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Jeroen van Dongen
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Introduction

"This exposition has fulfilled its purpose when it shows the reader how a life's efforts are related to one another and why they have led to expectations of a particular kind."[1] Thus end Albert Einstein's "Autobiographical notes," as we begin our account. Einstein's critical self-assessment was published in a collection of essays entitled *Albert Einstein*: *Philosopher-Scientist*, and it appeared in 1949, only six years before Einstein's final passage. These "Notes" constitute a rich source of personal reflections on his life and science; in particular, they discuss in detail the ideas that dominated his later years. The latter are the subject of this book, and it is with the above sense of purpose that we wish to approach it.

Thus, this study will be about Einstein's search for a unified theory for all physical forces, and its relation to his larger oeuvre in science and philosophy. Our story starts with the events leading up to the 1915 discovery of general relativity and its scope extends to Einstein's passing away in 1955. We have a vast subject, and it will be impossible to address minutely all its elements; both some familiar and some less familiar themes of Einstein's later physics will have to remain undiscussed. What we will present, rather, is an attempt, from a historical perspective, at a synthesis, and our selection will be aimed to serve that synthesis.

When leaving out of consideration the elaborations of general relativity, one can characterize Einstein's later work according to roughly two strands: on the one hand there are his numerous publications on classical unified field theories, and on the other hand we find his critique of quantum theory. The secondary literature on the latter subject is quite voluminous. Yet, the larger part of this literature only superficially engages with Einstein's efforts in unified field theory, despite the fact that

[1] "Diese Darlegung hat ihren Zweck erfüllt, wenn sie dem Leser zeig[t], wie die Bemühungen eines Lebens miteinander zusammenhängen und warum sie zu Erwartungen bestimmter Art geführt haben." As in Einstein (1949a), p. 94.

he published about twice as many papers on this topic as on quantum mechanics.[2] This is a rather unfortunate oversight, in particular when one realizes that these efforts were intended to yield an alternative for the quantum theory – that they did not bear any fruit can hardly justify the lacuna. One of our aims will be to see how and why Einstein chose to pursue unified field theory on the one hand, and on the other hand how this related to his dissenting attitude to quantum theory. Thus we hope to arrive at a coherent perspective on Einstein's later efforts.

A central observation in our discussion will be that Einstein's increasing engagement with unified field theory and his distance from the quantum program developed in tandem with a changing recollection of his route to the general theory of relativity. From the perspective of the older Einstein, it seemed as if the successful formulation of the latter theory had been the result of seeking the mathematically most natural formulation for a generally relativistic gravity theory. This recollection furthermore became a point of reference, even of justification, for his later work. These developments were well reflected in Einstein's changing views on the method of science, just as these views influenced as well as mirrored his contemporary practice in theory construction. The following excerpt from a letter that Einstein sent to Louis de Broglie in 1954 (just when David Bohm sought to revive de Broglie's ideas about hidden variables) illustrates our perspective rather well:

> That I am writing you has a peculiar reason. Namely, I would like to tell you *how* I have come to my method, which from the outside must seem quite bizarre: I must look like an ostrich that keeps his head buried in relativistic sand so that he does not have to look the evil quanta in the eye. In reality I am, just like you, convinced that one should look for a substructure, the necessity of which has been cleverly disguised by the current quantum theory through its use of the statistical mold.
>
> I have however long been convinced that one shall not be able to find this substructure *in a constructive way* from the known empirical relations between physical things, because the required mental leap would exceed human powers. I have arrived at this opinion not only because of the fruitlessness of the efforts of many years, but rather also through the experiences with the gravitation theory. The gravitational equations could *only* be found by a purely formal principle (general covariance), that is, by trusting in the largest imaginable logical simplicity of the natural laws. As it was obvious that the theory of gravity constitutes only a first step in finding the simplest possible general field laws, it seemed to me that this logical route should first be thought through to the end before one can hope to arrive also at

[2] An insightful exception to the above mentioned literature is Abraham Pais's biography (1982), see in particular p. 328, pp. 460–469.

a solution of the quantum problem. This is how I became a fanatic believer in the method of "logical simplicity." [...] This should explain the ostrich policy.[3]

Einstein acknowledged that, in the eyes of his contemporaries, he must have looked like an ostrich in his obstinate attitude to quantum theory. But he felt that his actual position was more subtle: he had not buried his head in the sand, but was rather trying to look beyond the statistical quantum theory. In this attempt, he cited as his foremost guiding principle a maxim of "logical simplicity" – we will see that we can just as well read here "mathematical naturalness." Both were intimately related to his quest for unification, and to understand better the motivations of this striving, as the letter to de Broglie indicates, we have to involve his struggles to find the general theory of relativity.

