EPISTEMOLOGY OF A THEORY OF EVERYTHING:
WEYL, EINSTEIN, AND THE UNIFICATION OF PHYSICS

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D. Brandon Fogel, B.S., B.A., M.A., M.S.

Don Howard, Director

Graduate Program in History and Philosophy of Science
Notre Dame, Indiana
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In this dissertation, I examine the debate between Hermann Weyl and Albert Einstein over Weyl’s 1918 unified field theory, both for its own merit and for the epistemological implications it holds for the ultimate goal of physics: a theory of everything. I provide a detailed explication of Weyl’s response to Einstein’s objection, including a careful exegesis of Weyl’s somewhat enigmatic distinction between the determination of physical quantities through persistence versus adjustment. I show that Einstein’s concerns were ultimately more methodological in nature than has been previously argued in the literature, thereby providing a novel explanation for the apparent inconsistency he evinces in the well-known 1921 lecture, “Geometry and Experience.” Seen in this light, the differences between Einstein and Weyl in this period appear rather small, especially regarding one of the central themes of the dispute: how to make a physical theory testable. They both argue that, in a fully-developed theory, the behavior
of measuring devices should not be taken as given but rather derived from first principles; i.e., as composite objects able to determine the properties of more basic entities, rigid rulers and regular clocks should arise as particular solutions of the fundamental equations. I argue that this requirement, though important for explaining how theories can make claims about the world that are capable of test through observation, leaves the task half-finished. Equally important is the task of showing why those measuring devices should be considered observable in the first place. I conclude that if we demand that this also be done without arbitrary stipulation, then we will need a detailed, scientific account of the sense-organs we use to make observations, constructed on the basis of the first principles of the theory we wish to test.
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CHAPTER 1:
INTRODUCTION

When Herman Weyl set out in 1918 to expand Einstein’s general theory of relativity, his intention had been merely to repair what he perceived to be an epistemological defect. The result was something almost miraculous; the new theory appeared to unify gravity and electromagnetism in a single geometric source. As gravity and electromagnetism were the only forces of nature known at the time, Weyl’s theory represented a genuine attempt at a theory that could describe all possible natural phenomena—a theory of everything. Although he was not the first to bring gravity and electromagnetism into the same theoretical framework, he was the first to propose a credible and genuine ontological unification, one in which the two forces appear as different aspects of a single entity, in this case the spacetime geometry itself, dubbed by Weyl the “world-metric.” With understandable excitement, Weyl sent a paper describing the theory to Einstein, who found it brilliant but problematic. Einstein argued that, under

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\[ \text{\footnotesize \footnotesize \footnotesize \footnotesize David Hilbert (1915) took first honors for constructing a theoretical framework encompassing both gravity and electromagnetism. His theory did not represent a true ontological unification, however, in that the electromagnetic field potentials did not enter into the fundamental equations in a natural fashion. The unificatory aspects of the work consisted in a necessary connection between the gravitational and electromagnetic field equations (brought about by a coupling between the gravitational and electromagnetic potentials in the underlying Hamiltonian). Yet Weyl, who worked closely at times with Hilbert, recognized the work as an important predecessor to his own, as well as its shortcomings: “Hilbert’s endeavors must be looked upon as a forerunner of a unified field theory of gravitation and electromagnetism. However, there was still much too much arbitrariness involved in Hilbert’s Hamiltonian function” (Weyl 1944, p. 171). For more on Hilbert’s unified theory, see Vizgin (1994, p. 54-69).} \]
a straightforward empirical interpretation, the theory had easily disconfirmable experimental consequences; in order to save the theory, one would have to do away with the direct link to experiment enjoyed by general relativity, raising the possibility that the new theory might have no empirical content whatsoever. Weyl did not agree with Einstein’s objection, and he spent the following five years crafting a sophisticated response, elements of which he continued to advocate to the end of his life. By the end of the 1920s, however, Weyl had concluded that the new quantum mechanics made his theory fundamentally untenable, and he ceased advocating it. He was ultimately able to salvage some of the theory’s novel mathematics, relocating it within quantum mechanics and somewhat unwittingly laying the foundation for the quantum gauge theories that came to dominate physics in the latter half of 20th century. For his part, Einstein soon overcame his own concerns about the empirical content of extensions of general relativity, and by the mid-1920s he was ardently pursuing Weyl’s lead; Einstein would spend the last three decades of his life searching, ultimately unsuccessfully, for his own theory of a unified gravitational and electromagnetic field.

In this dissertation, I scrutinize Weyl’s response to Einstein’s objection, both for its internal consistency and for the applicability of its constituent concepts to the problems of physics of their day and of today, in particular the implications for an epistemology of a theory of everything. Despite their initial differences on Weyl’s 1918 theory, and despite their shifting views on the feasibility of the unified field theory program, the two ultimately shared a common view on how the measurement process should be treated theoretically in any final physics; both thought that the behavior of measuring devices should be derived from the first principles of the theory under test.
This common reasoning will motivate the conclusions drawn at the close of this dissertation, that theoretical representations become capable of test through observation when theorizers situate themselves with some determinate physical constitution, usually implicitly, within the representations they create and evaluate. In a theory of everything, those representations will have to be explicit and complete.

I begin in Chapter 2 with a presentation of Weyl’s field theory and Einstein’s immediate objection to it. As mentioned above, Weyl’s theory was motivated primarily by an epistemological concern; he identified the tangent space at a point of the spacetime manifold with the space of observation immediately accessible to an observer passing through that point. In general relativity, the orientation of a vector parallel-transported from one tangent space to another will in general depend on the particular path taken, but the magnitude of the vector will not. Viewing this as an epistemological defect of the underlying Riemannian geometry, Weyl set out to create a more general geometry, and he found that to do so he needed to introduce a set of quantities that naturally obeyed equations formally identical to Maxwell’s equations. He identified them with the components of the electromagnetic field tensor, and thus claimed to have found a geometric origin for electromagnetism, arising from the same, now-expanded world-metric as do gravity and inertial phenomena.

Einstein objection to Weyl’s theory had two distinct facets, which, along with Weyl’s responses, are detailed in Chapters 3 and 4. The first, discussed in Chapter 3, concerns Einstein’s claim that Weyl’s theory could be easily disconfirmed. Einstein argued that, in Weyl’s theory, the lengths of measuring rods and the rates at which clocks run should depend on the particular local geometry they had previously encountered.
Two clocks, initially together and running at the same speed, will not necessarily continue to run at the same speed if separated and reunited. The frequency of light emitted by atoms, he reasoned, should then depend on their prior histories, and the spectra of celestial objects should not contain sharp emission or absorption lines. Since sharp lines do appear in the observed spectra, he concluded that “internal” clocks of all atoms run synchronously, irrespective of their prior histories.

In response, Weyl outlined a possible mechanism for how the spatial and temporal characteristics of physical objects might fail to display path-dependence, even if the underlying geometry embodies the principle of relativity of magnitude. He argued that the fundamental constituents of physical objects might have properties that adjust to the local conditions of the underlying metrical field, in such a way that some macroscopic properties of the object, such as its length, appear fixed as it is transported, even as the fundamental geometric properties of the constituents undergo transport-related shifts. Quantities determined through persistence, on the other hand, such as the orientation of a spinning top, are determined in part by their values at earlier times. This line of argument appears in print several times, although it is not until the fifth edition of Raum, Zeit, Materie (1923) that Weyl provides a full, technical clarification along with a complete model of an actual adjustment mechanism. This model, the “celestial compass,” does not quite work the way he intends, although it is still helpful for clarifying how he understood the distinction between adjustment and persistence to operate. After cataloguing the instances of the argument concerning determination through adjustment, I analyze the development of Weyl’s understanding of the distinction, which he continued to advocate to the end of his life, long after he had abandoned his field theory. I conclude that the
distinction itself has merit, but that it functions as a promissory note, without much explanatory power.

In Chapter 4, I examine a second aspect of Einstein’s objection, one that was more fundamental, seemingly more epistemological in character than that of empirical disconfirmation. Einstein argues that Weyl’s handling of the measurement process and its relation to the metric robbed the theory of its empirical basis, rendering it mere mathematical or formal speculation. Einstein appears to argue that without a direct link between the spacetime metric and the behavior of measuring rods and clocks, presupposed to be rigid, a theory cannot be given empirical content, a position taken up and formally articulated by the logical empiricists Schlick and Reichenbach in the 1920s. In response, Weyl argues that such presuppositions about the behavior of measuring devices have no place in a systematic theory; as complex objects, their behavior should be derived from first principles. General relativity suffers just as much from the lack of such a derivation as his own theory, Weyl claims, and in fact even more so, since it has no hope of accounting for the electromagnetic nature of molecular structure.

In the well-known lecture “Geometry and Experience” (1922), Einstein seems to reverse himself and argue along with Weyl that presuppositions about the rigidity of rods and clocks should be employed only provisionally, and that they should not play a fundamental role in the “edifice of physics.” There and in other writings, Einstein also articulates a clear holism about the confirmation of physical theories, arguing that the division of a theory into a priori and a posteriori parts is always arbitrary and that empirical claims cannot be confirmed or disconfirmed in isolation. Such views distinguish him sharply from the logical empiricists, but they also appear to contradict his
objection to Weyl’s theory, which is more or less repeated in “Geometry and Experience.” I argue that we can reconcile the apparent contradiction by reading Einstein as registering not an epistemological complaint, but rather a methodological one about the source of Weyl’s motivation. With the help of Einstein’s distinction between principle and constructive theories, we can resolve the apparent conflicts in Einstein’s thinking about rigid bodies as well as his subsequent endorsement of the search for a unified field theory. In such light, Einstein’s and Weyl’s views on how to identify the empirical content of physical theory appear much more similar than is evident from their opposition regarding Weyl’s particular theory; most importantly, they both agree that, in a systematic theory, the behavior of measuring devices should be derived from first principles.

In Chapter 5, I examine Weyl’s and Einstein’s scientific methodologies in more detail, in order to determine with greater precision their views on the ultimate goal of physics and of science more generally. Both tell a story in which science progresses in iterated steps from sense-experience toward logical-mathematical systems of greater and greater abstractness. Both argue strongly that the physical world is inherently unified and that there are deep interconnections among all the sciences, doctrines that today fall under the general heading “unity of science.” Conversely, their views on the role of a priori considerations in the development of science are rather different. Einstein argued that good physicists are “tamed metaphysicists,” guided but not constrained by a desire for simplicity and naturalness; theories originate in acts of untraceable creativity, and empirical adequacy is ultimately the sole criterion of success. Conversely, Weyl was more comfortable with the idea that our theories reflect, at least in part, our construction
as cognizers; every theory should contain structures, such as tangent spaces on a
differentiable manifold, that represent the space of intuition accessible to the observer,
and the theory should indicate how those spaces are to be connected. That only certain
structures are capable of representing such spaces of intuition is indeed an a priori
constraint on theory construction.

Finally, an important and somewhat novel commonality can be found in
Einstein’s and Weyl’s visions of the end-goal of physics. Both indicate that, as theory
becomes more and more abstract, the internal resistance to modification, or “rigidity,” of
the overall logical-mathematical structure should become a critical epistemic virtue.
Einstein invokes the idea explicitly, citing it as a primary feature of principle theories and
claiming an important role for it in guiding him to general relativity, and even more so to
his unified field theory. The notion of theoretical rigidity appears in Weyl’s writings in a
more nuanced way, as a prohibition against a final theory containing continuous
parameters that require determination through measurement. A complete theory, he
argues, should leave “no room for play,” but rather should have parts that fit together
tightly and harmoniously.

