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Progress and Gravity: Overcoming Divisions Between General Relativity and Particle Physics and Between Physics and HPS

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13.1 Introduction: Science and the Philosophy of Science

The ancient "problem of the criterion" is a chicken-or-the-egg problem regarding knowledge and criteria for knowledge, a problem that arises more specifically in relation to science and the philosophy of science. How does one identify reliable knowledge without a reliable method in hand? But how does one identify a reliable method without reliable examples of knowledge in hand? Three possible responses to this problem were entertained by Roderick Chisholm: one can be a skeptic, or identify a reliable method(s) ("methodism"), or identify reliable particular cases of knowledge ("particularism") (Chisholm, 1973). But why should the best resources be all of the same type? Might not some methods and some particular cases be far more secure than all other methods and all other particular cases? Must anything be completely certain anyway? Why not mix and match, letting putative examples and methods tug at each other until one reaches (a personal?) reflective equilibrium?

This problem arises for knowledge and epistemology, more specifically for science and the philosophy of science, and somewhere in between, for inductive inference. Reflective equilibrium is Nelson Goodman's method for induction (as expressed in John Rawls's terminology). One need not agree with Goodman about deduction or take his treatment of induction to be both necessary and *sufficient* to benefit from it. He writes:

> *A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend.* The process of justification is the delicate one of making mutual adjustments between rules and accepted inferences; and in the agreement achieved lies the only justification needed for either.
>
> All this applies equally well to induction. An inductive inference, too, is justified by conformity to general rules, and a general rule by conformity to accepted inductive inferences. Predictions are justified if they conform to valid canons of induction; and the canons are valid if they accurately codify accepted inductive practice. (Goodman, 1983, p. 64, emphasis in the original)

Most scientists and (more surprisingly) even many philosophers do not take Hume's problem of induction very seriously, although philosophers talk about it a lot. As Colin Howson notes, philosophers often declare it to be insoluble and then proceed as though it were solved (Howson, 2000). I agree with Howson and Hans Reichenbach (Reichenbach, 1938, pp. 346, 347) that one should not let oneself off the hook so easily. That seems especially true in cosmology (Norton, 2011). Whether harmonizing one's rules and examples is sufficient is less clear to me than it was to Goodman, but such reflective equilibrium surely is *necessary* – although difficult and perhaps rare.

My present purpose, however, is partly to apply Goodman-esque reasoning only to a *special case* of the problem of the criterion, as well as to counsel unification within physical inquiry. What is the relationship between philosophy of science (not epistemology in general) on the one hand, and scientific cosmology and its associated fundamental physics, especially gravitation and space-time theory (not knowledge in general) on the other? Neither dictation from philosopher-kings to scientists (the analogue of methodism) nor complete deference to scientists by philosophers (the analogue of particularism) is Goodman's method. It is not popular for philosophy to give orders to science, but it once was. The reverse is more fashionable, a form of scientism or at least a variety of naturalism. I hope to show by examples how sometimes each side should learn from the other.

While Goodman's philosophy has a free-wheeling relativist feel that might make many scientists and philosophers of science nervous, one finds similar views expressed by a law-and-order philosopher of scientific progress, Imre Lakatos. According to him, we should seek

> a pluralistic system of authority, partly because the wisdom of the scientific jury and its case law has not been, and cannot be, fully articulated by the philosopher's statute law, and partly because the philosopher's statute law may occasionally be right when the scientists' judgment fails. (Lakatos, 1971, p. 121)

Thus there seems to be no irresistible pull toward relativism in seeking reflective equilibrium rather than picking one side always to win automatically.

13.2 Healing the GR *vs.* Particle Physics Split

A second division that should be overcome to facilitate the progress of knowledge about gravitation and space-time is the general relativist *vs.* particle physicist split. Carlo Rovelli discusses

> ...the different understanding of the world that the particle physics community on the one hand and the relativity community on the other hand, have. The two communities have made repeated and sincere efforts to talk to each other and understand each other. But the divide remains, and, with the divide, the feeling, on both sides, that the other side is incapable of appreciating something basic and essential.... (Rovelli, 2002)

This split has a fairly long history going back to Einstein's withdrawing from mainstream fundamental physics from the 1920s – that largely being quantum mechanics, relativistic quantum mechanics and quantum field theory. A further issue pertains to the gulf between how Einstein actually found his field equations (as uncovered by recent historical work (Renn, 2005; Renn and Sauer, 1999, 2007)) and the much better known story that Einstein told retrospectively. Work by Jürgen Renn *et al.* has recovered the importance of Einstein's "physical strategy" involving a Newtonian limit, an analogy to electromagnetism, and a quest for energy-momentum conservation; this strategy ran alongside the better advertised mathematical strategy emphasizing his principles (generalized relativity, general covariance, equivalence, etc.). Einstein's reconstruction of his own past is at least in part a persuasive device in defense of his somewhat lonely quest for unified field theories (van Dongen, 2010). Readers with an eye for particle physics will not miss the similarity to the later successful derivations of Einstein's equations as the field equations of a massless spin-2 field assumed initially to live in flat Minkowski space-time (Feynman *et al.*, 1995), in which the resulting dynamics merges the gravitational potentials with the flat space-time geometry such that only an effective curved geometry appears in the Euler–Lagrange equations. One rogue general relativist has recently opined:

> HOW MUCH OF AN ADVANTAGE did Einstein gain over his colleagues by his mistakes? Typically, about ten or twenty years. For instance, if Einstein had not introduced the mistaken Principle of Equivalence and approached the theory of general relativity (GR) via this twisted path, other physicists would have discovered the theory of general relativity some twenty years later, via a path originating in relativistic quantum mechanics. (Ohanian, 2008, p. 334, capitalization in the original).