After the formulation of general relativity Einstein began playing down the importance of induction from the phenomena. He then added to this an emphasis on the creative merit of aiming for mathematical simplicity and naturalness. When these epistemological ideas had settled, they found their clearest public expression in his 1933 Herbert Spencer lecture at the University of Oxford, "On the method of theoretical physics." The contents of that lecture will be an important element in our story. Its ideas were later, in 1952, concisely captured in a schema that Einstein drew in one of his letters to his friend Maurice Solovine, and this schema will prove to be quite practical in describing Einstein's intellectual development.

When writing to de Broglie, Einstein indeed stated that he believed in a particular "method" while he was engaged in his latest efforts. In the second half of his professional career, his view "on the method of theoretical physics" became the premier vantage point from which he would assess his involvement with his discipline, and we will pick up on these references. In recent historiography of science, however, methodological perspectives have regularly been discounted as indicative of an outdated approach to history; in this outdated approach, history was supposedly regarded as the hand-maiden of philosophy, and barely rose above presenting a

[3] "Dass ich Ihnen nun schreibe, hat eine eigentümliche Ursache. Ich will Ihnen nämlich sagen, *wie* ich zu meiner Methodik getrieben worden bin, die von aussen gesehen recht bizarr ist. Ich muss nämlich erscheinen wie der Wüsten-Vogel Strauss, der seinen Kopf dauernd in dem relativistischen Sand verbirgt, damit er den bösen Quanten nicht ins Auge sehen muss. In Wahrheit bin ich genau wie Sie davon überzeugt, dass man nach einer Substruktur suchen muss, welche Notwendigkeit die jetzige Quantentheorie durch Anwendung der statistischen Form kunstvoll verbirgt. Ich bin aber schon lange der Überzeugung, dass man diese Substruktur nicht *auf konstruktivem Wege* aus dem bekannten empirischen Verhalten der physikalischen Dinge wird finden können, weil der nötige Gedankensprung zu gross wäre für die menschlichen Kräfte. Zu dieser Meinung kam ich nicht nur durch die Vergeblichkeit vieljähriger Bemühungen, sondern auch durch die Erfahrungen bei der Gravitationstheorie. Die Gravitationsgleichungen waren *nur* auffindbar auf Grund eines rein formalen Prinzips (allgemeine Kovarianz), d.h. auf Grund des Vertrauens auf die denkbar grösste logische Einfachheit der Naturgesetze. Da es klar war, dass die Gravitationstheorie nur einen ersten Schritt zur Auffindung möglichst einfacher allgemeiner Feldgesetze darstellt, schien es mir, dass dieser logische Weg erst zu Ende gedacht werden muss, bevor man hoffen kann zu einer Lösung auch des Quantenproblems zu gelangen. So wurde ich zu einem fanatischen Gläubigen der Methode der „logischen Einfachheit". [...] Dies zur Erklärung der Vogel-Strauss-Politik." Einstein to Louis de Broglie, 8 February 1954, EA (i.e., Albert Einstein Archives, Jerusalem) 8-311. Emphasis as in original.

cumulative account that suggested an inevitable progress in knowledge, effected by contributions of great geniuses. As philosophy would try to capture the "right" method, reflective of a timeless human *ratio*, history's task was foremost to provide case studies that either illustrated or disproved.[4]

This approach has been replaced by a historiography that regards physics, and other scientific disciplines, as part of the larger culture, and rightfully so – even if its excesses did for a while turn history of science into the battleground of an unfortunate culture war that pitted science against the humanities. The new perspective has given the opportunity to answer a multitude of insightful questions, some of which, in the old viewpoint, could hardly even have been raised. Thus, for example, we now have insight into how physics as an academic discipline – essentially a cultural niche – was formed out of a variety of scientific groups and practices. Yet, to discount wholly the issue of method has overshot the mark. In the particular case of Einstein, a discussion of methodology might in fact be unavoidable, as he expressed himself with regular reference to his "method," not unlike other physicists of his generation. As we will argue, his methodology furthermore developed in interaction with both his current and past work in physics; understanding Einstein as a practicing scientist therefore inevitably leads one to consider his methodological stances.