In the sixth and final chapter, I broaden the scope of the analysis by attempting to
determine what we should learn from the Weyl-Einstein debate about the general relation
between theory and world, in particular how it is that logical-mathematical constructs can
be used to make claims about the world that are capable of test through observation. This
question becomes particularly acute in the context of a theory that purports to describe
everything in existence—a theory of everything—where the common bootstrapping
strategy, that of provisionally holding some background theory to be well-understood and unproblematic, is by definition unavailable.

I begin with the principle found in Chapter 4 to be common to both Weyl and Einstein: in a complete, systematic theory, assumptions about the behavior of measuring devices should be derived from first principles. This reasoning involves a kind of completeness claim, one related to but distinguishable from the more general completeness claim inhering in the definition of a theory of everything. I state both notions of completeness in terms of a device used frequently in physics but that has received little attention in the philosophical literature: the degree of freedom. The Weyl-Einstein reasoning requires that observations of measuring devices evince no degrees of freedom that do not have counterparts in their theoretical representations, i.e., that the representations describe their subject matters completely. A theory of everything is then one that contains subject-matter-complete representations for every observable object or phenomenon.

Equally important to the completeness claim is the requirement that the theory determine for itself, through derivation from first principles rather than arbitrary stipulation, what is and what is not observable. I argue that a solution to the latter difficulty is not possible without a detailed account of the operation of those physical components of the theorizer that enable physical interaction with the objects being theorized about (whether mediated by other objects or not)—that is, a complete derivation from first principles of the theorizer’s sense-organs in precisely the manner envisioned by Einstein and Weyl for measuring rods. This is not to say that a theory of everything cannot be found without taking the physiology of the human sense-organs into
account, but rather that the story of how the empirical content of such a theory is identified—i.e., of how it is connected to the physical world through observation—will be incomplete without doing so.
By 1918, general relativity enjoyed widespread admiration among theoretical physicists, though not quite acceptance; it would be another year before Eddington’s eclipse measurements offered convincing empirical evidence and delivered worldwide fame to Einstein and to his theory. Weyl, then in his early 30s, thought Einstein’s theory a natural step in a grand historical progression of principles of relativity, from the equivalence of inertial reference frames in Newtonian mechanics, through special relativity and its relativity of simultaneity, to general relativity and its equivalence of all reference frames. Yet, Weyl was also convinced that the theory had stopped short, and that a further, final extension of relativity was needed. His belief was not motivated by any experimental evidence or allegiance to some competing theory, but rather by epistemological concerns. He thought that an observer should have access only to information available in his or her immediate vicinity, and that general relativity allowed a kind of knowledge at a distance. While directions cannot be compared at distant locations in general relativity, lengths can be; Weyl thought that measures of distance, or “gauges,” should be valid only locally. In order to rectify this, to make gauge of length a local concept, Weyl had to introduce a certain structure to the Riemannian geometry that underlay general relativity; that structure, to his great surprise, turned out to be identical to that of the Maxwellian electromagnetic field. The result was a new, more general kind
of geometry, one that appeared to have the resources to describe all known natural phenomena, to unify the gravitational and electromagnetic fields into a single geometric entity. That he had not set out to do this, but rather had been attempting to alleviate an unrelated epistemological concern, was only more evidence in its favor.

In this chapter, I present and clarify Weyl’s unified field theory. The truly revolutionary aspect of the theory was its creation of a new kind of geometry; this opened a path for later generalizations that would form the foundation of much of 20th century physics. As a theory of physics, Weyl’s 1918 theory did not on its own have a lasting impact, in part because Weyl was never able to finalize its fundamental field equations. His attempts to derive unique equations by varying an action principle were ultimately unsuccessful, and this surely played a role in his subsequent abandonment of his theory. However, by the end of the 1920s, he had successfully relocated the central mathematical components of the theory, gauge transformations and gauge symmetry, within quantum mechanics, and that eventually set the stage for the generalizations that led to the current theories of the weak and strong nuclear forces. After giving a conceptual overview of Weyl’s theory in Section 2.1, I will offer a technical review of the basic concepts of differential geometry in Section 2.2, which will allow a rigorous technical presentation of the theory in Section 2.3. In Section 2.4, I will discuss Einstein’s objection to Weyl’s theory and Weyl’s later recasting of gauge symmetry as a feature of the quantum mechanical wavefunction.
2.1. Conceptual overview of Weyl’s field theory

Between 1918 and 1923, Weyl presented his unified field theory in print several times, in various articles and in five editions of his book on relativity theory, *Raum, Zeit, Materie*. The theory does undergo some evolution over the course of the different presentations, driven primarily by Weyl’s growing realization that its fundamental laws could not be uniquely specified. This led him to separate the theory into two distinct parts: 1) the “pure, infinitesimal geometry,” which provides a set of geometric structures, and 2) the action principle, which provides a field equation, or “law of nature,” that governs how those structures evolve over and above their intrinsic natures. In the 1918 article, the determination of the action principle flows naturally out of the pure, infinitesimal geometry; in the 1923 fifth edition of *Raum, Zeit, Materie*, discussion of the two takes place over 100 pages apart. The separation also facilitates the presentation of the new geometry not as a correction to one already given, but from a fundamental perspective, one in which Riemannian geometry arises only as a special case. Because the geometric portion attained a greater level of sophistication than the action principle and ultimately had the more lasting impact, it will receive comparatively more attention here.

2.1.1. Pure, infinitesimal geometry

The primary lesson that Weyl drew from the development of relativity theory is that physics should concern itself only with quantities that could be defined locally: “The principle that the world should be understood through its behavior in the infinitely small is the driving epistemological motivation of the physics of local action” (Weyl 1923, p. 86). However, Weyl was prompted to believe this not from a distaste for superluminal
causal influences, but by consideration of the nature of observation. As physically-constituted observers situated at particular locations in space and time, the thinking goes, we have access only to a localized fragment of the world. In a mathematical representation, Weyl shrinks the observer to a single point in spacetime, which he then dubs the “ego center” or “point eye” (Weyl 1949, p. 135). The space of intuition, constructed within us from sense-data, is just like the tangent plane on a curved surface:

The intuitive space may be likened to a tangent plane touching a curved surface (the physical space) at a point $O$; in the immediate vicinity of $O$ the two coincide, but the larger the distance from $O$ the more arbitrary will the one-to-one correspondence between plane and surface become that one tries to establish by continuing the relation of coincidence near $O$. (ibid.)

The actual geometry of the space we live in might be very different globally from the geometry we construct locally. But our physical descriptions of the world, and the geometry on which they are based, should include nothing but locally accessible quantities.

The theory of differentiable manifolds provided Weyl the formal means to articulate this vision. A differentiable manifold is a set of points with certain smoothness properties that supports the construction of two kinds of vector spaces at each point, the tangent space and co-tangent space (see Section 2.2.1 for discussion of the difference). The surface of a sphere provides a simple example. At each point on the surface, a tangent plane can be constructed by considering all of the possible vectors aiming out from that point, tangent to the sphere (Figure 2.1). The key point is that different tangent planes (i.e., vector spaces) will be constructed at each point on the sphere. Each tangent vector is thus tied to a specific point, and vectors attached to different points will not be directly comparable—they live in completely separate spaces. A very rich set of
geometric structures, known as tensors, can be constructed by combining the vectors in various ways, all in complete isolation from those at every other point. Weyl’s idea is that quantities that are measurable at a given location should be defined only in terms of the vectors and tensors existing at that point, again, not from concerns about locality, but about epistemic access.

Figure 2.1: A tangent space on a two-dimensional surface.

Physics is concerned with describing the motions of objects from point to point, and so, in this framework, the task of physics is to connect the different tangent spaces at each point with one another. In fact, the problem is even more pressing than it might seem; the manifolds of relativity theory represent spacetime, not just space, and objects in it are represented as continuous lines, meaning that they are never “stationary” in the manifold.\(^2\) Objects at rest in a given frame of reference simply have all of their motion pointed in that frame’s temporal direction.\(^3\) This means that the space of intuition of a given observer, if it is to be identified with (or at least be a mirror of) the tangent space at

\(^2\) More specifically, the 4-vector velocity of every material particle has the same, constant norm—the speed of light.

\(^3\) Note that this is one way to understand the origin of time dilation in relativity; in a given frame of reference, objects move by shifting some of their (constant) spatiotemporal motion from the temporal to the spatial direction, meaning that they move more slowly through time.
the observer’s location, must be constantly shifting, transported into and out of different tangent spaces within the manifold. The evolution of physical quantities over time, then, is also a matter of tangent spaces being connected with, or transported into, one another.

The question of how to connect vector spaces constructed on differentiable manifolds is a purely mathematical one, and it is this that falls under the heading of “geometry.” Different ways of connecting the spaces produce different kinds of geometries; the Riemannian and Euclidean are special cases. It is not until some quantities are designated as observable that a physical theory is generated, and for Weyl this does not occur until laws of nature are established, usually by way of an action principle. Determination of the geometry is the first, necessary step, which sets the stage for physics by providing a set of locally-defined quantities along with general, but well-defined rules for transporting them along continuous paths between locations.

2.1.2. Parallel transport

Weyl concerns himself with 1-to-1 mappings between vector spaces that preserve the linearity properties of those spaces (scalar multiplication and vector addition). The procedure that accomplishes this, known as parallel transport, was introduced by Tullio Levi-Civita (1917) and can be explained in intuitive terms. A tangent vector is “dragged” along some arbitrary path between two points, $P$ and $Q$, as in Figure 2.2, such that it is always kept pointing in the same direction; if the path turns, then the angle between the vector and the direction of travel will change. Otherwise, that angle stays the same. At the end of the route, at $Q$, the vector will coincide with another vector in the tangent space there; that is, it is pointed in some direction at $Q$. A vector from one tangent space
will thus have been mapped or transported into another. Do this with all of the vectors at \( \mathcal{P} \), and the desired mapping results.

**Figure 2.2: Vector transport on a two-dimensional surface.**

There are two key features of the mapping produced by parallel transport. The first is that it depends on the shape of the space; if the surface were deformed in some way along the path, then the vectors at \( \mathcal{P} \) would be mapped to different vectors at \( \mathcal{Q} \).

The second is that the mapping depends on the path chosen. The classic demonstration of this is depicted in Figure 2.3. Let \( \mathcal{P} \) and \( \mathcal{R} \) be points on the “equator” of the sphere and \( \mathcal{Q} \) be at the “north pole.” If one transports the vector at \( \mathcal{P} \) along the equator to \( \mathcal{R} \), it will remain perpendicular to the direction of travel the entire time, always pointing “north.” One can then parallel transport the vector from \( \mathcal{R} \) to \( \mathcal{Q} \), during which it will point in the direction of travel the whole time. If one starts over, and transports the same vector directly along the path from \( \mathcal{P} \) to \( \mathcal{Q} \), then two different vectors will result at \( \mathcal{Q} \). The vectors transported along the two paths from \( \mathcal{P} \) to \( \mathcal{Q} \) will differ by an angle equal to the difference in “longitude” between \( \mathcal{P} \) and \( \mathcal{R} \); these are shown in Figure 2.4.
The information needed to parallel transport a vector—to keep it pointing in the same direction at each point along a given path—is stored in a geometric object known as the affine connection. It defines a mapping between the tangent space at a given point and the tangent space at a point infinitesimally close in some specified direction. As such, it describes the shape of the space from a purely local point of view. The mapping between two distant points along some path is constructed by combining, in series, all of the infinitesimal mappings along that path.