It is much clearer that these derivations work to give Einstein's equation than it is what they mean. Do they imply that one need not and perhaps should not ever have given up flat space-time? Do they, on the contrary, show that theories of gravity in flat space-time could not succeed, because their best effort turns out to give curved space-time after all (Ehlers, 1973)? Such an argument is clearly incomplete without contemplation of massive spin-2 gravity (Freund *et al.*, 1969; Ogievetsky and Polubarinov, 1965). But it might be persuasive if massive spin-2 gravity failed – as it seemed to do roughly when Ehlers wrote (not that he seems to have been watching). But since 2010 massive spin-2 gravity seems potentially viable again (de Rham *et al.*, 2011; Hassan and Rosen, 2011; Maheshwari, 1972) (though some new issues exist). Do the spin-2 derivations of Einstein's equations suggest a conventionalist view that there is no fact of the matter about the true geometry (Feynman *et al.*, 1995, pp. 112, 113)? Much of one's assessment of conventionalism will depend on what one takes the modal scope of the discussion to be: Should one consider only one's best theory (hence the question is largely a matter of exegeting General Relativity, which will favor curved space-time), or should one consider a variety of theories? According to John Norton, the philosophy of geometry is not an enterprise rightly devoted to giving a spurious air of necessity to whatever theory is presently our best (Norton, 1993, pp. 848, 849). Such a view suggests the value of a broader modal scope for the discussion than just our best current theory. On the other hand, the claim has been made that the transition from Special Relativity to General Relativity is as unlikely to be reversed as the transition from classical to quantum mechanics (Ehlers, 1973, pp. 84, 85). If one aspires to proportion belief to evidence, that is a startling claim. The transition from classical to quantum mechanics was motivated by grave empirical problems; there now exist theorems (no local hidden variables) showing how far any empirically adequate physics *must* diverge from classical.

But a constructive derivation of Einstein's equations from a massless spin-2 shows that one can naturally recover the phenomena of GR without giving up a special relativistic framework in a sense. The cases differ as twilight and day. Ehlers's remarks are useful, however, in alerting one for Hegelian undercurrents or other doctrines of inevitable progress in the general relativity literature. A classic study of doctrines of progress is Bury (1920).

13.3 Bayesianism, Simplicity, and Scalar *vs.* Tensor Gravity

While Bayesianism has made considerable inroads in the sciences lately, it is helpful to provide a brief sketch before casting further discussion in such terms. I will sketch a rather simple version – one that might well be inadequate for science, in which one sometimes wants uniform probabilities over infinite intervals and hence might want infinitesimals, for example. Abner Shimony's tempered personalism discusses useful features for a scientifically usable form of Bayesianism, including open-mindedness (avoiding prior probabilities so close to 0 or 1 that evidence cannot realistically make much difference (Shimony, 1970)) and assigning non-negligible prior probabilities to seriously proposed hypotheses.

With such qualifications in mind, one can proceed to the sketch of Bayesianism. One is not equally sure of everything that one believes, so why not have degrees of belief, and make them be real numbers between 0 and 1? Thus one can hope to mathematize logic in shades of gray *via* the probability calculus. Bayes's theorem can be applied to a theory T and evidence E:

$$P(T|E) = P(T)\frac{P(E|T)}{P(E)}. \tag{13.1}$$

One wakes up with degrees of belief in all theories (!), "prior probabilities". One opens one's eyes, beholds evidence E, and goes to bed again. While asleep one revises degrees of belief from priors $P(T)$ to posterior probabilities $P(T|E)$. Today's $P(T|E)$ becomes tomorrow's prior $P(T)'$. Then one does the same thing tomorrow, getting some new evidence E', etc. Now the priors $P(T)$ might be partly subjective. If there are no empirically equivalent theories and everyone is open-minded, then eventually evidence should bring convergence of opinion over time (though maybe not soon).

A further wrinkle in the relation between evidence and theory comes from looking at the denominator of Bayes's theorem, $P(E) = P(E|T)P(T) + P(E|T_1)P(T_1) + P(E|T_2)$ $P(T_2) + \ldots$. While one might have hoped to evaluate evidence theory T simply in light of evidence E, this expansion of $P(E)$ shows that such an evaluation is typically undefined, because one must spread degree of belief $1 - P(T)$ among the competitors T_1, T_2, etc. Hence the predictive likelihoods $P(E|T_1)$, etc., subjectively weighted, appear unbidden in the test of T by E. Theory testing generically is *comparative*, making essential reference to rival theories. This fact is sometimes recognized in scientific practice, but Bayesianism can alert one to attend to the question more systematically.

