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Einstein's physical strategy, energy conservation, symmetries, and stability: "But Grossmann & I believed that the conservation laws were not satisfied"

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ABSTRACT

Recent work on the history of General Relativity by Renn et al. shows that Einstein found his field equations partly by a physical strategy including the Newtonian limit, the electromagnetic analogy, and energy conservation. Such themes are similar to those later used by particle physicists. How do Einstein's physical strategy and the particle physics derivations compare? What energy–momentum complex(es) did he use and why? Did Einstein tie conservation to symmetries, and if so, to which? How did his work relate to emerging knowledge (1911–1914) of the canonical energy–momentum tensor and its translation-induced conservation? After initially using energy–momentum tensors hand-crafted from the gravitational field equations, Einstein used an identity from his assumed linear coordinate covariance $x^{\mu'} = M^{\mu}_{\nu} x^{\nu}$ to relate it to the canonical tensor. Usually he avoided using matter Euler–Lagrange equations and so was not well positioned to use or reinvent the Herglotz–Mie–Born understanding that the canonical tensor was conserved due to translation symmetries, a result with roots in Lagrange, Hamilton and Jacobi. Whereas Mie and Born were concerned about the canonical tensor's asymmetry, Einstein did not need to worry because his *Entwurf* Lagrangian is modeled not so much on Maxwell's theory (which avoids negative-energies but gets an asymmetric canonical tensor as a result) as on a scalar theory (the Newtonian limit). Einstein's theory thus has a symmetric canonical energy–momentum tensor. But as a result, it also has 3 negative-energy field degrees of freedom (later called "ghosts" in particle physics). Thus the *Entwurf* theory fails a 1920s–1930s *a priori* particle physics stability test with antecedents in Lagrange's and Dirichlet's stability work; one might anticipate possible gravitational instability.

This critique of the *Entwurf* theory can be compared with Einstein's 1915 critique of his *Entwurf* theory for not admitting rotating coordinates and not getting Mercury's perihelion right. One can live with absolute rotation but cannot live with instability.

Particle physics also can be useful in the historiography of gravity and space–time, both in assessing the growth of objective knowledge and in suggesting novel lines of inquiry to see whether and how Einstein faced the substantially mathematical issues later encountered in particle physics. This topic can be a useful case study in the history of science on recently reconsidered questions of presentism, whiggism and the like. Future work will show how the history of General Relativity, especially Noether's work, sheds light on particle physics.

1. Introduction

> …but Grossmann & I believed that the conservation laws were not satisfied and Newton's law did not result in first-order approximation. (Einstein, 1996b,p. 160)

So wrote Einstein to Besso on December 10, 1915, explaining why the newly accepted 'right' field equations had previously been rejected. This first concern of Einstein and Grossmann, that the conservation laws were not satisfied, is actually rather mysterious, as will appear shortly.

General Relativity is traditionally credited to Einstein's mathematical strategy of Principles (equivalence, generalized relativity, general covariance, eventually Mach) in the contexts of discovery

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and justification. According to this common (often implicit) view, not only did Einstein discover his late 1915 field equations (note the success term "discover") using these principles, but also we today should believe his theory on the same grounds. At any rate we today should have a reasonably high prior degree of belief in it on those grounds, and a low prior for theories violating those principles, so that empirical confirmation that has arisen since the mid-1910s can leave us rationally with a high degree of belief in his theory. In an important sense the physics of space–time and gravity "arrived" in 1915–1919, when the final field equations were found and striking empirical confirmation arrived in the form of the bending of light. Since that time little important has happened relevant to space–time philosophy, so understanding the 1910s makes one up to date. This story (or perhaps caricature), though widely circulated partly due to Einstein's media penetration (reinforced on everything from T-shirts to the American Physical Society's robot-repellent process of selecting Einstein's face in order to read *Physical Review* journals), can be challenged on both descriptive historical and normative philosophical grounds.

In fact a number of leading historians of General Relativity have argued recently that Einstein found his field equations by a quite different route. Indeed Einstein's "physical strategy" (it has been called, to contrast it with the more familiar "mathematical strategy" of Principles) has been recovered by historians by a combination of meticulous study of his notebooks during the early-mid 1910s and taking seriously the published record of his work in that era. Einstein's physical strategy involves seeking relativistic gravitational field equations using the Newtonian limit, an analogy between gravitation and electromagnetism (the most developed relativistic field theory at the time), the coupling of all energy–momentum *including gravity's* as a source for gravity, and energy–momentum conservation (Brading, 2005; Janssen, 2005; Janssen & Renn, 2007; Norton, 1989; Renn, 2005a, 2005b; Renn & Sauer, 1999, 2007). Thus the descriptive historical claim of the role of the mathematical strategy of principles in Einstein's process of discovery has been significantly exaggerated. Indeed at least one reason why has been offered: Einstein re-wrote his own history in order to justify his decreasingly appreciated unified field theory quest (van Dongen, 2010). Einstein might well also simply have come to believe that his mathematical strategy made the crucial difference in 1915.