The path-dependence of the vector space mapping is due to the curvature of the surface; if the two-dimensional surface were a flat plane, then the mapping between any
two points would be independent of the path taken between them. In Riemannian spaces, the curvature of the surface is allowed to vary freely from point to point. The sphere is one rather special case; its curvature is constant everywhere. Most Riemannian spaces cannot be visualized; the two-dimensional surfaces that we can imagine embedded in a three-dimensional Euclidean space are, again, special cases. Riemannian manifolds can exist in any dimension, and they need not be thought of as embedded in higher-dimensional spaces. The affine connection describes the properties of any surface using quantities entirely intrinsic to (or constructed from) the manifold itself.

Levi-Civita showed that, in all Riemannian spaces, the affine connection must have a certain form. The major geometric innovation in Weyl’s unified field theory was the identification of a more general affine connection, one which had the Riemannian connection as a special case. But whereas Riemann was led to his manifolds by visualizing surfaces embedded in Euclidean space, Weyl was led to his by considering how vectors transform under parallel transport. He concluded that there was a peculiar asymmetry in the way the basic properties of vectors are treated, a vestige of the Euclidean roots of the development of geometry.

2.1.3. Metricity and Weylian manifolds

In introductory geometry, one learns about vectors as arrows drawn from one point to another in a Euclidean space. The vectors have a certain direction and a certain length, given by the Pythagorean theorem. In introductory physics, one learns to abstract this concept somewhat, so that vectors representing physical quantities such as momentum need not be thought of as extended in space, but rather as existing at a single point, with some orientation and magnitude there. However, in the preceding discussion
of vector spaces on manifolds and mappings between them, no mention was made of the magnitude of the vectors, either before or after transport. Indeed, only one mention was made of orientation, in noting the difference between the vectors resulting from parallel transport around two different paths on the sphere—and then all we really needed to know was that the vectors did not coincide. The notions of vector space mapping, parallel transport, and affine connection do not require the notions of magnitude and orientation. The latter are metrical concepts, and they require more structure than the connection for precise definition; that structure is the metric. Indeed, it is one of the more amazing things about differential geometry that one can do so much with vectors while knowing so little about them.

The asymmetry that Weyl noticed is evident in the example of parallel transport discussed above. In transporting the vector from $P$ to $Q$, we assumed that its magnitude would not change—we had no reason to think it would. The vectors resulting from transport along the two paths in Figure 2.3, depicted in Figure 2.4, differ in orientation, but not in magnitude. This is a feature of all Riemannian spaces; the orientation of a vector is tied to the particular tangent space in which the vector resides, but the magnitude is not. For Weyl, who identified the tangent space with the observer’s space of intuition, this meant that physical quantities based on vector orientation would be relative, but those on magnitude absolute and comparable at a distance, an unacceptable incompleteness in the relativity revolution. According to the basic precepts of the pure, infinitesimal geometry, one should have access only to information available in one’s local vicinity and should not be able to know magnitudes at distant locations. Thus, he
proposed to append a principle of the relativity of vector magnitude to the principle of relativity underlying Einstein’s general relativity.

The question Weyl initially posed was this: If the Riemannian connection is loosened, so that vector magnitude is allowed to vary freely, what extra structure is needed so that the affine connection (i.e., the mappings between the tangent spaces of infinitesimally close points) is still well-defined? He would later reverse the order of presentation, so that relativity of vector magnitude would be introduced as an integral part of the notion of metricity and thus Riemannian space would be derived. Either way, the additional structure needed was most neatly expressed as a covariant vector (or linear differential form) obeying certain constraints that turned out to be formally identical to two of the four field equations of Maxwellian electrodynamics. He found that the other two equations could be generated from most natural choices of action principle. The same action principles also produced equations that, while not formally identical to the Einstein field equation of general relativity, were close enough that large deviations would not be expected. With understandable excitement, he concluded that, just as general relativity represented a geometrization of gravitational phenomena, his new theory represented a unified geometrization of both gravitational and electromagnetic phenomena, which were, at that point, the only kind known.

2.2. Technical definition of basic concepts of differential geometry; derivation of the Riemannian connection

In the previous section, the concepts of differentiable manifold, tangent space, affine connection, and metric were introduced and discussed in loose, intuitive fashion.
In this section, I will give those concepts rigorous definition, so that Weyl’s theory can be presented more precisely. The basic ideas of differential geometry can be introduced in a variety of ways, some better suited for calculations and others for clarifying the foundations. Most presentations of Weyl’s theory (including his own) adopt calculational approaches (the so-called “tensor calculus”); this has the unfortunate effect of obscuring some of the foundational aspects of the work. I will instead begin with a more abstract, algebraic introduction to the concepts and then indicate how they enter into Weyl’s more heuristic approach.4

2.2.1. Differentiable manifolds, vector spaces, and the dual product

An $n$-dimensional differentiable manifold is a set of points such that, around every point, there exists a region that can be mapped to $\mathbb{R}^n$, the $n$-dimensional Euclidean space. A coordinate system $\{x^\alpha\}$ (where $\alpha$ runs from 0 to $n-1$) is a system of functions from points to the reals that covers part or all of the manifold.

The differentiability properties of a manifold can be used to construct two different vector spaces at each point. One is the tangent vector space, the collection of directions emanating from the point. The elements of the second vector space go by several names, “co-tangent vectors,” “covariant vectors,” and “linear differential forms.”5 I will use the latter two here. Tangent vectors will generally be represented by bold-
faced, lower-case Latin letters, such as $\mathbf{u}$, and covariant vectors by bold-faced, lower-case Greek letters, such as $\boldsymbol{\sigma}$.

Every coordinate function uniquely determines a covariant vector at each point in its domain; we can define an operator, $\mathbf{d}$, that identifies this vector when applied to a function: $\mathbf{d}f$. Note that it is a linear operator: $\mathbf{d}(cf) = c\mathbf{d}f$ (where $c$ is a scalar) and $\mathbf{d}(f + g) = \mathbf{d}f + \mathbf{d}g$ (where $f$ and $g$ are functions). Given a coordinate system $\{x^\alpha\}$, the vectors $\{dx^\alpha\}$ form a basis for the covariant vector space, so that any covariant vector can be written as a sum of them$^6$

$$\boldsymbol{\sigma} = \sigma_\alpha dx^\alpha.$$ 

Arbitrary covariant bases are written $\{\omega^\alpha\}$. The action of $\mathbf{d}$ on an arbitrary function $f$ is

$$\mathbf{d}f = \frac{\partial f}{\partial x^\alpha} dx^\alpha.$$ 

The differential operator $\frac{\partial}{\partial x^\alpha}$ thus serves to pick out a direction in the manifold—the one in the direction of increasing $x^\alpha$, but constant for all other $x^\beta$. It can be used to determine a basis for the tangent vector space, written $\{e_\alpha\}$. A kind of product can be defined between tangent vectors and covariant vectors, which I will refer to as the dual product$^7$:

$$\langle \sigma, \mathbf{u} \rangle = \sigma_\alpha u^\alpha.$$ 

---

$^6$ Note that the Einstein summation convention is used here and throughout this presentation.

$^7$ Misner, Thorne, and Wheeler (1973, p. 202) use the dual product without naming it and without quite stressing its fundamental importance.
Bases are usually chosen so that $\omega^\alpha, e_\beta = \delta^\alpha_\beta$ (such bases are called dual). Note that the dual product is invariant under changes of both coordinate and basis.

Tensors are created from vectors by use of the tensor product: $a \otimes b$. The tensor product is linear and associative, so that strings of vectors can be multiplied together without cumbersome parentheses. Tensor rank is indicated by $t$, where $t$ is the number of tangent vectors in the product and $c$ the number of covariant vectors. Tensors of a given rank form an $n^t c$-dimensional vector space whose basis elements are the tensor products of the constituent tangent and covariant vector bases.

2.2.2. The affine connection, covariant derivative, and parallel transport

Tensor fields are formed by smoothly picking out a single tensor of a given rank at each point in the manifold. These can be represented with a point argument $v(\mathcal{P})$ or coordinates $v(x^\alpha)$, although the argument is typically omitted. The smoothness properties of the tensor fields are used to define a derivative that captures the notion of infinitesimal parallel transport. Because this notion is so central to Weyl’s theory, it is worth stating precisely here. This is typically done for vector fields, with that of other tensor fields following by analogy or extension.

Take a vector field $v(\mathcal{P})$ and a curve $\mathcal{C}$ parameterized by a real variable $\lambda$. Let $u$ be the tangent vector to the curve at $\mathcal{C}(0)$, pointing in the direction of increasing $\lambda$. The covariant derivative is defined by taking $v(\mathcal{C}(\lambda))$, transporting it to $\mathcal{C}(0)$, subtracting $v(\mathcal{C}(0))$ from it, and then taking the limit as $\lambda$ goes to 0:
In the limit, the only information about the curve that is needed is its tangent vector at that point, which is why \( \dot{C} \) does not appear on the left-hand side, only \( u \). The covariant derivative indicates how much a vector field changes at a point in a given direction. It is linear in both the vector being operated on and on the tangent vector indicating the direction.

The affine connection encodes the behavior of the covariant derivative over the vectors in a given pair of bases. The relationship is expressed neatly by

\[
\Gamma^\alpha_{\beta\gamma} \equiv \left\langle \omega^\alpha, \nabla_{e_\gamma} e_\beta \right\rangle. \tag{2.1}
\]

As long as the space is torsion-free\(^8\), the covariant derivative must observe the chain rule\(^9\) on the dual product, and this implies that \( \left\langle \nabla_{e_\gamma} \omega^\alpha, e_\beta \right\rangle = -\Gamma^\alpha_{\beta\gamma} \). Also, in torsion-free spaces, the connection is symmetric in its lower indices: \( \Gamma^\alpha_{\beta\gamma} = \Gamma^\alpha_{\gamma\beta} \).

We can use the properties of the dual product to define a directionless derivative, \( \nabla v \). That is, let \( \left\langle \nabla v, u \right\rangle \equiv \nabla_u v \), so that

\[
\nabla e_\mu = \Gamma^\alpha_{\beta\gamma} e_\alpha \otimes \omega^\gamma \tag{2.2}
\]

---

\(^8\) This involves the assumption that the dual product is the same whether the covariant vector is transported in one direction or the tangent vector is transported in the other along the same path. That is,

\[
\left\langle \sigma(\dot{\gamma}), v(\dot{\gamma}) \right\rangle = \left\langle \sigma(\dot{\gamma})\, \text{transported to } \dot{\gamma}, v(\dot{\gamma}) \right\rangle
\]

\(^9\) That is, \( \nabla_u \left\langle \sigma, v \right\rangle = \left\langle \nabla_u \sigma, v \right\rangle + \left\langle \sigma, \nabla_u v \right\rangle \).
Then, since the covariant derivative observes the chain rule over the tensor product,\(^{10}\)

\[
\nabla v = \nabla \left( v^\alpha e_\alpha \right) = \left( \frac{\partial v^\alpha}{\partial x^\gamma} + v^\beta \Gamma^\alpha_{\beta \gamma} \right) e_\alpha \otimes \omega^\gamma.
\]

(2.3)

This derivation makes clear that the connection coefficients arise because of the way that the basis vector fields twist and turn. The inability to define basis vector fields that do not twist and turn over finite regions is one measure of curvature.\(^{11}\)

Weyl’s use of the affine connection differs slightly in that he focuses attention only on tensor components, and not on the tensors themselves. If a vector \(v\) is parallel-transported (i.e., \(\nabla v = 0\)) an infinitesimal amount \(\varepsilon\) in some direction \(u\), then according to (2.3) it is altered by

\[
\frac{\partial}{\partial x^\alpha} v^\alpha + v^\beta \Gamma^\alpha_{\beta \gamma} \varepsilon u^\gamma e_\alpha. \]

If we define the change in coordinates by

\[
dx^\alpha = \varepsilon u^\alpha, \text{ and the change in the components of } v \text{ by } dv^\alpha \equiv \frac{\partial v^\alpha}{\partial x^\beta} dx^\beta, \text{ then }
\]

\[
dv^\alpha = -v^\beta \Gamma^\alpha_{\beta \gamma} dx^\gamma. \quad (2.4)
\]

It is in this form that Weyl prefers to introduce the affine connection components. Unfortunately, this can appear somewhat mysterious without a precise understanding of its relation to the vanishing of the covariant derivative, \(\nabla v\).