Scientists and philosophers tend to like simplicity. Simplicity might not be objective, but there is significant agreement regarding scientific examples. That is a good thing, because there are lots of theories, especially lots of complicated ones, way too many to handle.

If degrees of belief are real numbers (not infinitesimals), then normalization $\Sigma_i P_i = 1$ requires lots of 0s and or getting ever closer to 0 on some ordering (Earman, 1992, pp. 209, 210). There is no clear reason for prior plausibility to peak away from the simple end. Plausibly, other things equal, simpler theories are more plausible *a priori*, getting a higher prior $P(T)$ in a Bayesian context. Such considerations are vague, but the alternatives are even less principled.

One can now apply Bayesian considerations to gravitational theory choice in the 1910s. One recalls that Einstein had some arguments against a scalar theory of gravity, which motivated his generalization to a tensor theory. Unfortunately they do not work. As Domenico Giulini has said,

> On his way to General Relativity, Einstein gave several arguments as to why a special-relativistic theory of gravity based on a massless scalar field could be ruled out merely on grounds of theoretical considerations. We re-investigate his two main arguments, which relate to energy conservation and some form of the principle of the universality of free fall. We find such a theory-based *a priori* abandonment not to be justified. Rather, the theory seems formally perfectly viable, though in clear contradiction with (later) experiments. (Giulini, 2008, emphasis in original)

Scalar (spin-0) gravity is simpler than rank-2 tensor (spin-2). Having one potential is simpler than having ten, especially if they are self-interacting. With Einstein's help, Gunnar Nordström eventually proposed a scalar theory that avoided the theoretical problems mentioned by Giulini. Given simplicity considerations, Nordström's theory was more probable than Einstein's *a priori*: $P(T_N) > P(T_{GR})$. Einstein's further criticisms are generally matters of taste. So prior to evidence for General Relativity, it was more reasonable to favor Nordström's theory. As it actually happened, Einstein's "final" theory and the evidence from Mercury both appeared in November 1915, leaving little time for this logical moment in actual history. Einstein's earlier *Entwurf* theory (Einstein and Grossmann, 1996) could be faulted for having negative-energy degrees of freedom and hence likely being unstable (a problem with roots in Lagrange and Dirichlet (Morrison, 1998)), although apparently no one did so.

Where was the progress of scientific knowledge–truth held for good reasons? Mercury's perihelion gave non-coercive evidence confirming GR and disconfirming Nordström's theory. It was possible to save Nordström's theory using something like dark matter, matter (even if not dark – Seeliger's zodiacal light) of which the mass had been neglected (Roseveare, 1982). Hence there was scope for rational disagreement because Nordström's theory was antecedently more plausible

$$P(T_N) > P(T_{GR})$$

but evidence favored Einstein's non-coercively

$$0 < P(E_{Merc}|T_N) < P(E_{Merc}|T_{GR}).$$

The scene changed in 1919 with the bending of light, which falsified Nordström's theory: $P(E_L|T_N) = 0$. There were not then other plausible theories that predicted light bending, so $P(E_L|T_{GR}) \approx 1 >> P(E_L)$. It is possible to exaggerate the significance of this result, as happened popularly but perhaps less so academically (Brush, 1989), where a search for plausible rival theories that also predicted light bending was made. (Bertrand Russell may have considered Whitehead's to be an example (Russell, 1927, pp. 75–80).) Unfortunately many authors wrongly take Einstein's arguments against scalar gravity seriously (Giulini, 2008). In the long run one does not make *reliable rational* progress by siding with genius as soon as possible: Einstein made many mistakes (often correcting them himself), some of them lucky (Ohanian, 2008) (such as early rejection of scalar theories), followed by barren decades. Given this Bayesian sketch, it was rational to prefer GR over Nordström's scalar theory only when evidence from Mercury was taken into account, and not necessarily even then. The bending of light excluded scalar theories but did not exclude possible rival tensor theories.

13.4 General Relativity Makes Sense About Energy

Resolving conceptual problems is a key part of scientific progress (Laudan, 1977). In the 1910s and again in the 1950s controversy arose over the status of energy-momentum conservation laws of General Relativity. Given Einstein's frequent invocation of energy-momentum conservation in his process of discovery leading to General Relativity (Brading, 2005; Einstein and Grossmann, 1996; Renn and Sauer, 2007, 1999), as well as his retrospective satisfaction (Einstein, 1916), this is ironic. Partly in response to Felix Klein's dissatisfaction, Emmy Noether's theorems appeared (Noether, 1918). Her first theorem says that a rigid symmetry yields a continuity equation. Her second says that a wiggly symmetry yields an identity among Euler–Lagrange equations, making them not all independent. For General Relativity there are four wiggly symmetries, yielding the contracted Bianchi identities $\nabla_\mu \mathcal{G}^\mu_\nu \equiv 0$. In the wake of the conservation law controversies there emerged the widespread view that gravitational energy exists, but it "is not localized". This phrase appears to mean that gravitational energy is not anywhere in particular, although descriptions of it often do have locations. That puzzling conclusion is motivated by mathematical results suggesting that where gravitational energy is depends on an arbitrary conventional choice (a coordinate system), and other results that the total energy/mass does not.

