnetic processes*. 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 instead. In Maxwell’s theory, light travels at (naturally) the speed of light and hence at the same speed, 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 a limiting speed achieved at high frequencies. The Lagrangian density sprouts a “photon mass” term the mass term $-\frac{1}{2}m^2 A^\mu A_\mu$, which breaks the gauge symmetry. Crucially, de Broglie-Proca electromagnetism has no gauge freedom: all of A_μ is physical wheat, with no chaff, that is, no gauge freedom (Jackson, 1975; Sundermeyer, 1982). The field equations are deterministic. In some respects de Broglie-Proca electromagnetism is cleaner to work with and simpler to understand than Maxwell’s theory. How fast light of a given frequency travels depends upon a new parameter m that distinguishes theories within the de Broglie-Proca family T_m . The meaning of m is the “mass of the photon,” though the concept makes sense in terms of classical waves (without photons, particles of light) and is essentially a new inverse length scale in the equations. 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.

Empirically, 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. Because we examine the world with blunt fingers, all that we can do, presumably, 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 seems to be 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 would count more strongly against massive photons.) For any experimental setup, there are sufficiently small photon masses m that are indistinguishable from 0 by that experiment. 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 . For any experiment, there exists a photon mass m that evades it. If someone manages to come up with a plausible objective prior probability distribution for the photon mass, then it might be possible to show that experimental data make it highly probable that the photon is massless. Until then one has taste, opinion, judgment. Not to *care* about the difference between $m = 0$ and $m \neq 0$ is to embrace something akin to instrumentalism.

Perhaps inspired by the logical fact $\forall \exists \neq \exists \forall$ that existential quantifiers do not commute with universal quantifiers, one might consider switching the roles of what is varied and what is fixed. In the real world, the mass of the photon, whatever it is, doesn't vary. A theorist might boldly come out in favor of de Broglie-Proca electromagnetism and then back-peddle to ever lower values of m when pressed by facts, but the world does not. Hence we can hope that experiments will catch the photon mass some day if it isn't 0. For any choice of a theory (nonzero value of m) from the family of de Broglie-Proca theories, there exists in principle an experiment that detects 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.

3 Exemplifying 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, a 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.

4 Clarification of Particle Physics Terminology

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 given above. 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 (Wigner, 1939; Pauli and Fierz, 1939; Wentzel, 1973). There is no handy alternative language for these concepts. The “spin” is determined by 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 Planck’s reduced constant $\hbar = h/(2\pi)$, a particle physicist will call Maxwell’s theory the theory of a massless spin 1 particle or massless photon; likewise with gravity and “graviton” as spin 2. Massive particles previously have been called “mesons”; the “masses” involved are actually inverse length scales in the classical field equations (with the speed of light c and the reduced Planck constant \hbar both set to 1). As noted above for electromagnetism, a field is “massive” if its equation of motion contains an algebraic linear term in the field potential; quantization yields “particles” (quanta) with nonzero rest mass. Their frequency determines their speed; for high frequencies they move at nearly the speed c .

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 that privilege 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 to put massive theories into the running (*c.f.* (Thorne et al., 1973; Lee et al., 1974; Thorne et al., 1971), where massive theories are missing).

5 Serious Quantitative Underdetermination Rivalries

As will appear shortly, there are gravitational analogs, both dated and contemporary, to the electromagnetic rivalry just discussed. 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 must be assessed on a case-by-case basis. Adding a mass term and getting underdetermination between massless and low-mass particles/fields is only a heuristic, not an algorithm, evidently. 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 some skill in physical theory construction and some 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 it is easy to 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 in generating the rivals,
- 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, their prior probability (collectively) $P(T)$, obtained by integrating the probability over particle mass, should also be 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).) 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 in Bayesianism (Shimony, 1970, p. 132) (Salmon, 1990)—and in reality. 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.

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, as scientific realists among them would prefer. Einstein’s arguments are often not so compelling (Norton, 1995). Such theories, once conceived, pose a problem of underdetermination of theory by data. 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 massive scalars are well known as a toy theory (the Klein-Gordon equation (Kragh, 1984)), their use in a gravitational context has received consideration in less than a dozen papers ever, several of them recent (Pitts, 2011a; Pitts, 2011b; Pitts, 2016d; Pitts, 2016c).

Table 1: Blanks in the Taxonomy of Spin and Mass

	Spin 0	Spin 1	Spin 2
$m = 0$	Nordström	Maxwell	Einstein
$m \neq 0$?	dB-Proca	?

6 Unconceived Alternative: Massive Scalar Gravity

Nordström’s scalar gravity, a serious competitor to Einstein’s program for some years 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 due to T. Y. Thomas (Thomas, 1925; Pitts, 2016d), 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 rather than just the 10-parameter Poincaré group standard in 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, as a consequence of the smoothness of the limit of a massive scalar field theory as the mass goes to zero (Boulware and Deser, 1972) (Weinberg, 1995, p. 246). There is therefore a one-sided permanent underdetermination between Nordström’s theory and its 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, 2016d).