2.2.3. Metricity and the Riemannian connection

So far, we have said nothing about a metric; the notions of vector space, affine connection, and even curvature are independent of that of metricity. Indeed, this was one

\(^{10}\) That is, \(\nabla (A \otimes B) = (\nabla A) \otimes B + A \otimes (\nabla B)\).

\(^{11}\) See Section 2.3.3 for discussion of another way to measure curvature, by way of the curvature form.
of the great insights of Levi-Civita and Weyl. Metricity comes into play when we wish to compare tangent vectors with one other, by way of an inner product: $u \cdot v$. All that is required of this new product is that it be linear in both vectors, commutative, and have an inverse. It can thus be represented by a symmetric \( \begin{pmatrix} 0 \\ 2 \end{pmatrix} \)-rank tensor as follows:

$$u \cdot v \equiv \langle g, u \otimes v \rangle = g_{\alpha\beta} u^\alpha v^\beta .$$

(2.5)

$g = g_{\alpha\beta} \omega^\alpha \otimes \omega^\beta$ is the metric tensor, where $g_{\alpha\beta} = g_{\beta\alpha}$. The inverse of the metric tensor is given by $\bar{g} = g^{\alpha\beta} e_\alpha \otimes e_\beta$, where $\langle g, \bar{g} \rangle = 1$ (in components: $g^{\alpha\beta} g_{\beta\gamma} = \delta^\alpha_\gamma$).\(^{12}\)

The Riemannian affine connection can be derived in a very straightforward way. All that is needed is to require the inner product to observe the chain rule under covariant differentiation, i.e., that

$$\nabla (u \cdot v) = (\nabla u) \cdot v + u \cdot (\nabla v) .$$

Then, since the tensor and dual products both observe the chain rule,

$$\nabla g = 0$$

(2.6)

must hold. This condition is often referred to as “metric compatibility”—the metric is compatible with the covariant derivative if the inner product it defines observes the chain rule. It is the necessary and sufficient condition to describe a Riemannian manifold. This could equally be called a “constancy” condition, since it requires that the metric tensor (though not its components) be constant under transport everywhere on the manifold (see Appendix A for further discussion of this point).

\(^{12}\) The tensor $1 \equiv e_\alpha \otimes \omega^\alpha$ is the “unit tensor.” It has the same components in all bases.
The component form of the compatibility condition follows from (2.2) and the chain rule on the tensor product:

\[ \nabla g = \left( \frac{\partial g_{\alpha\beta}}{\partial x^\gamma} - g_{\rho\sigma} \Gamma^\rho_{\alpha\gamma} - g_{\alpha\mu} \Gamma^\mu_{\beta\gamma} \right) \omega^\alpha \otimes \omega^\beta \otimes \omega^\gamma \]

Since the basis vectors are independent of one another, (2.6) means that each term must vanish independently. Using the inverse of the metric, we can solve for the connection coefficients in terms of the metric:

\[ \Gamma^\alpha_{\beta\gamma} = \frac{1}{2} g^{\alpha\mu} \left( \frac{\partial g_{\nu\mu}}{\partial x^\gamma} + \frac{\partial g_{\mu\nu}}{\partial x^\beta} - \frac{\partial g_{\nu\beta}}{\partial x^\mu} \right) \quad (2.7) \]

These are the coefficients of the Riemannian connection.

2.3. Technical presentation of Weyl’s field theory

We now have the conceptual and notational tools to go through Weyl’s theory in detail. The major geometric innovation of the theory was the loosening of the affine connection. As shown in the previous section, the Riemannian connection can be specified entirely in terms of the metric, an \( (0, 2) \)-rank tensor field. This is a highly non-trivial condition; in 4 dimensions, there are 40 independent connection coefficients and only 10 metric components. Weyl’s accomplishment was to find a way to exploit the unused degrees of freedom within the connection; in his geometry, the connection is specified by the combination of a symmetric \( (0, 2) \)-rank tensor field and a linear differential form (i.e., a \( (0, 1) \)-rank tensor field), with 10 and 4 degrees of freedom, respectively. This obviously leaves much room within the connection for further structure, and others, including Einstein, Eddington, and Cartan, would continue to
explore that space, using different structures to specify the degrees of freedom in various, often very clever ways.\textsuperscript{13}

The most important questions for Weyl, as far as the geometry is concerned, were how to introduce the new differential form and how to motivate the desired relation to the affine connection. His answers to these questions undergo some evolution between the 1918 paper and the later editions of \textit{Raum, Zeit, Materie}. The 1918 presentation begins with the Riemannian metric and generalizes it by requiring all inner products between tangent vectors to be variable under transport. By the final edition of \textit{Raum, Zeit, Materie}, Weyl had inverted the concepts somewhat; he argues there that the concept of distance itself should be local and capable of transport, the link to vector transport being secondary. The physical question of how the new structure relates to the electromagnetic field is handled separately in both places, in part by way of action principles, with decreasing optimism in the later work.

In this section, I will present and compare Weyl’s two derivations of the new differential form.\textsuperscript{14} This will be followed by a discussion of curvature in the new theory and how it allows the derivation of the homogeneous Maxwell’s equations. I will conclude with a discussion of Weyl’s use of action principles and his derivation of the inhomogeneous Maxwell’s equations.

\textsuperscript{13} See Vizgin (1994, chapters 4 and 5) for more on Einstein’s and Eddington’s unified field theories, and also Ryckman (2005, chapter 8) on Eddington’s theory of the affine field. See O’Raifeartaigh (1997, Chapters 2 and 3) for discussion of Cartan’s theory of gravitation. Goenner (2004) is also an excellent source for information on Einstein’s and Cartan’s unification programs (and Weyl’s as well).

\textsuperscript{14} A more concise, more abstract presentation of my own design can be found in Appendix A.
2.3.1. Derivation in the original 1918 paper

The 1918 paper in which Weyl first presents his theory is fairly short, only about fifteen pages. He opens with a general discussion of basic geometric and physical concepts, intended to motivate both the expansion of relativity and the geometrization of electromagnetism. He begins the derivation by considering a differentiable manifold with a metric defined on it, asserting that the metric should be determined only up to some arbitrary proportionality-factor. He expresses this mathematically by requiring that, in any expression containing the metric, one should be able to replace the metric, \( g \), with the same tensor multiplied by some arbitrary positive factor, \( \lambda g \). He refers to the factor, \( \lambda \), as a “gauge” (Eich), and allows it to vary smoothly with location—it is thus a local gauge. The gauge factor has the same status as the coordinate functions and thus has no physical significance; physically meaningful expressions should be invariant under changes in it—gauge transformations—and this new invariance property is taken to characterize the theory.\(^\text{15}\)

Weyl next asserts that a “similarity map” exists between the tangent spaces at any two points on the manifold—as discussed above, this means, at a minimum, that the manifold has an affine connection and that the map is linear.\(^\text{16}\) But this places no constraint on the inner product given by the metric; Weyl asserts only that the mapping

\(^{15}\) Although, as I show in Appendix A, simply adding gauge freedom is not quite enough to produce a new geometry.

\(^{16}\) Weyl characterizes parallel transport by means of two axioms (Weyl 1918, p. 32), only the first of which asserts the existence of a similarity map. The second requires that if two infinitesimal tangent vectors are each transported in the direction pointed out by the other, they will form a closed parallelogram, which has the effect of making the space torsion-free (see fn. 8).
preserves that inner product up to some constant factor. That is, for all \( u, v \), where \( u \rightarrow u' \) and \( v \rightarrow v' \),
\[
\mathbf{u} \cdot \mathbf{v} \rightarrow c \mathbf{u}' \cdot \mathbf{v}',
\]
where \( c \) is some scalar.

Weyl then proceeds to derive the new affine connection by considering a similarity mapping between infinitesimally close points, using infinitesimal variations in the components of the various vectors and tensors. A vector \( \mathbf{u} \) that is transported from a point with coordinates \( \{x^\alpha\} \) to one with coordinates \( \{x^\alpha + dx^\alpha\} \), where each \( dx^\alpha \) is an infinitesimal quantity, has components that transform according to \( u^\alpha \rightarrow u^\alpha + du^\alpha \), where \( du^\alpha \) is given as in (2.4). Weyl asserts that the change in the norm of any two vectors should also be an infinitesimal quantity, so that the proportionality factor in (2.8) should be written \( c = 1 + d\phi \). If all components are then allowed to vary by infinitesimal amounts, then
\[
\mathbf{u}' \cdot \mathbf{v}' = c \mathbf{u} \cdot \mathbf{v}
\]

Ignoring all terms of infinitesimal order 2 or greater, this reduces to
\[
\left( g_{\alpha \beta} + dg_{\alpha \beta} \right) (u^\alpha + du^\alpha)(v^\beta + dv^\beta) = \left( 1 + d\phi \right) g_{\alpha \beta} u^\alpha v^\beta.
\]

From (2.4) and the fact that (2.9) must hold for all vectors \( \mathbf{u} \) and \( \mathbf{v} \),
\[
dg_{\alpha \beta} - g_{\alpha \beta} \Gamma_{\alpha \gamma}^\delta dx^\gamma - g_{\alpha \delta} \Gamma_{\beta \gamma}^\delta dx^\gamma = g_{\alpha \beta} d\phi.
\]

From this, Weyl says, it follows that \( d\phi \) is some general sum of the coordinate differentials:
\[
d\phi = \phi_{\alpha} dx^\alpha.
\]
As it happens, this does not quite follow; all that one can conclude is that \( d\phi \) is, at a minimum, a total differential, not a general sum of coordinate differentials (see Appendix A). (2.11) thus represents a genuine, if ad hoc, generalization. In any case, the \( d\phi \) in (2.11) does indeed behave like a linear differential form; one need only reinterpret \( dx^\alpha \) as covariant vector generated from the coordinates: \( dx^\alpha \).

To isolate the connection coefficients, Weyl assumes that the metric components also change linearly: \( dg_{\alpha\beta} = \frac{\partial g_{\alpha\beta}}{\partial x^s} dx^s \). In the 1918 paper, Weyl does not solve for the connection coefficients completely, but simply isolates them on one side of the equation:

\[
\Gamma_{\alpha\beta\gamma} + \Gamma_{\alpha\gamma\beta} = \frac{\partial g_{\alpha\beta}}{\partial x^\gamma} - g_{\alpha\beta} \phi^\gamma
\]

(2.12)

where \( \Gamma_{\alpha\beta\gamma} \equiv g_{\alpha\delta} \Gamma_{\delta\gamma} \). It is not difficult to show that (2.12) does in fact determine the coefficients uniquely; the resulting connection is the Weyl connection (stated explicitly in (2.16) below).