Normative issues also arise regarding the relative strengths of the physical and mathematical strategies as arguments. On the one hand, the mathematical strategy of Principles is sometimes seen as not compelling (Norton, 1995; Stachel, 1995). Thus one might look for something more compelling, such as eliminative inductions, Norton argued. On the other hand, Einstein's physical strategy also has its puzzles. Just how Einstein arrived at (what we take to be) his definitive field equations is tortuous and paradoxical. Renn and Sauer find

> what might be called the three epistemic paradoxes raised by the genesis of general relativity:
>
> *The paradox of missing knowledge*….
>
> *The paradox of deceitful heuristics*…
>
> *The paradox of discontinuous progress*. How could general relativity with its nonclassical consequences—such as the dependence of space and time on physical interactions—be the outcome of classical and special-relativistic physics although such features are incompatible with their conceptual frameworks? (Renn & Sauer, 2007, pp. 118, 119)

If the physical strategy and the mathematical strategy both have their limitations as arguments, where does that leave us?

I suggest that we should not be afraid to find both strategies' lines of argument quite imperfect during the 1910s. A great deal has been learned in the last 100 years, some of it empirical, some of it theoretical, which makes our epistemic situation very different from Einstein's in late 1915. It also isn't clear that the most reasonable views to hold in 1915 were Einstein's. Nordström's (second) theory, for example, was simpler and hence arguably more plausible *a priori*, before or after the Einstein–Fokker geometrization (Einstein & Fokker, 1914). While Einstein's late 1915 theory handled Mercury better than Nordström's theory did, why must the evidence from Mercury more than offset the greater antecedent plausibility of Nordström's theory by the standards of the day (von Laue, 1917)? As von Laue put it,

> This agreement between two individual numbers [the perihelion prediction of Einstein and the Newcomb anomaly], achieved under conditions which cannot be arbitrarily altered, so that it seems uncertain whether the suppositions (specifically the assumption of two mass points) are fulfilled with sufficient accuracy, does not seem to be a sufficient reason, even though it is note-worthy, to change the whole physical conception of the world to the full extent as Einstein did in his theory. Roseveare (1982, p. 182).[1]

In 1912–1915 it wasn't so clear that a revised gravitational theory was needed anyway. The matter-based zodiacal light hypothesis (or something close enough) had seemed to significant people (Newcomb's last view, expressed posthumously in 1912, Newcomb, 1912, pp. 226, 227, and Seeliger) to be doing well enough in addressing Mercury's problem (Roseveare, 1982, pp. 69, 156). Mercury's perihelion was not the only empirical difficulty for astronomy anyway (Roseveare, 1982, p. 86), so a gravitational solution for just that problem was not especially plausible in advance. Zenker makes an interesting effort to relate this episode to formal philosophy of science (Zenker, 2009).

As Lakatos emphasized, if we want to speak of progress rather than mere change, we need normative standards (Lakatos, 1971). If we distinguish our epistemic situation in 2015 from that of 1915 as we should, we both can and must find something(s) in the last 100 years to fill in the gap that 1915 cannot fill for us, in order to identify the rational progress made in space-time and gravitational theory. Chang has emphasized the value of attending to contemporary knowledge in writing the history of science and integrating the history and philosophy of science (Chang, 2009, 2012).

Fortunately there is at least one way to do that, a way largely unexplored, namely, using later particle physics arguments (Feynman et al., 1995; Kraichnan, 1955)—which bring us to the normative challenge to the use of Einstein's principles. Much of the reason that Einstein's work has generated a scholarly industry (as only a few scholars do), presumably, is the belief that Einstein's reasons for General Relativity, whatever they were, are also good reasons for us, indeed among the best that we have. More broadly, General Relativity is one of the most impressive scientific theories, and it is at least plausible that something methodologically interesting can be learned from the process by which it arose. But maybe our reasons are quite different from and even much better than Einstein's reasons. Particle physicists know that Einstein's equations are what one naturally arrives for a local interacting

[1] Diese Übereinstimmung zwischen zwei einzelnen Zahlen, erhalten unter Bedingungen, an denen jede willkürliche Veränderung unmöglich ist, bei der uns selbst unsicher scheint, ob die Voraussetzungen (wir denken an die Annahme zweier Massenpunkte) mit ausreichender Genauigkeit erfüllt ist, scheint uns, so bemerkenswert sie ist, doch kein hinreichender Grund, das gesamte physikalische Weltbild von Grund aus zu ändern, wie es die Einsteinsche Theorie tut. (von Laue, 1917, p. 269).