Our discussion of methodology should be viewed in that light: we do not aspire to capture "Einstein's method" in order subsequently to chastise its failures or instruct a chase after its successes. Rather, we intend to approach methodology as an inalienable, active element of Einstein's engagement with his science, just as it was a reflection thereof. After all, it was Einstein himself who, particularly in his later years, deliberated on his practice in theoretical physics at a methodological meta-level. This circumstance should be of benefit to historians as it gives crucial insight into how he expected theoretical physics ought to describe and explain the natural phenomena – how theoretical physics should and should not be done. The methodological perspective is thus a valuable instrument when trying to put Einstein's historical contingency in a sharper light.

From the perspective of the Spencer lecture "on the method," we will first look back on the formulation of general relativity, with a particular focus on the heuristic strategies that Einstein employed. The frustrations and successes of these strategies shed light on the gradual epistemological reorientation that Einstein subsequently underwent. The ideas of the Spencer lecture were to Einstein a personal reflection on his own work, rather than a chiselled charter for theoretical research. We will see, however, that he also engaged his methodological ideas, and the discovery of general relativity, to find his direction in his later efforts in unified field theory. The

[4] For this characterization of the outdated approach, see for example Morus (2005), pp. 4–5.

subject in which we will study this interaction in particular is his research on the "semivector," the topic he was pursuing at the time of his lecture in Oxford.

To come to grips with Einstein's ideal for a physical theory, we give an outline of his encounter with the Kaluza–Klein theory. As for any of the unified field theories that he studied, his hope was that this theory could somehow undercut the quantum theory. This hope was partly motivated by his ideas on how a physical theory should give explanations of the phenomena – how a theory could yield understanding of nature. As suggested, these ideas were closely related to his methodological convictions.

In the second paragraph of the letter to de Broglie quoted above, Einstein criticized in an implicit way the heuristics of the quantum theorists. These were, much more than his own, motivated by an empirically oriented philosophy – that was in fact rather akin to the philosophical convictions he had held in his younger years. Such a methodology was to the later Einstein somewhat naive, and could only lead to a superficial description of nature.[5]

We will see that the difference in the practice of theory construction between Einstein and the quantum theorists was an important cause for his negative appraisal of quantum mechanics. This aspect complements in a natural way the objections that he addressed at the foundations of the theory; for example issues regarding completeness, determinism, etc.; the classical field theories under his scrutiny would furthermore presumably not exhibit these flaws. To Einstein, the foundational shortcomings of quantum theory were intimately linked to a conviction that experientially oriented methods could at best produce a mere phenomenological theory. Thus, as in the letter to de Broglie, Einstein pointed in particular to his alternative methodology to justify his rejection of quantum mechanics, and his pursuit of another theory.

[5] Einstein wrote to the mathematical physicist André Lichnerowicz, again in 1954: "If [...] the physical world cannot be reduced to logically simple elements, then there is no hope at all for us to understand things other than superficially," and continued by suggesting that both physicists and philosophers were currently too much under the sway of such superficial approaches. ("Wenn [...] die physikalische Welt nicht auf logisch einfache Elemente zurückführbar ist, dann gibt es für uns überhaupt keine Hoffnung, die Dinge anders als oberflächlich zu erfassen." Einstein to Lichnerowicz, January 1954, EA 16-319; for similar remarks, see also for example Einstein to Erwin Schrödinger, 22 December 1950, p. 36 in Einstein *et al.* (1963).)

1

Formulating the gravitational field equations

On 25 November 1915, Albert Einstein presented the final version of the field equations of the general theory of relativity to the Royal Prussian Academy of Sciences.[1] These equations were generally covariant: their form remained unchanged under arbitrary transformations of the space and time coordinates. This was a mathematical manifestation of Einstein's principle of equivalence, which held that the state of affairs in a homogeneous gravitational field is identical to the state of affairs in a uniformly accelerated coordinate system.

Einstein's first publication that contained the principle of equivalence appeared in 1907. It was included in a review paper of his relativistic account of electrodynamics of 1905. The principle immediately proved its heuristic value: on its basis Einstein already proposed in the same article the existence of a gravitational redshift of light, and the bending of light trajectories in a gravitational field.[2] Nevertheless, eight years would pass between the first formulation of the equivalence principle and its final vindication in 1915, when it acquired a firm footing in the field equations. During those years, Einstein remained nearly silent on gravitation from late 1907 until June 1911. He did not publish any substantial articles on the subject and, even more surprising, he hardly discussed it with his correspondents.