Finally, Weyl asserts that the connection coefficients should be invariant under gauge transformations: \( g_{\alpha\beta} \rightarrow \lambda g_{\alpha\beta} \). This will be the case if and only if

\[
\phi_\delta \rightarrow \phi_\delta + \frac{1}{\lambda} \frac{\partial \lambda}{\partial x^\delta}.
\]

(2.13)

Thus, if \( \lambda \) is taken to be arbitrary, then the new differential form must also be arbitrary, up to a total differential,

\[
\frac{1}{\lambda} \frac{\partial \lambda}{\partial x^s} dx^s = \frac{\partial (\ln \lambda)}{\partial x^s} dx^s = d (\ln \lambda).
\]
2.3.2. Derivation in Raum, Zeit, Matter, fifth edition

From a conceptual perspective, Weyl’s presentation of the new geometry in the fifth and final (in his lifetime) edition of Raum, Zeit, Materie, published in 1923, is considerably more sophisticated than that in the 1918 article. He introduces the notion of gauge as an integral component of the concept of metricity, thus offering his geometry as the natural—and final—realization of all previous efforts to describe the nature of space.\textsuperscript{17} For Weyl, a “metrical space” is a manifold with a localized notion of distance (or length). The possible measures of distance, or gauges, themselves form a 1-dimensional manifold; if we allow a different copy of this 1-dimensional manifold at each point in the spacetime, then the requirement that these manifolds be smoothly connected, i.e., that local gauges be capable of “parallel” transport, is what produces the new geometry. Because the mathematics is not very different, the conceptual shift from 1918 is easily overlooked; nonetheless, it is important to mark, because it sets the stage for later generalizations, in particular Weyl’s reinterpretation of the gauge as a phase factor in the quantum mechanical wave function, but also the non-Abelian gauge theories that eventually formed the basis for much of 20\textsuperscript{th} century physics. It is also an important step in the creation of the notion of the fiber bundle, a generalization of the differentiable manifold.\textsuperscript{18}

\textsuperscript{17} He writes: “The systematic construction of the pure infinitesimal geometry…takes place naturally in three stages: from the continuum, absent any more exact definition, through the affinely-connected manifold, to metrical space. This theory, in which, in my opinion, a great progression of ideas has succeeded in obtaining its ultimate, definitive form, is a real geometry, a theory of space itself, and not merely a theory of the figures that are possible within space, as was the geometry of Euclid and almost everything else that has passed under the name of geometry” (Weyl 1923, p. 104).

\textsuperscript{18} See Penrose (2004, chapter 15) for discussion of fiber bundles and their relation to modern physics.
Weyl’s treatment of metrical space begins with the declaration that every tangent vector defines a distance [*Strecke*] at a given point and that a corresponding quadratic form (or \((0\ 2)\)-rank tensor) is determined up to some proportionality-factor, or gauge (Weyl 1923, p. 121). Once a measure of the distances is given (i.e., a gauge is fixed), one can put aside the vectors and talk directly of the distances, which together form a one-dimensional “totality” [*Gesamtheit*]. Though Weyl does not use the term “manifold” here, he is in fact describing a particular one—the real number line. A particular distance (or length) can be represented as a point on the real number line, and each gauge is a particular measure of that line. A gauge transformation is then simply a uniform re-scaling of the line, stretching or compressing it by some factor: \( l \rightarrow l' = \lambda l \). As long as the constant factor \( \lambda \) is not zero, the number line, i.e., the set of distance measures on the local tangent and covariant vector spaces, retains its form.\(^{19}\) It is worth noting that Weyl makes a nontrivial assumption here, that the only permissible gauge transformations are the linear ones, where \( \lambda \neq \lambda(l) \); nonlinear transformations also preserve the topology of the number line, but he does not consider them.

The expansion in the geometry can now be visualized, in a way. At each point of the basic manifold, there is attached a tangent vector space *and* a real number line, one that allows “measure-determination” [*Maßbestimmung*] within the tangent space to occur. In order to refer to points along each number line, we must choose a scale (i.e., gauge) for each; for Weyl, this is no different from the need to choose coordinates in order to refer

\(^{19}\) More specifically, its affine form. Multiplication and division, which are well-defined on the number line, are not preserved by gauge transformations.
to the points of the basic manifold. Together, the coordinates and the scales (i.e., gauges) form a “system of reference” [Bezugssystem] (Weyl 1923, p. 123).

Weyl then argues that, just as the manifold should support the mapping of tangent vector spaces at different points, it should do the same for the measure-determination lines. And just as the infinitesimal change in vectors is linear, so, too, should be the change in measures. The infinitesimal change in a measure \( l \) under transport should thus be proportional to it:

\[
dl = -l \, d\phi .
\] (2.14)

The quantity \( d\phi \) is being treated more like a connection here than part of the metric, a sort of “metrical connection,” although Weyl does not use the term (and, in fact, he refers to \( d\phi \) as part of a “world-metric,” not a “world-connection”). Under a gauge transformation, where \( l \rightarrow l' = \lambda l \), the change in the measures are related by

\[
dl' = d(\lambda l) = l d\lambda + \lambda dl .
\]

If (2.14) is to hold for \( l' \) and \( \phi' \), so that \( dl' = -l' \, d\phi' \), then the metrical connection must transform independently of \( l \) according to

\[
d\phi \rightarrow d\phi' = d\phi - \frac{d\lambda}{\lambda} .
\] (2.15)

This is precisely the same rule as (2.13) (up to an unimportant minus sign).

Weyl next argues that in order for the mapping in (2.15) to represent a parallel transport (i.e., one in which the distance measure is kept constant), there must exist at every point a choice of gauge in which the components of the metrical connection vanish

---

\(^{20}\) The minus sign is added for convenience, in analogy with the rule for change in vector components, (2.4).
at that point. He writes that it is “obvious” [offenbar] (Weyl 1923, p. 123) that $d\phi'$ can be made to vanish if and only if $d\phi$ is a linear differential form, just as he does in (2.11). However, as with (2.10), one cannot conclude from this that $d\phi$ is an arbitrary differential form; it could still be merely a total differential, in which case the geometry remains Riemannian (again, see Appendix A).

Weyl relates the metrical connection to the affine connection by considering how the length of some vector $u$, given by $l = g_{\alpha\beta}u^\alpha u^\beta$, changes under transport according to (2.14), where infinitesimal quantities are handled as in Section 2.3.1. This leads to the exact same relation between the $\phi_\alpha$ and the connection coefficients as (2.12). Here, he states the full expression for the connection coefficients:

$$
\Gamma_{\alpha\beta\gamma} = \frac{1}{2} \left( g_{\alpha\beta,\gamma} + g_{\alpha\gamma,\beta} - g_{\beta\gamma,\alpha} \right) + \frac{1}{2} \left( g_{\alpha\beta}\phi_\gamma + g_{\alpha\gamma}\phi_\beta - g_{\beta\gamma}\phi_\alpha \right) \quad (2.16)
$$

where, again, $\Gamma_{\alpha\beta\gamma} = g_{\alpha\delta} \Gamma_{\beta\gamma}^{\delta}$ and $g_{\alpha\beta,\gamma} \equiv \frac{\partial g_{\alpha\beta}}{\partial x^\gamma}$. This is the Weyl connection; it differs from the Riemannian case (2.7) by the additive terms involving the $\phi_\alpha$.

2.3.3. Curvature and electromagnetism

Because he required gauge transformations to be physically meaningless, Weyl needed to find objects invariant under them. The form of the gauge transformations naturally suggested such an object: the curvature tensor associated with the new differential form. It is this tensor that he identified with the electromagnetic field; it satisfies two of Maxwell’s equations by identity, and Weyl was able to show that it would satisfy the other two for any gauge-invariant action principle. In this section, I will
discuss the curvature tensor and its relation to the first two Maxwell equations; discussion of the action principle will follow in Section 2.3.4.

The clearest method for understanding curvature on differentiable manifolds is by way of the exterior calculus, which offers a more general notion of differentiation than that discussed in Section 2.2.2. It is not tied to transport along curves, and is thus capable of producing densities over areas, volumes, and any higher-dimensional region. Conversely, it allows a well-defined notion of integration over regions of arbitrary dimension. The crucial notions for our purposes here are those of the \( p \)-form and the exterior derivative.\(^{21} \)

A \( p \)-form is a tensor of rank \( \binom{0}{p} \) with components antisymmetric in all indices. Given a covariant basis \( \{ \omega^\alpha \} \), an arbitrary \( p \)-form is written

\[
\sigma_p = \sigma_{\alpha_1 \alpha_2 \ldots \alpha_p} \omega^{\alpha_1} \wedge \omega^{\alpha_2} \wedge \ldots \wedge \omega^{\alpha_p}
\]

where \( \wedge \) is the exterior product (or wedge product).\(^{22} \) \( P \)-vectors are defined analogously. Note that all scalar functions are 0-forms and all covariant vectors (i.e., linear differential forms) are 1-forms.

The \textit{exterior derivative} is a way of generating a \( (p+1) \)-form from a \( p \)-form. It is defined recursively, in terms of the \( d \) operator introduced in Section 2.2.1. For the present purposes, its most important property is known as “closure”.\(^{23} \)

\(^{21} \) For a more comprehensive treatment, see Misner, Thorne, and Wheeler (1973, chapter 4) and Penrose (2004, section 12.6). The modern form of the subject dates back to Cartan (1945).

\(^{22} \) The exterior product is a kind of antisymmetrized tensor product that takes a \( p \)-form and a \( q \)-form and produces a \( (p+q) \)-form. It is linear and associative, and can be built up from its behavior with 1-forms: \( \sigma \wedge \rho = \sigma \otimes \rho - \rho \otimes \sigma \). For arbitrary forms, its commutation rule is given by \( \sigma \wedge \rho = (-1)^{pq} \rho \wedge \sigma \).

36
\[ d \left( d \sigma \right) = 0. \]  \hspace{1cm} (2.17)

This expresses the idea that the boundaries of closed regions themselves have no boundaries.\(^{24}\) Integration over a region of a manifold is the inverse of the exterior derivative, a fact expressed neatly by the fundamental theorem of the exterior calculus:\(^{25}\)

\[ \int_{\partial A} d \sigma = \oint_{A} \sigma \]  \hspace{1cm} (2.18)

where \( \partial A \) is the \( p \)-dimensional boundary of the \( (p+1) \)-dimensional region \( A \).

The curvature form, \( R \), combines the exterior and covariant derivatives. If a vector is parallel-transported around a closed curve \( C \) back to its initial position, it will not necessarily map back to itself. The difference will be given by

\[ \oint_{C} \nabla u = \int_{A} d (\nabla u) = \int_{A} \left( R, u \right) \]

where \( \partial A = C \) and \( R \) is defined such that, in the appropriate “slot,”

\[ \left( R, u \right) = d (\nabla u). \]  \hspace{1cm} (2.19)

\( R \) measures the change in a tangent vector that is parallel-transported over an infinitesimally small area, given by some 2-vector. Its components are written

\[^{23}\text{The exterior derivative is defined by this and one other axiom:}\]

\[ d \left( \sigma \wedge \rho \right) = \left( d \sigma \right) \wedge \rho + (-1)^{p} \sigma \wedge \left( d \rho \right). \]

\[^{24}\text{For example, the boundary of a disk is a circle, which itself has no boundary. Likewise, the surface of a sphere has no boundaries. See Misner, Thorne, and Wheeler (1973, chapter 15) for an excellent discussion of this idea.}\]

\[^{25}\text{Note that the old Leibniz-Newton fundamental theorem is just the special case where} \sigma \text{ is a 0-form. Likewise, Stokes’ theorem and Gauss’ theorem are the special cases of 1-forms and 2-forms, respectively.}\]
The relation between $\mathbf{R}$ and the affine connection is fixed by (2.19), independently of any metric that might exist—recognition of this was one of Levi-Civita’s greatest achievements. On a Riemannian manifold, the curvature form is known as the Riemann curvature tensor.