While the energy non-localization lore is harmless enough as long as one knows the mathematical results on which it is based, it has self-toxifying quality. Having accepted that gravitational energy is not localized, one is likely to look askance at the Noether-theoretic calculations that yield it: pseudotensors. The next generation of textbooks might then dispense with the calculations while retaining the lore verbally. Because the purely verbal lore is mystifying, at that point one formally gives license to a variety of doubtful conclusions. Among these are that because General Relativity lacks conservation laws, it is false – a claim at the origins of the just-deceased Soviet/Russian academician A. A. Logunov's high-profile dissent (Logunov and Folomeshkin, 1977). One also hears (for references see Pitts (2010)) that the expansion of the universe, by virtue of violating conservation laws, is false (a special case of Logunov's claim). One hears that the expansion of the universe is a resource for creation science by providing a heat sink for energy from rapid nuclear decay during Noah's Flood. Finally, one hears that General Relativity is more open to the soul's action on the body than is earlier physics, because the soul's action violates energy conservation, but General Relativity already discards energy conservation anyway. That last claim is almost backwards, because Einstein's equations are logically equivalent to energy-momentum conservation laws (Anderson, 1967). (If one wants souls to act on bodies, souls had better couple to gravity also.) The question whether vanishing total energy of the universe (given certain topologies) would permit it to pop into being spontaneously is also implicated.

Given that Noether's theorems – the first, not just the second – apply to GR, can one interpret the continuity equations sensibly and block the unfortunate inferences? The Noether operator generalizes canonical stress-energy tensor to give conserved quantities due to symmetry vector fields ξ^μ (Bergmann, 1958; Goldberg, 1980; Sorkin, 1977; Szabados, 1991; Trautman, 1962). For simpler theories than GR, the Noether operator is a weight 1 tangent vector density $\mathfrak{T}^\mu_\nu \xi^\nu$, so the divergence of the current $\partial_\mu(\mathfrak{T}^\mu_\nu \xi^\nu)$ is tensorial (equivalent in all coordinate systems) and, for symmetries ξ^ν, there is conservation $\partial_\mu(\mathfrak{T}^\mu_\nu \xi^\nu) = 0$. GR (the Lagrangian density, not the metric!) has *uncountably many* 'rigid' translation symmetries $x^\mu \to x^\mu + c^\mu$, where $c^\mu{}_{,\nu} = 0$, for any coordinate system, preserving the action $S = \int d^4x\mathcal{L}$. These uncountably many symmetries yield uncountable conserved energy-momentum currents. Why can they not all be real? The lore holds that because there are infinitely many currents, really there are not any. But just because it is infinite does not mean it is 0 (to recall an old phrase). Getting $\infty = 0$ requires an extra premise, to be uncovered shortly. For GR, the Noether operator is a conserved but *non-tensorial differential* operator on ξ, depending on $\partial\xi$ also. Hence one obtains coordinate-dependent results, with energy density vanishing at an arbitrary point, etc., the usual supposed vices of pseudotensors. If one expects only one energy-momentum (or rather, four), it should be tensorial, with the transformation law relating faces in different coordinates. But Noether tells us that there are *uncountably many* rigid translation symmetries.

If one simply "takes Noether's theorem literally" (Pitts, 2010) (apparently novelly, although Einstein and Tolman (Tolman, 1930) said nice things about pseudotensors), then uncountably many symmetries imply uncountably many conserved quantities. How does one get $\infty = 0$? By assuming that the infinity of conserved energies are all supposed to be faces of the same conserved entity with a handful of components – the key tacit premise of uniqueness. Suppose that one is told in Tenerife that "George is healthy" and "Jorge está enfermo" (is sick). If one expects the two sentences to be equivalent under translation (analogous to a coordinate transformation), then one faces a contradiction: George is healthy and unhealthy. But if George and Jorge then walk into the room together, there is no tension: *George* $\neq$ *Jorge*. An expectation of uniqueness underlies most objections to pseudotensors, but it is unclear what justifies that expectation. Making more sense of energy conservation makes its appearance in Einstein's physical strategy in finding his field equations less ironic. Indeed, conservation due to gauge invariance is a key step in spin-2 derivations, which improve on Einstein's physical strategy (Einstein and Grossmann, 1996; Deser, 1970; Pitts and Schieve, 2001). Noether commented on *converses* to her theorems (Noether, 1918); one should be able to derive Einstein's equations from the conservation laws, much as the spin-2 derivations do using symmetric gravitational stress-energy (hence perhaps needing Belinfante–Rosenfeld technology).

But what is the point of believing in gravitational energy unless it does energetic things? Can it heat up a cup of coffee? Where is the physical interaction? Fortunately these questions have decent answers: gravitational energy is roughly the non-linearity of Einstein's equations, so it mediates the gravitational self-interaction.

Why did Hermann Bondi change from a skeptic to a believer in energy-carrying gravitational waves (Bondi, 1957)?[1] Given a novel plane wave solution of Einstein's equations in vacuum, his equation (2), he wrote:

> there is a non-flat region of space between two flat ones, that is, we have a plane-wave zone of finite extent in a non-singular metric satisfying Lichnerowicz's criteria [reference suppressed]. Consider now a set of test particles at rest in metric (2) before the arrival of the wave. (Bondi, 1957)

After the passage of the wave, there is relative motion.