There are, however, strong indications that Einstein continued to think about the problems of gravity, as the equivalence principle and accelerated motions led to conceptual difficulties in the special theory of relativity. A discussion involving such luminaries as Max Laue and the mathematician Gustav Herglotz raged on the conflicts that rotation produced for straightforward definitions of relativistic rigid bodies. A rotating cylinder should have a Lorentz-contracting circumference, as seen from a reference system at rest, yet its radius – perpendicular to

[1] See Einstein (1915d).
[2] See Einstein (1907b); the above formulation of the principle of equivalence derives from Einstein's formulation on its p. 454; on Einstein's 1907 paper see also Miller (1992), Stachel (2007). For a historical discussion of the equivalence principle, see Norton (1985).

the rotation – would remain unaltered. Could one in this case still maintain the usual relation between radius and circumference from Euclidean geometry? The problem was known as the "Ehrenfest paradox," after Paul Ehrenfest, who had posed it in a concise one page paper in 1909.[3]

Einstein's silence ended in 1911, when he published an article that further elaborated the idea of gravitational light deflection and that included a first value for this effect in the case of light grazing the surface of the sun. Soon, Einstein set out to find field equations for a relativistic theory of gravity; first for the case of a static field, and then for the full dynamical case, spurred on by competing publications by Max Abraham and Gunnar Nordström.[4]

In the 1911 publication Einstein argued that the speed of light c should depend on the gravitational potential and thus on the spatial coordinates.[5] In his subsequent theory for the static field of 1912, the speed of light started playing the role of the potential itself. Einstein found that in a static field "local" time τ was to be related to "universal" time[6] t through $\mathrm{d}\tau = c\mathrm{d}t$. He pointed out that the equations of motion of a particle could easily be expressed in the Lagrangian formalism, in which they would take the form:

$$\delta \int \sqrt{c^2\mathrm{d}t^2 - \mathrm{d}x^2 - \mathrm{d}y^2 - \mathrm{d}z^2} = 0. \tag{1.1}$$

Einstein observed that these equations "[let] us anticipate the structure of the equations of motion of a particle in a dynamical gravitational field."[7] Changing the coordinates would transform the single static gravitational potential c into ten functions $g_{\mu\nu}$, so Einstein may at this point have begun to surmise that the equation of motion for a particle coincides with the geodesic equation for non-flat spaces.[8]

The above equation suggests a departure from Euclidean geometry. This had already been suggested earlier by the rotating disk of the Ehrenfest paradox. Rotation involves inertial forces, and these can be seen as gravitational forces according to the equivalence principle. Thus, if in the case of a rotating disk one runs into trouble with Euclidean geometry, one could expect the same for the equivalent gravitational field.

[3] See Ehrenfest (1909); for historical discussion, see Stachel (1989a; 2007), Sauer (2007b).

[4] For the light reflection result, see Einstein (1911); Einstein's 1911 prediction was off by a factor of 2 when compared to the later theoretical and observed value; for a discussion, see Earman and Glymour (1980). Einstein's static theory is found in Einstein (1912a,b); for Abraham's and Nordström's theories, see Abraham (1912a), Nordström (1912), reprinted in English translation in Renn and Schemmel (2007a), pp. 331–339, 489–497.

[5] Einstein (1911), p. 906.

[6] Einstein (1912a), p. 366. In modern parlance, $\mathrm{d}\tau$ would be a proper time increment and $\mathrm{d}t$ a time increment in coordinate time, in units in which a constant light velocity reduces to unity.

[7] "Die zuletzt hingeschriebene Hamiltonsche Gleiching lässt ahnen, wie die Bewegungsgleichungen des materiellen Punktes im dynamischen Gravitationsfelde gebaut sind," (Einstein 1912b), p. 458.

[8] See Stachel (2007), pp. 95–103.

In the summer of 1912, Einstein's former fellow student and friend Marcel Grossmann, now professor of mathematics at their alma mater, the *Eidgenössische Technische Hochschule* in Zurich, indicated that Einstein should take up the Riemannian theory of geometry and the relatively new mathematical theory of tensors.[9] Einstein wanted to find appropriate field equations for the $g_{\mu\nu}$-potentials. He soon realized, on the basis of elaborations of four-dimensional relativistic electrodynamics and hydrodynamics, that the ten-component stress-energy tensor was the right generalization of the Newtonian mass density in such field equations.[10] The central question then was, what term should the left-hand side of the gravitational field equations contain?