In the fifth edition of Raum, Zeit, Materie, Weyl calculated a similar quantity: the amount that a measure $l$ changes when transported around a closed loop. The change in $l$ on infinitesimal transport, given by (2.14), is $\nabla l = -l \varphi$. By (2.18), the finite change around a closed loop is then

$$\oint_C \nabla l = -\oint_C l \varphi = -\int_A l \, d \varphi.$$ 

Here, the relevant curvature tensor is easy to extract:

$$\mathbf{F} = d \varphi.$$

A gauge transformation simply adds the exterior derivative of some function $f$ to $\varphi$; the contribution to $\mathbf{F}$ is then $d (df)$, which vanishes according to (2.17). In other words, $\mathbf{F}$ is invariant under gauge transformations. Furthermore, because by definition it measures the nonintegrability of length—the amount that the metric changes under transport around a closed loop—its vanishing is the necessary and sufficient condition for the recovery of Riemannian geometry.

---

26 This relation can be expressed compactly by $\mathbf{R} = (d \gamma^\alpha_\beta + \gamma^\alpha_\mu \wedge \gamma^\mu_\beta) \otimes e_\alpha \otimes \omega^\beta$ where $\gamma^\alpha_\beta \equiv \Gamma^\alpha_\beta \omega^\nu$. In component notation, this gives $R^\alpha_{\beta\gamma\delta} = \Gamma^\alpha_{\beta\gamma,\delta} - \Gamma^\alpha_{\beta\delta,\gamma} + \Gamma^\alpha_{\gamma\mu} \Gamma^\mu_{\beta\delta} - \Gamma^\alpha_{\gamma\delta} \Gamma^\mu_{\beta\mu}$. 

38
Because $F$ is derived from a 1-form, (2.17) requires that its exterior derivative vanish:

$$dF = 0.$$ 

In component notation, this is

$$\frac{\partial F_{\alpha\beta}}{\partial x^\gamma} + \frac{\partial F_{\alpha\gamma}}{\partial x^\beta} + \frac{\partial F_{\beta\gamma}}{\partial x^\alpha} = 0.$$  \hspace{1cm} (2.20)

As it happens, (2.20) is formally identical to the homogenous Maxwell equations. Indeed, Maxwellian electrodynamics attains its most elegant form with the exterior calculus. Because $F$ is antisymmetric, it has only six degrees of freedom (in four dimensions). If we identify three of these with the electric field components, $F_{i0} = E_i$ ($i$ goes from 1 to 3), and the other three with the magnetic field components, $F_{12} = B_3$, $F_{23} = B_1$, and $F_{31} = B_2$, then the desired laws, Faraday’s law and the prohibition of magnetic monopoles, follow directly from (2.20):

$$\tilde{\nabla} \times \vec{E} + \frac{\partial \vec{B}}{\partial t} = 0$$

$$\tilde{\nabla} \cdot \vec{B} = 0$$

where $\vec{E}$, $\vec{B}$, and $\tilde{\nabla}$ have their traditional vector-notation meanings. (2.20) must hold regardless of whatever field law is also postulated; it is for this reason that Weyl concludes that the homogenous Maxwell’s equations are identities. They arise simply from the closure of the exterior derivative and the requirement that vector lengths be nonintegrable.
The overall curvature form in Weyl’s theory differs from that of Riemannian geometry, but the two are related in a simple way:\textsuperscript{27}

\[ \mathbf{R} = \mathbf{\bar{R}} - \frac{1}{2} \mathbf{1} \otimes \mathbf{F} \]  

(2.21)

where \( \mathbf{R} \) is the curvature tensor in Weyl’s theory and \( \mathbf{\bar{R}} \) is the curvature tensor of Riemannian geometry (i.e., the Riemann tensor), now defined explicitly by its relation to the metric tensor. In component notation, (2.21) reads:

\[ R^\sigma_{\rho\alpha\beta} = \bar{R}^\sigma_{\rho\alpha\beta} - \frac{1}{2} \delta^\sigma_{\rho} F_{\alpha\beta} \]  

(2.22)

2.3.4. Action principles, electromagnetism, and the Einstein field equation

Weyl’s derivation of the inhomogeneous Maxwell’s equations is considerably more complicated than that of the homogeneous. It involves taking the variation of an action magnitude, the preferred method at the time for deriving laws of nature. The main point of arbitrariness in the method is the selection of the action magnitude, and the typical strategy is to justify the choice by simplicity considerations. Weyl was able to show that a wide variety of action magnitudes do reproduce the remaining Maxwell equations, as well as close approximations of the Einstein field equation of general relativity, but, to his great disappointment, he was unable to show that simplicity considerations picked out any of them uniquely. This prevented him from using the theory to make any definite experimental predictions, which surely was a factor in his subsequent abandonment of the theory. I will not attempt to reproduce Weyl’s reasoning

\textsuperscript{27} Goenner (2004, p. 36) claims that Weyl actually miscalculated the curvature tensor and that the correct, considerably more complicated expression was given by Schouten (1954).
regarding the action principle in great detail, but rather will sketch it in broad outline in order to indicate both what excited and disappointed him about it.\textsuperscript{28}

To generate an equation from an action principle, one constructs a scalar quantity (the action) from the dynamical quantities and then finds the conditions needed to restrict the scalar to an extremum (a maximum or minimum) with respect to variations in those dynamical quantities, the thinking being that nature always tries to maximize or minimize \textit{something}. The trick (and a trick it is) is to find the right action and the right dynamical quantities to produce the desired equations. Weyl’s approach was to assume that the action is a function of the components of the “world-metric” (the tensor fields $\varphi$ and $g$ taken together) and that it should be invariant under gauge-transformations; from these assumptions alone, he derives equations similar in form (though not identical) to Maxwell’s equations (Weyl 1923, p. 308-314). Through a rather complicated series of steps,\textsuperscript{29} Weyl concludes that there must be a tangent vector field $s$ and a 2-vector field $h$,\textsuperscript{30} both derived from the action, such that

\[
 s^\alpha - \frac{\partial h^{\alpha \beta}}{\partial x^\beta} = w^\alpha \tag{2.23}
\]

where $w^\alpha$ is the coefficient arising from varying the action with respect to $\phi_\alpha$. If $\phi_\alpha$ represents a truly independent quantity (a non-trivial claim), then $w^\alpha$ will have to vanish.

\textsuperscript{28} Further discussion of the action principle can be found in Section 3.2, where I consider Weyl’s attempts to explain the behavior of measuring rods and clocks by locating a “natural gauge of the world.”

\textsuperscript{29} Brading (2001, p. 130-131) calls Weyl’s reasoning here an instance of the “Klein-Utiyama Theorem,” which can be understood as an extension of Noether’s second theorem.

\textsuperscript{30} Technically, these are tensor-densities, not tensor fields. Each term includes a factor of $\sqrt{-|g|}$, which I have suppressed here.
If we identify $h$ with the electromagnetic field\textsuperscript{31} and $s$ with the charge 4-current and enforce the condition $w^\alpha = 0$, then (2.23) expresses the inhomogeneous Maxwell equations (Coulomb’s law and the Ampere-Maxwell law):

$$\frac{\partial F^{\alpha\beta}}{\partial x^\beta} = J^\alpha.$$  \hspace{1cm} (2.24)

This leads Weyl to declare, in the fourth edition of *Raum, Zeit, Materie*: “Without specialising the *Action* at all we can read off the whole structure of Maxwell’s Theory from the calibration [gauge] invariance alone” (Weyl 1922, p. 292, original emphasis).\textsuperscript{32}

An important implication of (2.23) is an equation expressing conservation of charge:

$$\frac{\partial J^\alpha}{\partial x^\alpha} = 0.$$

The intimate connection between this law and the requirement that the action be gauge-invariant impressed Weyl greatly. As he points out, one need not even assume that $w^\alpha = 0$; one can assume instead that the equations produced by varying the $g_{\alpha\beta}$ hold true. Each is sufficient, a consequence of gauge-invariance.\textsuperscript{33}

As exciting as these results were, they also had to be somewhat disappointing; they implied that, given a straightforward interpretation of the field quantities, the theory

\textsuperscript{31} The 2-vector field that appears in the inhomogeneous Maxwell equations is not the same object as the 2-form appearing in the homogenous equations. The two are related by contraction with the metric tensor and the Hodge dual operation: $F^{\alpha\beta} = g^{\alpha\mu} g^{\beta\nu} F_{\rho\sigma} \epsilon_{\rho\sigma\mu\nu}$.

\textsuperscript{32} Interestingly, even though this derivation remains largely intact in the fifth edition, this particular comment does survive (Weyl 1923, p. 314).

\textsuperscript{33} Brading (2001, sections 5.4 and 5.5) shows that Weyl’s derivation of the conservation law is a novel use of Noether’s Second Theorem; the novelty lies in his enforcing one set of the field equations produced by varying the action in order to derive the conservation equation. See also Brading (2002).
could not produce any deviation from Maxwellian electrodynamics, which would have been the most direct route to experimental test. However, some hope remained on the gravitational side, as the field equations generated by varying the $g_{\alpha\beta}$ did in fact depend on the form of the action. What remained, then, was to determine exactly what that form should be. Weyl required that the quantities appearing in the action be “rational” (Weyl 1923, p. 316), by which he meant gauge- and coordinate-invariant scalar quantities created from $\phi$ and its 1st order derivatives and $g$ and its 1st and 2nd order derivatives. Others had shown that there were only six linearly independent such quantities, three of which vanished when varied.\textsuperscript{34} Of the remaining three, Weyl pays significant attention only to two, the curvature scalar and the square of the electromagnetic field tensor:

$$R \equiv g^{\beta\delta} R^\alpha_{\beta\alpha\delta}$$

$$L \equiv F_{\alpha\beta} F^{\alpha\beta}.$$ (2.25)

Weyl does use a third invariant at times, the square of the curvature tensor,

$$|R|^2 \equiv R^\alpha_{\beta\gamma\delta} R_\alpha^{\beta\gamma\delta} = |\bar{R}|^2 + \frac{1}{4} L,$$ (2.26)

but it turns out to be a linear combination of the two in (2.25) plus one of the quantities whose variation vanishes, as it does not produce different field equations. The third linearly independent invariant includes some contribution from the square of the generalized Ricci tensor:

$$R_{\alpha\beta} R^{\alpha\beta} = R_{\alpha\beta\gamma\delta} R^{\alpha\beta\gamma\delta}.$$ 

\textsuperscript{34} According to Weyl (1923, p. 316), Weitzenböck (1920) showed that there were six invariants, but that two required the manifold to evince a kind of preferred chirality. Bach (1921) showed that the variation of both of those chiral invariants vanished, as did a specific linear combination of the other four.
Weyl notes that it produces gravitational equations that make the equivalence principle impossible (by producing forces proportional to something besides the mass)\textsuperscript{35} (Weyl 1923, p. 305), so that it cannot be included in the action for empirical reasons, although he later laments that he can find no “cogent \textit{stichhaltig} reasons for why nature has spurned the use of the third invariant” (Weyl 1923, p. 317).