> Clearly, this system of test particles in relative motion contains energy that could be used, for example, by letting them rub against a rigid friction disk carried by one of them. (Bondi, 1957)

This argument has carried the day with most people since that time: gravitational energy-transporting waves exist and do energetic things.

This argument has roots in Feynman (Anonymous, 2015) (DeWitt, 1957, p. 143) (Feynman *et al.*, 1995, xxv, xxvi) Kennefick (2007). John Preskill and Kip Thorne, drawing partly on unpublished sources, elaborate:

> At Chapel Hill, Feynman addressed this issue in a pragmatic way, describing how a gravitational wave antenna could in principle be designed that would absorb the energy "carried" by the wave [DeWi 57, Feyn 57]. In Lecture 16, he is clearly leading up to a description of a variant of this device, when the notes abruptly end: "We shall therefore show that they can indeed heat up a wall, so there is no question as to their energy content." A variant of Feynman's antenna was published by Bondi [Bond 57] shortly after Chapel Hill (ironically, as Bondi had once been skeptical about the reality of gravitational waves), but Feynman never published anything about it. The best surviving description of this work is in a letter to Victor Weisskopf completed in February, 1961 [Feyn 61]. (Feynman *et al.*, 1995, p. xxv)

Gravitational energy in waves exists in GR, and one of the main objections to localization can be managed by taking Noether's theorem seriously: there are infinitely many symmetries and energies. Another problem is the non-uniqueness of the pseudotensor, which one might address with either a best candidate (as in Joseph Katz's work) or a physical meaning for the diversity of them in relation to boundary conditions (James Nester

[1] I thank Carlo Rovelli for mentioning Bondi.

et al.). Even scalar fields have an analogous problem Callan *et al.* (1970). With hope there as well, energy in GR, although still in need of investigation, is not clearly a serious conceptual problem anymore. That is scientific progress à la Laudan.

13.5 Change in Hamiltonian General Relativity

Supposedly, change is missing in Hamiltonian General Relativity (Earman, 2002). That seems problematic for two reasons: change is evident in the world, and change is evident in Lagrangian GR in that most solutions of Einstein's equations lack a time-like Killing vector field (Ohanian and Ruffini, 1994, p. 352). A conceptual problem straddling the internal *vs.* external categories is "empirical incoherence", being self-undermining. According to Richard Healey,

> [t]here can be no reason whatever to accept any theory of gravity... which entails that there can be no observers, or that observers can have no experiences, some occurring later than others, or that there can be no change in the mental state of observers, or that observers cannot perform different acts at different times. It follows that there can be no reason to accept any theory of gravity... which entails that there is no time, or no change. (Healey, 2002, p. 300)

Hence accepting the no-change conclusion about Hamiltonian GR would undermine reasons to accept Hamiltonian GR. Change in the world is safe. But what about the surprising failure of Hamiltonian–Lagrangian equivalence?

A key issue involves where one looks for change, and relatedly, one what means by "observables". According to Earman (who would not dispute the point about the scarcity of solutions with time-like Killing vectors), "[n]o genuine physical magnitude countenanced in GTR changes over time" (Earman, 2002). Since the lack of time-like Killing vectors implies that the metric does change, clearly genuine physical magnitudes must be scarce, rarer than tensors. Tim Maudlin appeals to change in solutions to Einstein's equations: "stars collapse, perihelions precess, binary star systems radiate gravitational waves..." but "a sprinkling of the magic powder of the constrained Hamiltonian formalism has been employed to resurrect the decomposing flesh of McTaggart..." (Maudlin, 2002). Maudlin's appeal to common sense and Einstein's equations is helpful, as is Karel Kuchař's (Kuchař, 1993), but one needs more detail, motivation and (in light of Kuchař's disparate treatments of time and space) consistency.

Fortunately the physics reveals a relevant controversy, with reformers recovering Hamiltonian–Lagrangian equivalence (Castellani, 1982; Gràcia and Pons, 1988; Mukunda, 1980; Pons and Salisbury, 2005; Pons *et al.*, 1997; Pons and Shepley, 1998; Pons *et al.*, 2010; Sugano *et al.*, 1986). Hamiltonian–Lagrangian equivalence was manifest originally (Anderson and Bergmann, 1951; Rosenfeld, 1930; Salisbury, 2010); its loss needs study. In constrained Hamiltonian theories (Sundermeyer, 1982), some canonical momenta are (in simpler cases) just 0 due to independence of $\mathcal{L}$ from some $\dot{q}_i$; these are "primary constraints". In many cases of interest (including electromagnetism, Yang–Mills fields, and

General Relativity), some functions of p, q, $\partial_i p$, $\partial_i q$, $\partial_j \partial_i q$ are also 0 in order to preserve the primary constraints over time. Often these "secondary" (or higher) constraints are familiar, such as the phase space analog $\partial_i p^i = 0$ of Gauss's law $\nabla \cdot \vec{E} = 0$, Gauss–Codazzi equations embedding space into space-time in General Relativity, etc. Some constraints have something to do with gauge freedom (time-dependent redescriptions leaving the state or history alone). One takes Poisson brackets (q, p derivatives) of all constraints pairwise. If the result is in every case 0 (perhaps using the constraints themselves), then all constraints are "first-class", as in Clerk Maxwell's electromagnetism, Yang–Mills, and GR in their most common formulations. In General Relativity, the Hamiltonian, which determines time evolution, is nothing but a sum of first-class constraints (and boundary terms). Given that first-class constraints are related to gauge transformations, the key question is how they are related. Does each do so by itself, or do they rather work as a team? There is a widespread belief that each does so individually (Dirac, 1964). Then the Hamiltonian generates a sum of redescriptions leaving everything as it was, hence there is no real change. This is a classical aspect of the "problem of time." Some try to accept this conclusion, but recall Healey's critique.