Recent scholarship – based on extensive studies of Einstein's actual research notes – has shown that Einstein was working along a two-pronged strategy as he tried to formulate candidate field equations.[11] On the one hand, he abstracted from a number of physical constraints. On the other hand, he looked for the laws that were most naturally written in the new tensor formalism. Einstein expected that both approaches would lead to the same results, but in 1913 it seemed to him that they would not. He had to choose, and he chose the first, the physical approach. This led to a first set of field equations that was, surprisingly, not generally covariant.

Einstein struggled to come to terms with his 1913 theory for two years. He recognized his errors towards the end of 1915, just as David Hilbert had turned his mathematical powers to the theory of gravity. Einstein spent November 1915 in strenuous labor, believing that Hilbert might be closing in on him. In this last month the natural mathematical constructions of Riemannian geometry and variational calculus pointed the way to the final field equations.

Nevertheless, Einstein's success should not be seen as exclusively the result of his newly acquired skills and sensibilities in mathematics, but rather as the result of a long learning process in which these skills and sensibilities had been interacting with a multifaceted complex of physical demands and beliefs. This learning process had brought him to the doorstep of the final theory, with the return to the Riemann tensor giving him the last push to the end result.

By 1933 Einstein was lecturing in Oxford that the creative principle in theory construction lies in mathematics, as by then he had become convinced that "nature is the realization of the simplest conceivable mathematical ideas."[12] He

9 See Stachel (2007), pp. 101–108.
10 Renn and Sauer (2007), pp. 157–162.
11 See the articles in the volumes authored and edited by Michel Janssen, John Norton, Jürgen Renn, Tilman Sauer and John Stachel (Janssen *et al.*, 2007a,b); earlier pertinent publications on the subject that we depend on include Norton (1984), Renn and Sauer (1999), Norton (2000); the last article has also been very helpful for the larger thesis of this book.
12 As in Einstein (1933a), on p. 300 in Einstein (1994).

further advised his audience that if they wanted to learn anything about a scientist's methods, they should "not listen to their words," but "fix [...] attention on their deeds."[13] Einstein himself indicated where to look in his case: his experiences in the discovery of general relativity. We will retrace his path to the field equations, to understand how Einstein came to his methodological position of 1933 and hence, to better understand his later scientific pursuit.

1.1 The dual method and the Zurich notebook

Let us first turn to the debate between Einstein and Max Abraham in 1912 over the latter's proposal for a relativistic theory of gravity. From this debate we will learn more about Einstein's heuristic mind-set at the time when he embarked on his search for the field equations of general relativity.

1.1.1 Einstein contra Abraham: the need "to think physically"

Early in 1912 Abraham published a new dynamical theory of gravity.[14] In order to follow through on the special theory of relativity and its rejection of instantaneous action at a distance, Abraham had adjusted the formulae of Newtonian gravitation theory in the following, somewhat obvious way (for unit mass and acceleration a, with potential ϕ and mass density ρ):

$$
\begin{array}{lclcll}
\textit{Newton}: & & & & \textit{Abraham}: & \\
-\vec{\nabla}\phi = \vec{a} & \text{(I)} & \longrightarrow & & -\partial_\mu\phi = a_\mu & \text{(I)} \\
\vec{\nabla}^2\phi = \kappa\rho & \text{(II)} & \longrightarrow & & \partial_\mu^2\phi = \kappa\rho. & \text{(II)}
\end{array}
$$

Because of its apparent mathematical appeal, Einstein was at first quite taken by the theory; he wrote to his intimate friend Michele Besso: "At the first moment (for 14 days!) I [...] was totally 'bluffed' by the beauty and simplicity of his formulas."[15] But he soon saw past the beauty of Abraham's theory.

The first of Abraham's relations (I) implied a variable speed of light. This was not what troubled Einstein; as we saw, he himself contemplated the possibility of a variable speed of light when gravity entered the fray. Abraham, however, initially made free use of the Lorentz transformations in his theory, which according to

[13] Ibid., on p. 296 in Einstein (1994).
[14] Abraham (1912a). Our notation deviates slightly from Abraham's; throughout this book, we have tried to stay as close as possible to the notation of primary literature. However, when notation is obsolete compared to today's usage and a change would not alter content we sometimes differ from the notation of sources.
[15] "Im ersten Augenblick (14 Tage lang!) war ich [...] ganz „geblüfft" durch die Schönheit und Einfachheit seiner Formeln." Einstein to Michele Besso, 26 March 1912, Doc. 377 in Klein *et al.* (1993), pp. 435–438, on p. 437; translation as in Norton (2000), p. 142.