Weyl uses a linear combination of the invariants in (2.25) to derive modified gravitational equations; they turn out to be 4\textsuperscript{th}-order in the derivatives of \(g_{\alpha\beta}\), unlike the Einstein field equation, which is only 2\textsuperscript{nd}-order. He initially took this to be an advantage of the theory, since it allowed for potential empirical differentiation from Einstein’s theory. However, the 4\textsuperscript{th}-order equations were ultimately too difficult to work with, in part due to complications with specifying boundary conditions, and the solutions that were found did not diverge from those of the Einstein equation very much, if at all.\textsuperscript{36} In the end, Weyl would use gauge-fixing techniques to attempt to simplify the boundary value problem, effectively reducing the equation to 2\textsuperscript{nd}-order, but also eliminating its predictive novelty.\textsuperscript{37} Those techniques would also play a role in his response to Einstein’s objection that the theory was empirically disconfirmable (see Sections 3.2 and 4.2).

In general relativity, the derivation of the Einstein field equation by the variational principle is relatively simple; one varies the curvature scalar, the simplest

\textsuperscript{35} He later (p. 317) attributes this result to Pauli (1919).

\textsuperscript{36} Pauli (1919) found that the Schwarzschild solution to the Einstein field equation was also a solution of Weyl’s, so that the precession of the perihelion of Mercury and the bending of light rays would also be predictions of Weyl’s theory.

\textsuperscript{37} For more on Weyl’s later attempts to massage the action magnitude, see Vizgin (1994, p. 129-137) and Ryckman (2005, p. 163-166).
scalar invariant in Riemannian geometry, by the connection coefficients. The
c omparative complexity regarding the action principle in his own theory was
discouraging for Weyl; his arguments concerning it change from edition to edition of

Raum, Zeit, Materie, never reaching a completely satisfying form.

2.4. Aftermath

2.4.1. Einstein’s objection

As mentioned above, the reception of the theory among physicists was generally
negative, owing partly to the circumstances of the publication. Weyl sent the original
paper to Einstein for presentation to the Prussian Academy of Sciences, in hopes of
publication in their Proceedings. Einstein found the theory brilliant, calling it “a terrific
achievement of the mind” (Einstein to Weyl, 8 April 1918 [CPAE 8, Doc. 499]).
Nevertheless, he quickly found reasons to object to it, which he communicated to the
Academy immediately after presenting Weyl’s paper. The Academy members then
refused to publish Weyl’s paper in the Proceedings unless Einstein’s objection was
appended. Weyl was given space in the publication to respond, but it was of little use.
Einstein was already regarded as one of the preeminent physicists of the day (if not the
single most) and Weyl’s theory clearly still needed some work.

Einstein’s primary objection was that the path-dependence of length was
empirically disconfirmable (Einstein 1918, p. 40). The spacetime metric can be
measured directly, he argued, by rigid rods and clocks. If lengths were nonintegrable,
then rigid bodies of the same length should not remain so if separated and reunited, and
ideal clocks running with the same period should not continue do so if separated and reunited. Since atoms are, effectively, clocks whose period determines the frequency of light they emit and absorb (so the argument goes), the path-dependence of length can be tested empirically by examining emission and absorption spectra of excited atoms. If their periods are non-integrable, then atoms of the same type will in general emit or absorb light of different frequencies, depending on their individual histories. But when we look at stellar emission and absorption spectra, we see sharp lines, indicating that prior history is irrelevant.

Weyl responded by essentially breaking the strict link between rigid bodies and tangent vectors implicit in Einstein’s claim that rulers and clocks can measure the metric directly. The behavior of such objects should be determined by the dynamics of the theory (derived from an action principle, of course), he argues, not by stipulation. By 1920, he had developed a conceptual distinction that would aid him in making this argument; quantities in nature could be determined in two ways, he argues, either through adjustment or persistence. Objects such as measuring rods and clocks, or even atoms, might have internal mechanisms that adjust to local conditions, allowing their macroscopic properties to appear constant. Weyl found this distinction useful enough that he continued to advocate it long after he had abandoned his unified field theory, indeed, all the way to the end of his life. His development of the distinction between persistence and adjustment and its employment in the debate with Einstein is detailed in Chapter 3.

Einstein also objected in a way that anticipated Weyl’s response. He argues that the metric has “real significance” and that without the connection to measuring rods and
clocks, “the theory of relativity loses its empirical basis entirely” (Einstein to Weyl, 15 April 1918 [CPAE 8, Doc. 507]). Weyl responds to this by further developing the idea that, in a complete, systematic theory, the behavior of complex objects should be derived, not stipulated. In the well-known lecture, “Geometry and Experience” (1922), Einstein seems both to reiterate this objection and to agree with Weyl that assumptions about rigidity should only be employed provisionally. In Chapter 4, I investigate this aspect of the objection, Weyl’s responses, and Einstein’s apparent equivocation, ultimately concluding that the two were in fact in agreement on this point and that Einstein’s concerns about Weyl’s theory are better (and self-consistently) understood in methodological terms.

In the end, the debate between Einstein and Weyl over the 1918 theory was given up before it was won. By the end of the 1920s, Weyl had abandoned his theory, though for reasons having to do primarily with quantum mechanics, not Einstein’s objection. Somewhat ironically, Einstein put aside his concerns about how rigid bodies should be connected to the metric and spent the rest of his life ardently pursuing a geometric unified field theory, ultimately without success (see fn. 13 above).

2.4.2. Weyl and quantum mechanics

Why, exactly, did Weyl abandon his field theory? His stated reasons are clear, although not entirely satisfactory. What he says is that quantum mechanics provides an absolute standard of length, thereby disproving the principle of relativity of magnitude:

By this new situation, which introduces an atomic radius into the field equations themselves—but not until this step—my principle of gauge-invariance, with
which I had hoped to relate gravitation and electricity, is robbed of its support. (Weyl 1929b, p. 233)\textsuperscript{38}

The Compton wavelength $\frac{h}{mc}$ appears in the Dirac equation (as Weyl writes it), and since the equation holds everywhere in space, the quantity serves as a globally accessible unit of length. The curious thing about Weyl’s willingness to accept this as a disconfirmation of his theory is that he had an alternative explanation ready at hand; he could have argued that the Compton wavelength (or Planck constant) could be determined through adjustment, just as he did with the electron’s mass and charge. He even comes close to making this argument just the year before, in the first edition of his remarkable *Philosophie der Mathematik und Naturwissenschaft* (Weyl 1927, p. 102). One possibility is that he had already come to view the Planck constant as much more fundamental than the constants tied to particular particles, making it easier to imagine some substructure determining the latter but not the former. However, given his other pronouncements about constants of nature, that they should not appear in a systematic theory (see Section 5.2.3), this is rather unlikely.

The more likely explanation is that by 1929 he had simply moved on. His field theory program had not turned out to be very fruitful, in part due to the difficulties with field laws discussed in the previous section, and he was not impressed with the directions in which Einstein and Eddington had taken the research. On the other hand, he was very impressed with the new quantum mechanics, both in its nascent formalism and its many empirical successes. By 1929, he had found a way to incorporate the mathematics of

\textsuperscript{38} A similar statement can be found in Weyl (1929c, p. 218).
gauge-invariance into quantum mechanics, and this left him more than happy to call it a
day and move on. It is precisely then that he officially threw in the towel on his unified
field theory and the principle of the relativity of magnitude.\textsuperscript{39}

What Weyl was able to show was that the Dirac equation for an electron in an
electromagnetic field is invariant under the substitutions

\begin{align}
\psi \rightarrow \psi' &= e^{i\lambda} \psi \\
\phi_\alpha \rightarrow \phi'_\alpha &= \phi_\alpha - \frac{\partial \lambda}{\partial x^\alpha}
\end{align}

(2.27)

where \(\psi\) is the wavefunction representing the electron, \(\lambda\) is any arbitrary function of
spacetime, and \(\phi_\alpha\) are the components of the electromagnetic 4-potential. The
transformations in (2.27) are formally identical to those in his field theory—compare to
(2.13) (or, even better, to (A.4))—with one, crucial difference: the scale factor becomes
imaginary. This greatly complicated the physical interpretation of the transformations;
certainly they could not be viewed as changes of measuring units. However, Weyl kept
the word “gauge,” and it stuck. From (2.27), one can, just as before, construct a gauge-
invariant 2-form by taking the exterior derivative on the \(\phi_\alpha\); it will, again, satisfy the
homogeneous Maxwell’s equations by identity. As long as the Lagrangian that produces
the Dirac equation is gauge-invariant (and includes the free-field term, \(F_{\alpha\beta} F^{\alpha\beta}\)), the

\textsuperscript{39} There is some tension in Weyl’s later pronouncements on geometrization projects in physics. In
Weyl (1931), he reaffirms the belief that the space of intuition is correctly represented by a tangent plane
on a curved surface (p. 341), though there is no reassertion of the relativity of vector magnitude. Later in
the same article, however, he argues against the geometrization of physics in general, due to Heisenberg’s
uncertainty principle and the dominance of the action quantum in the infinitely small (p. 345). The
apparent failure of causality at the micro-scale and the consequent need for a statistical description might
certainly have played a larger role in Weyl’s turn away from his unified field theory than I suggest here.
Sieroka (2007, p. 91) indicates that it does, though further investigation is required for a definitive answer.
inhomogeneous equations follow as well. The Dirac equation will in fact be gauge-invariant if the partial derivative is also transformed:

\[
\frac{\partial}{\partial x^\alpha} \rightarrow \frac{\partial}{\partial x^\alpha} + i \frac{e}{\hbar} \phi_a
\]

This substitution is the first example of what comes to be called the “gauge principle,”\(^{40}\) according to which invariance of the matter fields under local symmetries requires the introduction of force-carrying fields, which then couple to the matter fields by modifying the spatiotemporal derivative.\(^{41}\)

It should be stressed that Weyl was motivated here purely by formal considerations and not by lofty epistemological concerns about connecting spaces of intuition. Nevertheless, the Dirac equation enjoyed empirical support in a way his theory never did; he thus concluded, no less confidently than before:

> It seems to me that this new principle of gauge-invariance, which follows not from speculation but from experiment, tells us that the electromagnetic field is a necessary accompanying phenomenon, not of gravitation, but of the material wave-field represented by \(\psi\). (Weyl 1929a, p. 122)

What Weyl had done was to grope his way into a mathematical space that had enormous capacity for generalization, one where electromagnetism appears merely as one of the simplest cases. This is the space of fiber bundle theory, and it undergirds much of modern physics. A fiber bundle is essentially a differentiable manifold with a different

\(^{40}\) O’Raifeartaigh (1997, p. 16-17, 81) refers to it as the “minimal principle,” since it goes beyond gauge-invariance in ruling out interactions with the higher-order field tensor, \(F_{\alpha\beta}\).

\(^{41}\) It should be noted, however, that the use of the principle is not sufficient to generate complete theories of forces, since self-interaction terms must be added separately (e.g., \(F_{\alpha\beta} F^{\alpha\beta}\) in the case of the electromagnetic field).
copy of another differentiable manifold (the fiber) attached to each point. The plain, old differentiable manifold is a simple example, the tangent spaces serving as the fibers. As Section 2.3.2 makes clear, Weyl geometries are like Riemannian manifolds with an extra number line stuck to each point—just a particular kind of fiber bundle. Different geometries can be formed simply by changing the fiber, which is essentially what Weyl does when he reinterprets gauge transformations as a symmetry of the wavefunction; instead of a number line at each point, the fiber is a closed circle. The local topology of the line and circle are the same, which is why the form of the gauge transformation is so similar. Though it would be more than 20 years before anyone would exploit the opportunity for generalization opened up by Weyl—Yang and Mills (1954) were the first—most of the major physical theories produced in the second half of the 20th century, electroweak theory and quantum chromodynamics, in particular, are quantized field theories over fiber bundles, formulated using Weyl’s gauge principle.