Because Einstein's equations and common sense agree on real change, something must have gone wrong in Hamiltonian GR or the common interpretive glosses thereon, but what? Here the Lagrangian-equivalent reforming party has given most of the answer, namely, that what generates gauge transformations is not each first-class constraint separately, but the gauge generator G, a specially tuned sum of first-class constraints, secondary *and primary* (Anderson and Bergmann, 1951; Castellani, 1982; Pons, 2005; Pons *et al.*, 1997, 2010). Thus electromagnetism has two constraints at each point but only one arbitrary function; GR has eight constraints at each point but only four arbitrary functions. Indeed one can show that an isolated first-class constraint makes a mess (Pitts (2014b,a), such as spoiling the relation expected relation $\dot{q} = \frac{\delta H}{\delta p}$ making the canonical momentum equal to the electric field or the extrinsic curvature of space within space-time. These canonical momenta are auxiliary fields in the canonical action $\int dtd^3x(p\dot{q} - \mathcal{H})$, and hence get their physical meaning from $\dot{q}$. Because each first-class constraint makes a physical difference by itself (albeit a bad one), the GR Hamiltonian no longer is forced to generate a gauge transformation by being a sum of them. There is change in the Hamiltonian formalism whenever there is no time-like Killing vector, just as one would expect from Lagrangian equivalence.

> We have been guided by the principle that the Lagrangian and Hamiltonian formalisms should be equivalent … in coming to the conclusion that they in fact are. (Pons and Shepley, 1998, p. 17)

By the same token, separate first-class constraints do not change $p\dot{q} - \mathcal{H}$ by (at most) a total derivative, but *G does* (Pitts 2014a, 2014b).

To get changing observables in GR, one should recall the distinction between internal and external symmetries. Requiring that observables have 0 Poisson bracket with the electromagnetic gauge symmetry generator is just to say that things that we cannot observe (in the ordinary sense) are unobservable (in the technical sense). By contrast, requiring that observables have 0 Poisson bracket with the gauge generator in GR implies that the Lie derivative of an observable is 0 in every direction. Thus anything that varies spatiotemporally is "unobservable" – a result that cannot be taken seriously. The problem is generated by hastily generalizing the definition from internal to external symmetries. Instead one should permit observables to have Lie derivatives that are not 0 but just the Lie derivative of a geometric object – an infinitesimal Hamiltonian form of the identification of observables with geometric objects in the classical sense (Nijenhuis, 1952), *viz.*, set of components in each coordinate system and a transformation law.

13.6 Einstein's Real Λ Blunder in 1917

One tends to regard perturbative expansions and geometry as unrelated at best, if not negatively related.

> The advent of supergravity [footnote suppressed] made relativists and particle physicists meet. For many this was quite a new experience since very different languages were used in the two communities. Only Stanley Deser was part of both camps. The particle physicists had been brought up to consider perturbation series while relativists usually ignored such issues. They knew all about geometry instead, a subject particle physicists knew very little about. (Brink, 2006, p. 40)

But some examples will show how perturbative expansions can help to reveal the geometric content of a theory that is otherwise often misunderstood, can facilitate the conception of novel geometric objects that one might otherwise fail to conceive, and permit conceptual and ontological insight.

Perturbative expansions can help to reveal the geometric content of a theory that one might well miss otherwise. Einstein in his 1917 cosmological constant paper first reinvented a long-range modification of Newtonian gravity (Einstein, 1923) – one might call it (anachronistically) non-relativistic massive scalar gravity – previously proposed in the nineteenth century by Hugo von Seeliger and Carl Neumann. But he then made a false analogy to his new cosmological constant Λ, a mistake never detected till the 1940s (Heckmann, 1942), not widely discussed till the 1960s, and still committed at times today. According to Einstein, Λ was "completely analogous to the extension of the Poisson equation to $\Delta\phi - \lambda\phi = 4\pi K\rho$ " (Einstein, 1923). Engelbert Schücking, a former student of Heckmann, provided a firm evaluation. "This remark was the opening line in a bizarre comedy of errors" (Schucking, 1991). The problem is that Λ is predominantly 0th order in ϕ (having a leading constant term), whereas the modified Poisson is 1st order in ϕ. Λ gives a weird quadratic potential for a point source, but the modified Poisson equation gives a massive graviton with plausible Neumann–Yukawa exponential fall-off (Freund *et al.*, 1969; Schucking, 1991). "However generations of physicists have parroted this nonsense" (Schucking, 1991). Massive theories of gravity generically involve two metrics, whereas Λ involves only one. Understanding geometric content sometimes is facilitated by a perturbative expansion.