Einstein was no longer allowed owing to the variable c.[16] He started pressing this point with Abraham, through private correspondence and also in public. Their debate grew into a true polemic, which took an unpleasant turn when Einstein did not yield when Abraham started questioning the validity of special relativity. Einstein broke away from the public discussion, making clear that he had not changed his mind about the inadequacy of Abraham's theory.[17]

In his private correspondence he indicated how in his opinion Abraham had been led to his crippled theory:

> Abraham [. . .] made some serious mistakes in reasoning so that things are probably incorrect. This is what happens when one operates formally, without thinking physically![18]

> Abraham's theory has been created out of thin air, i.e. out of nothing but considerations of mathematical beauty, and is completely untenable. How this intelligent man could let himself be carried away with such superficiality is beyond me.[19]

According to Einstein, Abraham had been misled by mathematical esthetics.

Indeed, in his younger years Einstein had had a lukewarm attitude towards mathematics. For instance, he had been unimpressed by the appearance of Hermann Minkowski's representation of his own special theory, and in 1912 he welcomed the announcement that Paul Ehrenfest would succeed Hendrik Antoon Lorentz in Leiden by congratulating him as "one of the few theorists who has not been robbed of his natural mind by the mathematical epidemic!"[20] This attitude may have softened to a degree when he learnt of impressive new tensor methods the same year, yet, having had a forewarning in Abraham's failure, it is likely that Einstein would have started off on his own search for a relativistic theory of gravity with a cautious attitude towards considerations of mathematical beauty and simplicity.[21]

16 For an infinitesimal transformation: $\mathrm{d}x' = (\mathrm{d}x - v\mathrm{d}t)(1 - v^2/c^2)^{-1/2}$. To integrate this to a full coordinate x', the differential $\mathrm{d}x'$ needs to be exact: $\frac{\partial}{\partial t}(1 - v^2/c^2)^{-1/2} = \frac{\partial}{\partial x}[-v(1 - v^2/c^2)^{-1/2}]$. In the case of a static field in which c is a function of the spatial coordinates, this is not fulfilled; see Einstein (1912a), p. 368; see also Norton (2000), pp. 155–157, Renn (2007), p. 315.

17 See Abraham (1912b), Einstein (1912c), Abraham (1912c), Einstein (1912d). For overviews, see: Norton (2000), pp. 141–144, Klein *et al.* (1995), the editorial note, "Einstein and the static theory," pp. 122–128, and Renn (2007).

18 "Abraham [hat] bedenkliche Denkfehler [. . .] gemacht, sodass die Sache wohl unrichtig ist. Das kommt davon, wenn man formal operiert, ohne dabei physikalisch zu denken!" 27 January 1912, Einstein to Heinrich Zangger, Doc. 344 in Klein *et al.* (1993), pp. 394–395, on p. 395; translation as in Norton (2000).

19 "Abrahams Theorie ist aus dem hohlen Bauche, d.h. aus blossen mathematischen Schönheitserwägungen geschöpft und vollständig unhaltbar. Ich kann gar nicht begreifen, wie sich der intelligente Mann zu solcher Oberflächlichkeit hat hinreissen lassen können." 26 March 1912, Einstein to Michele Besso, Doc. 377 in Klein *et al.* (1993), pp. 435–438, on pp. 436–437; translation as in Norton (2000).

20 "Sie sind einer der wenigen Theoretiker, denen die Mathematik-Seuche nicht den natürlichen Verstand geraubt hat!" Einstein to Ehrenfest, before 20 June 1912, Doc. 409, pp. 484–486 on p. 484 in Klein *et al.* (1993); see also Frans van Lunteren, "Theoretische fysica als zelfstandig specialisme" (typescript, 2008). On Minkowski, see Pais (1982), p. 152.

21 Einstein's softening is suggested by a letter to Arnold Sommerfeld: "[. . .] I have acquired an enormous respect for mathematics" ("[. . .] ich [habe] grosse Hochachtung für die Mathematik eingeflösst bekommen [. . .]." 29 October 1912, Doc. 421 in Klein *et al.* (1993), pp. 505–506, on p. 505).