Boulware and Deser wrote a very influential work (among the particle physics-aware who are interested in gravity) on the possibility of massive variants of Einstein’s General Relativity (Boulware and Deser, 1972). While they had quite 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, the news was that underdetermination did not always arise; the expectation that massive theories are worth exploring, lack gauge freedom and can approximate massless theories arbitrarily well (though still unfamiliar in HPS) was already assumed.

7 Massive GR: Unconceived or Underconsidered

The precedent of massive electromagnetism motivated some authors to suppose that, given the analogy between electromagnetism as a massless spin 1 gauge theory and GR as a massless spin 2 gauge theory, there might well be a viable massive variant(s) of GR to be found. (Massive scalar gravity would 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). A key result with Markus Fierz was 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 (with inspiration 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, 2016f), 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. (The matter action is much 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 General Relativity arbitrarily closely, but completely differing from Einstein's theory in terms of the conceptual novelties involved (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 (Berends et al., 1979; Berends and van Reisen, 1980). For spin 1 and higher, one can construct a spin 0 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)), were another matter. Prior to 1970, the conceptual novelty of GR was *optional*, though fascinating and plausible, because one would expect it to be broken by a mass term and there was no good reason to reject a mass term. Simply failing to entertain such theories was to suffer from underconsideration or unconceived alternatives. One should not bet on it, but sometimes what you don't know, won't hurt you; lack of imagination finally was compensated by good luck. There finally being of 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), the acceptance of General Relativity itself, and not suspension of judgment between it and a massive variant(s), rightly seemed compelling to most particle physicists who worked on gravity, the community that did not suffer seriously from unconceived alternatives. (Even particle physics turned out to have a small problem of unconceived alternatives, however, it turned out in recent years.)

As one can see from Klaus Hentschel's exhaustive work, if one knows which absences to look for, 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). 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 1920s or 1920s or even 1938 is completely inadequate for assessing why *we today* should accept GR. The problem is not necessarily with histories of General Relativity, but with the common tacit assumption that they have some direct applicability to us today.

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. 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 draw an analogy between this massive

Newtonian gravity and his new cosmological constant Λ term. Unfortunately this analogy is false (Heckmann, 1942; 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. Unfortunately Einstein’s false analogy has continued to bewitch serious historians of physics until quite recently (North, 1965, p. 179) (Jammer, 1993, pp. 194, 195) (Pais, 1982, p. 286) (Kragh, 2004, p. 28). Engelbert Schücking, who was a student of Heckmann, mounted a vigorous, accurate and entertaining 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 anyone 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 in the 1970s, with the failure of massive gravity, would have been the right time to say what Schlick said prematurely in the 1910s. Probably 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, but did not recognize the *conceptual* significance of massive gravity.

Mittelstaedt’s eliminative induction approach to space-time theory was inspired by Gupta (Gupta, 1952; Gupta, 1954; Gupta, 1957). It 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 Alex Blum)

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 not differ on this point.

8 Recent Breakthroughs in Massive Gravity?

With few exceptions, there matters stood until the late 1990s, when they started slowly to re-open 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.*, (Vainshtein, 1972; Hamamoto, 1996; Visser, 1998; Dvali et al., 2000; Gruzinov, 2001; Scharf, 2001; Damour and Kogan, 2002; Deffayet et al., 2002; Damour et al., 2003; Arkani-Hamed et al., 2003; Babak and Grishchuk, 2003; Petrov, 2004; Gabadadze and Shifman, 2004; Creminelli et al., 2005; Deffayet and Rombouts, 2005; Vainshtein, 2006; Dvali et al., 2008; Rubakov and Tinyakov, 2008; Grigore and Scharf, 2008; de Rham et al., 2008; Izumi and Tanaka, 2009; Babichev et al., 2009; Babichev et al., 2010; de Rham et al., 2011; Hinterbichler, 2012; 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. It is worthy of notice by philosophers of science.

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 Arnowitt-Deser-Misner (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 an odd power $|N|$ after all in a sense. 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).

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.

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

One can now recall Lipton’s attempted refutation of the problem of underconsideration (Lipton, 1993) in light of these physical examples. As he formulates the issue, the arguer from underconsideration from underdetermination holds that (1) scientists’ ability to test theories is reliable as far as ranking *proposed* theories is concerned, but (2) there is little reason to believe that the true theory is among those proposed. One thus has to distinguish Lipton’s unitary group of “scientists” (more to the point, theoretical physicists) into two groups: those who employ conjectural principles to delimit the theories entertained, and those who more cautiously entertain a wider range of theories consistent with empirical knowledge to date.