13.7 Series, Non-linear Geometric Objects, and Atlases

Perturbative series expansions can also be useful for conceptual innovations. For example, non-linear realizations of the "group" of arbitrary coordinate transformations have tended to be invented with the help of a binomial series expansion for taking the symmetric square root of the metric tensor (DeWitt and DeWitt, 1952; Ogievetskiĭ and Polubarinov, 1965; Ogievetsky and Polubarinov, 1965). The exponentiating technology of non-linear group realizations (Isham *et al.*, 1971) is also at least implicitly perturbative. While classical differential geometers defined non-linear geometric objects (basically the same as particle physicists' non-linear group realizations as applied to coordinate transformations) (Aczél and Gołab, 1960; Szybiak, 1966; Tashiro, 1952), they generally provided no examples.

Perhaps the most interesting example involves the square root of the (inverse) metric tensor, or rather a slight generalization for indefinite metrics. The result is strictly a square root and strictly symmetric using $x^4 = ict$; otherwise it is a generalized square root using the signature matrix $\eta_{\alpha\beta} = diag(-1, 1, 1, 1)$. One has $r^{\mu\alpha}\eta_{\alpha\beta}r^{\beta\nu} = g^{\mu\nu}$ and $r^{[\mu\nu]} = 0$. Under coordinate transformations, the new components $r^{\mu\nu\prime}$ are non-linear in the old ones (Ogievetsky and Polubarinov, 1965; Pitts, 2012). These entities augment tensor calculus and have covariant and Lie derivatives (Szybiak, 1963; Tashiro, 1952).

Defining the symmetric square root of a metric tensor might seem more of a curiosity for geometric completists than an important insight – but the symmetric square root of the metric makes an important conceptual difference with spinor fields used to represent fermions. Spinors in GR are widely believed to require an orthonormal basis (Cartan and Mercier, 1966; Lawson and Michelsohn, 1989; Weyl, 1929). But they do not, using $r^{\mu\nu}$ (Bilyalov, 2002; DeWitt and DeWitt, 1952; Ogievetskiĭ and Polubarinov, 1965; Ogievetsky and Polubarinov, 1965). One can have spinors in coordinates, but with metric-dependent transformations beyond 15-parameter conformal group (Borisov and Ogievetskii, 1974; Isham *et al.*, 1971; Ogievetskiĭ and Polubarinov, 1965; Pitts, 2012), the conformal Killing vectors for the unimodular metric density $\hat{g}_{\mu\nu} = (-g)^{-\frac{1}{4}} g_{\mu\nu}$. Such spinors have Lie derivatives beyond conformal Killing vectors – often considered the frontier for Lie differentiation of spinors (Penrose and Rindler, 1986, p. 101) – but they sprout new terms in $\pounds_\xi \hat{g}_{\mu\nu}$. One can treat symmetries without surplus structure and an extra local $O(1, 3)$ gauge group to gauge it away.

The (signature-generalized) square root of a metric, although not very familiar, fits fairly nicely into the realm of non-linear geometric objects, yielding a set of components in every coordinate system (with a qualification) and a non-linear transformation law. The entity is useful especially if one wants to know what sort of space-time structure is necessary for having spin-$\frac{1}{2}$ particles in curved space-time (Woodard, 1984). Must one introduce an orthonormal basis, then discard much of it from physical reality by taking an equivalence class under local Lorentz transformations? Or can one get by without introducing anything beyond the metric and then throwing (most of?) it away?

A curious and little known feature of this generalized square root touches on an assumption usually made in passing in differential geometry. Although one can (often) make a binomial series expansion in powers of the deviation of the metric from the signature matrix, and (more often) one can take a square root using generalized eigenvalues, there are exotic coordinate systems in which the generalized square root does not exist due to the indefinite signature (Bilyalov, 2002; Deffayet *et al.*, 2013; Pitts, 2012). This fact is trivial to show in two space-time dimensions (signature matrix $diag(-1, 1)$) using the quadratic formula: just look for negative eigenvalues. The fact generally has not been noticed previously because most treatments (a great many are cited in Pitts, 2012) worked near the identity. Such a point could have been noticed some time ago by Hoek, but a fateful innocent inequality was imposed that restricted the coordinates (with signature $+---$).

> We shall assume that [the metric tensor $g_{\mu\nu}$] is pointwise continuously connected with the Minkowski metric (in the space of four-metrics of Minkowski signature) and has $g_{00} > 0$. (Hoek, 1982)

The lesson to learn is that there can be feedback from the fibers over space-time to the atlas of admissible coordinate systems for non-linear geometric objects given an indefinite signature. Naively assuming a maximal atlas causes interesting and quite robust entities not to exist. Such a result sounds rather dramatic when expressed in modern vocabulary. But coordinate inequalities are old (Hilbert, 2007), familiar (Møller, 1972), and not very dramatic classically; coordinates can have qualitative physical meaning while lacking a quantitative one. A principal square root is related to the avoidance of negative eigenvalues of $g^{\mu\nu}\eta_{\nu\rho}$ (Higham, 1987, 1997). Null coordinates are fine; the coordinate restriction is mild. Amusingly, coordinate *order* can be important: if (x, t, y, z) is bad, switching to (t, x, y, z) suffices (Bilyalov, 2002).

13.8 Massive Gravity: 1965–72 Discovery of 2010 Pure Spin-2

The recent (re)invention of pure spin-2 massive gravity (de Rham *et al.*, 2011; Hassan and Rosen, 2011) used the symmetric square root of the metric, as did the first invention (Ogievetsky and Polubarinov, 1965), though not the second (Pitts, 2011; Zumino, 1970). This problem has a curious history, from which Ogievetsky and Polubarinov (1965) have been unjustly neglected. That paper highly developed the symmetric square root of the metric perturbatively. It derived a two-parameter family of massive gravities, which, I note, includes two of the original three modern massive pure spin-2 gravities with a flat background metric. In light of the dependence of the space-time metric on the lapse function N in a 3 + 1 ADM split, there were only two Ogievetsky–Polubarinov theories with any chance of being linear in the lapse (hence having pure spin-2 (Boulware and Deser, 1972)), although the naive cross-terms are rather discouraging. These are the $n = \frac{1}{2}, p = -2$ theory built around $\delta^{\alpha}_{\mu}(g^{\mu\nu}\eta_{\nu\alpha}\sqrt{-g}^{2})^{\frac{1}{2}}$, a theory reinvented as equation (3.4) of Hassan and Rosen (2011), and the $n = -\frac{1}{2}, p = 0$ theory built around $\delta^{\mu}_{\alpha}(g_{\mu\nu}\eta^{\nu\alpha}\sqrt{-g}^{0})^{\frac{1}{2}}$. A truly novel third theory is now known (Hassan and Rosen, 2011). A second novel modern result is the non-linear field redefinition of the shift vector (Hassan *et al.*, 2012), which allows the square root of the metric to be linear in the lapse.

More striking than the proposal of such theories long ago is the fact that in 1971–1972 Maheshwari already showed that one of the Ogievetsky–Polubarinov theories had pure spin-2 *non-linearly* (Maheshwari, 1972)! Thus the Boulware–Deser–Tyutin–Fradkin ghost (Boulware and Deser, 1972; Tyutin and Fradkin, 1972) (the negative energy sixth degree of freedom that is avoided by Fierz and Pauli to linear order but comes to life non-linearly) was *avoided before it was announced*. Unfortunately Maheshwari's paper made no impact, being cited only by Maheshwari in the mid-1980s. With Vainshtein's mechanism also suggested in 1972 (Vainshtein, 1972), there was no seemingly insoluble problem for massive spin-2 gravity in the literature. Massive spin-2 gravity was largely ignored from 1972 until *c*. 2000 largely because of failure to read Maheshwari's paper. This example illustrates the point (Chang, 2012) that the history of a science has resources for current science.

13.9 Conclusions

The considerations above support the idea that progress in knowledge about gravity can be made by overcoming various barriers, whether between general relativity and particle physics, or between physics and the history and philosophy of science. GR does not need to be treated *a priori* as exceptional, either in justifying choosing GR over rivals or in interpreting it. GR is well motivated non-mysteriously using particle physicists' arguments about the exclusion of negative-energy degrees of freedom, arguments that leave only a few options possible. To some degree the same holds even for the context of discovery of GR, given the renewed appreciation of Einstein's "physical strategy".

Because conceptual problems of GR often can be resolved, there is no need to treat it as *a priori* exceptional in matters of interpretation, either. Regarding gravitational radiation, Feynman reflected on the unhelpfulness of GR-exceptionalism:

> What is the power radiated by such a wave? There are a great many people who worry needlessly at this question, because of a perennial prejudice that gravitation is somehow mysterious and different—they feel that it might be that gravity waves carry no energy at all. We can definitely show that they can indeed heat up a wall, so there is no question as to their energy content. (Feynman *et al.*, 1995, pp. 219, 220)

The conservation of energy and momentum – rather, energies and momenta – makes sense in relation to Noether's theorems. Change, even in local observables, is evident in the Hamiltonian formulation, just as in the Lagrangian/four-dimensional geometric form.

To say that GR should not be treated as *a priori* exceptional is not to endorse the strongest readings of the claim that GR is just another field theory, taking gauge-fixing and perturbative expansions as opening moves. The mathematics of GR logically entails some distinctiveness, such as the difference between external coordinate symmetries (with a transport term involving the derivative of the field) and internal symmetries as in electromagnetism and Yang–Mills. Identifying such distinctiveness requires reflecting on the mathematics and its meaning, as well as gross features of embodied experience, but it does not require conjectures about the trajectory of historical progress or divination of the spirit of GR.

Series expansions have their uses in GR. Einstein's failure to think perturbatively in 1917 about the cosmological constant generated lasting confusion and surely helped to obscure massive spin-2 gravity as an option. Many of the (re)inventions of the symmetric generalized square root of the metric began perturbatively. It permits spinors in coordinates, a fundamental geometric result, just as was Weyl's (1929) impossibility claim. Perturbative methods should not always be used or always avoided; they are one tool in the tool box for the foundations of gravity and space-time.

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