On this view, to believe that the best available theory is true would be rather like believing that Jones will win the Olympics when all one knows is that he is the fastest miler in Britain. (Lipton, 1993)

If one employs conjectural principles to delimit the theories entertained (akin to conjecturing that the fastest miler in Britain is the fastest miler, period), then there is little reason to believe that the true theory is among those proposed; that practically follows from the meaning of “conjectural.” One could, perhaps, dogmatize more boldly by denying that the conjectural principle is conjectural: *we know* that there is gauge freedom/no background geometry in electromagnetism/gravity/both, so the truth *is* among the theories entertained by those who insist on gauge freedom/no background geometry! But such a move, far from profiting from the philosophy of science in scientific practice, largely illustrates the familiar fact that a sufficiently strong prior commitment to a view tends to leave one strongly committed to it even after encountering contrary evidence. This is just pounding the table. One can analogously assign nearly extreme priors in Bayesianism—perhaps near 10^{-20} and $1 - 10^{-20}$, say—consistent with the letter of

subjective Bayesianism. But a *tempered* subjectivism, assigning non-negligible priors to seriously proposed theories—including massive electromagnetism and massive spin-2 gravity—is more prudent scientifically and otherwise (Shimony, 1970).

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. **Plausible** (fairly high prior probability?) in terms of local relativistic field theory.
2. **Different** from GR in terms of its philosophical lessons.
3. **Confirmed** by the data. not easily distinguished from GR *empirically* (similar likelihoods $P(E|T)$ where GR does well).

Hence a serious rival will be **Plausible**, **Different**, and **Confirmed**. These criteria are patterned after rational decision theory, with utility construed in terms of making a philosophical difference compared to the default theory. 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.) 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, so none is a serious rival in the specified sense. (It is of course not impossible that I have missed something noteworthy, 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, hence having a low prior probability in comparison to General Relativity (Whitrow and Morduch, 1960). Despite paying attention to “Lorentz invariant” theories, unfortunately they omit massive variants of General Relativity. One could easily enough draw an inductive lesson that “Lorentz invariant” gravitation theories all 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 likes 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 gravities 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, especially by people for whom the theories are named, 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?	$\neg P, D/P, \neg D, C?$

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; Zajt, 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). By contrast the massive gravities entertained here all have at least the special relativistic symmetry group. 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 been almost entirely omitted in the General Relativity literature, living instead only in the particle physics literature.

10 Hegelian Scientific Methodology?

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). It is a consequence of Bayesian confirmation theory that how strongly a body of evidence supports a theory depends on what other theories exist, not simply the theory of immediate 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. 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)

Ehler’s 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 arrive.

What is of special interest at the moment, however, is the degree of certainty at which Ehlers feels able to arrive (claiming that the collective “physicist” is with him) without any attention to rival theories. Ehlers wrote at a time when the results showing that massive spin 2 gravity was problematic were very new, so new that he might well not have known them. 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). What is striking is that he 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 6 years. But Ehlers in 1972 did not feel any need to entertain the idea of a graviton mass, which would have made a flat space-time interpretation difficult to resist, before arriving at certainty in favor of curved space-time. One can compare the approach to Mittelstaedt’s work (Mittelstaedt, 1970) above. In the same time period, others were placing empirical limits on the photon and graviton masses (Goldhaber and Nieto, 1971; Goldhaber and Nieto, 1974) rather than failing to entertain the idea and announcing irreversible progress. Efforts to place tighter bounds on the graviton mass are occurring once more among those sensitive to evidence on that issue (Arun and Will, 2009;

Abbott et al., 2016).

Presumably Ehlers was making not merely a descriptive sociological prediction based on physicists' relatively newfound deeply held commitment to curved space-time, but rather 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. Prior to the availability of particle physics arguments against every form of massive General Relativity in the early 1970s, the live epistemic possibility that experimental results might show the effects of a mass term, and hence confirm a merely Poincaré-invariant special relativistic theory of gravity as opposed to General Relativity, also did not undermine the rationality of their commitment to curved space-time, at least on their implicit view. 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).

My puzzlement is not wholly unprecedented (Norton, 1995; Norton, 2016). As Norton wrote in the 1990s,

[i]n this 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 toolbox 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)

Fortunately Norton proposed a remedy, eliminative induction. Such a remedy finds its fullest fulfillment in the particle physics literature, I would add (Pitts, 2016a).

Norton concludes:

Finally we might well wonder just how plausible it is for the process of discovery of a theory such as 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)

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, some since 2010. 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 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, at least among the particle physics-aware. (Given the problems of acceptance rules (Swain, 1970), acceptance has not obviously survived the transition to Bayesianism anyway. It is thus still less clear why one needs to anoint any theory “our best” and forget about rivals.) 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 might well arrive at the right answer, it will not be for the best reasons.

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