2.5. Conclusion

Weyl’s theory was unquestionably revolutionary, though in a different way from its predecessors; it did not itself successfully introduce new physical concepts or deconstruct old ones, as had Newtonian mechanics, relativity, and quantum mechanics. Instead, it expanded the sphere of a particular set of mathematical ideas and offered

\[ e^{i\lambda} = e^{i(\lambda + 2\pi)} \]

42 The new gauge \( \lambda \) measures a circle rather than a line thanks to the fact that \( e^{i\lambda} \) is cyclic. That is, \( e^{i\lambda} = e^{i(\lambda + 2\pi)} \).

43 For more on the use of the gauge principle in modern physics and the philosophical issues raised by it, see Brading and Castellani (2003), in particular Martin (2003), Earman (2003), and Redhead (2003).
suggestive hints as to how those new ideas could be fruitful for physics. Weyl himself
would take the first steps in cultivating that fruit, though others would eventually be the
ones to reap its full harvest. His original field theory would be largely forgotten, except
as an historical footnote as the birthplace of gauge theory; its epistemological motivations
would be buried even deeper.

The theory was also the first credible attempt at total unification, what now goes
by the name “theory of everything.” As such, it raised important philosophical questions
about how a theory that purports to describe everything in existence should be formulated
and connected to experiment. These questions lie just under the surface of Weyl’s debate
with Einstein, and they will be examined in the chapters that follow.
CHAPTER 3:
PERSISTENCE AND ADJUSTMENT

As discussed in the previous chapter (see Section 2.4.1), Einstein’s primary objection to Weyl’s unified field theory was that it could be easily disconfirmed. If clocks and rods measure $g$, he argued, then the theory implied that the rates of clocks and the lengths of rods would depend on the objects’ paths through spacetime, i.e., on their prior histories. Consequently:

If this were really so in nature, then there could not be chemical elements with spectral lines of determinate frequencies, but rather the relative frequency of two (spatially contiguous) atoms of the same kind must in general be different. (Einstein 1918, p. 40)

However, when we examine the spectral frequencies of atoms, especially those in far away galaxies, we find that they are all definite, depending only on the type of atom and not on its prior history. Thus, Weyl’s theory cannot be correct. Weyl’s initial response is simply to deny the claim that rods and clocks measure $g$; he argues that such behavior should be derived from the dynamical laws, not stipulated.\(^{44}\)

Beginning in 1920, with a brief note to *Physikalische Zeitschrift*, Weyl introduces a new element into his defense against the charge of empirical disconfirmation. He

\(^{44}\) See Chapter 4 for more on the debate that ensued between Weyl and Einstein on this point, and see Chapter 6 for further discussion of its broader epistemological consequences.
outlines a possible mechanism for how physical objects might fail to display path-
dependence of length and clock rate, even if the underlying geometry embodies the
principle of the relativity of vector magnitude. He argues that the fundamental
constituents of physical objects might have properties that adjust to the local conditions
of the underlying metrical field, in such a way that some macroscopic properties of the
object, such as its length, appear fixed as it is transported, even as the geometric
properties of the constituents undergo transport-related shifts. This line of argument
appears in print several more times, although it is not until the 5th edition of Raum, Zeit,
Materie that Weyl provides a full, technical clarification along with a complete model of
an actual adjustment mechanism. Prior to that, his reasoning is analogical and more
suggestive than compelling.

Weyl continued to advocate the usefulness of this distinction to the end of his life,
long after he had abandoned his unified field theory. He argued that the fact that
fundamental particles have the same properties in all places and at all times suggests that
there is some adjustment mechanism underlying their determination; indeed, the Higgs
mechanism of the Standard Model could be viewed as a somewhat imperfect realization
of just this idea. In this chapter, I examine Weyl’s development of the distinction
between persistence adjustment, cataloguing its appearances in his work, analyzing its
development over time, and considering it as a response to Einstein’s objection. I
ultimately conclude that the distinction has merit but is ultimately more of a promissory
note on future physics than a fully-developed, successful response to Einstein.
3.1. The gyroscope and the compass

Weyl first introduces adjustment by distinguishing its opposite, persistence, and offering paradigms for both:

A magnitude in nature can be determined through persistence or through adjustment. Example: one can assign an arbitrary initial direction to the axis of a rotating gyroscope, however, if the gyroscope is left alone, this then carries on [sich übertragen] through an effective tendency to persist from moment to moment (parallel transport); in contrast, the direction of a magnetic needle in a magnetic field is determined through adjustment. Whereas the affine and metric connections determine a priori how vectors and lengths vary if they exclusively follow a tendency to persist, the charge of an electron, atomic frequencies, and the length of a measuring rod are determined through adjustment. (1920, p. 141)

The distinction between the persistence and adjustment of a physical quantity appears to rest on two seemingly independent features: a magnitude’s capacity to be modified arbitrarily, and its ability to “carry on.” A quantity must be arbitrarily modifiable and must carry on in order to be determined exclusively through persistence. The sense of persistence invoked here is not that the state of the persisting system should remain unchanged, but rather that past states should have some impact on future states. Put in functional terms, a persisting quantity is determined in part by its values at previous times, and adjusting quantities are independent of their values at previous times.

We should be careful to distinguish an adjusting quantity, which must vary in accordance with changes in its environment, from a quantity determined through adjustment, which need not manifest such changes. To say that a quantity is determined through adjustment is to say that the quantity depends on some mechanism that itself varies dynamically in order to maintain some fixed relationship with the local environment. Weyl generally refers only to quantities determined through adjustment, and for good reason. A full description of the adjustment mechanism for these quantities
would require new physics. For example, the electric charge of a fundamental particle, one of Weyl’s oft-cited examples of a quantity determined through adjustment, does not adjust to anything as the particle moves—it does not change at all. Instead, the charge is determined by *something else* that itself adjusts to local conditions in such a way that the charge appears to be held fixed. One way to model this would be to introduce a second quantity, call it the “subcharge,” that adjusts to the local value of some field in some fixed ratio. The (heretofore unknown) physics of the subcharge and its interaction with electromagnetism would then be such that this ratio is what describes the strength of the particle’s coupling to the electromagnetic field, i.e., its charge. Thus, the observed quantity, the charge, is not what is doing the adjusting, though it is determined by something that is. Of course, the underlying, adjusting system might be much more complex than this scalar-to-scalar ratio; the important point is that there is a subsystem that maintains some kind of fixed relationship with the local environment. The specifics of the relationship might change as the system moves, but the fact that the subsystem maintains such a relationship does not.

In the case of the magnet needle, the observed quantity, the needle’s direction, is the adjusting quantity. There is thus no need to introduce an unobserved subsystem with its own adjustment mechanism. On the other hand, we might imagine a scalar quantity determined by the needle’s direction, say, the exponential of its angular separation from the local magnetic field, and suppose that this quantity measures the coupling strength of the needle to some new field. If the needle is perfectly adjusting, then this coupling will appear to be of constant strength, and we will say that it is determined through adjustment. Likewise, the lengths of measuring rods and the periods of ideal clocks do
not themselves adjust, but rather they are determined by quantities that do. Because these are complex, compound objects, there is probably no simple “sublength” or “subperiod” that is doing the adjusting; rather, the observable properties are likely determined by complicated adjusting properties of the constituent fundamental particles.

For understandable reasons, Weyl is vague about what sort of mechanism might underlie the determination of length, charge, and period through adjustment. Clearly, such a mechanism is needed, not only for composite objects, but also for the properties of fundamental particles, where adjustment implies the existence of new physics. It might be helpful in a descriptive way to say that electric charge or rod length is determined through adjustment, but without any understanding of why that should be the case, the distinction is of little explanatory power. If Weyl realized this in 1920, he chose not to say so. The possible mechanism he does offer is a mere reduction to Riemannian geometry: “The theoretical possibility of a determination of length through adjustment is given by a natural, perfect, uniform gauge over the entire world, for which the world-curvature becomes R=const” (1920, p. 141). It is not until 1923 that he produces a fully-specified model of an adjusting quantity, although it is not one that directly addresses measuring rod length or clock frequency.

Finally, the choice of magnet needle as a paradigm of an adjusting system is somewhat peculiar. While it is easy to see how Weyl wishes the needle to behave—by adjusting to the local magnetic field in a fixed way—no actual needle will behave that way. In fact, it is not possible to design such a needle even under ideal circumstances. If a magnet needle is made to line up with the magnetic field at its location and then is moved to a place where it does not match up with the field, it will not immediately adjust.
its orientation to do so. Instead, it will experience an impulse toward being aligned, which will cause it to oscillate back and forth around the direction of the field. If there is kinetic friction, the motion will be damped so that the oscillations shrink over time, asymptotically approaching alignment with the field. Only if static friction (or some other barrier) is present will the motion stop in a finite amount of time, and in that case the needle will almost certainly come to rest out of alignment with the magnetic field. Thus, no actual or ideal magnet needle will adjust exactly in the way Weyl suggests. At this stage, it is best to think of the example playing a heuristic rather than paradigmatic role.

3.2. Constitution and equilibrium

In 1921, Weyl published three papers that included discussion of the distinction between adjustment and persistence. Two of them, “Feld und Materie” and “Über die physikalischen Grundlagen der erweiterten Relativitätstheorie,” were published in German and were meant as companion pieces. The third, “Electricity and gravitation,” is a short piece likely intended to publicize the main results of the unified field theory to a wider, English-speaking scientific audience. It contains passages that appear to be more or less directly translated from “Feld und Materie.” The three works discuss adjustment and persistence in similar terms and will be taken together to represent a snapshot of Weyl’s thinking on the matter in 1921. Interestingly, the 4th edition of Raum, Zeit, Materie, also published in 1921, presents a more developed picture of the distinction; this will be discussed in the following section.
The 1921 articles correlate persistence and adjustment to the mathematical notion of integrability, the vanishing of the integral of a quantity over a closed loop in space. An object moving along a closed path will find its integrable quantities returning to their initial values as it reassumes its initial location, no matter what path it takes. Likewise, two objects departing from the same point and then reconvening elsewhere, thus forming a closed loop, will find that their initially equal integrable quantities, though perhaps changed, will still be equal. However, their initially equal non-integrable quantities will likely differ, in ways that depend on the particular path each had taken. In a spacetime context, non-integrability is an elegant way of capturing the idea discussed in the previous section, that a quantity should be determined at least in part by values at earlier times. A non-integrable quantity, if it can also be modified arbitrarily, must therefore be determined through persistence. Conversely, a quantity determined through adjustment must be integrable. However, a quantity determined through persistence might still be integrable, if the physics conspires to make it so. For example, the direction of a gyroscope in Euclidean space can be set arbitrarily, and it is certainly “carried forward.” But it is also integrable. The key difference, Weyl writes, is in what we can know a priori about the behavior:

We have no reason to assume a priori that a transfer [Verpflanzung] exclusively following the tendency to persist will be integrable, i.e., independent of the path on which the transfer takes place. (1921b, p. 258)

45 This is a sensible claim only for curves that are everywhere timelike. Non-integrability is well-defined for spacelike loops (and for those that are only spacelike along some segments), but for these there is no objective notion of “earlier time.” Since physical objects cannot traverse such paths, however, this complication is of little consequence here.
On the other hand, adjusting quantities are determined entirely by local conditions, and thus we can know a priori that they will be integrable. These implications can be summarized as follows:

\[(\text{Non-integrable} \& \text{ arbitrarily modifiable}) \rightarrow \text{Determined through persistence} \]

\[\rightarrow \text{Determined through adjustment} \rightarrow \text{Integrable}\]

The fact that quantities determined through persistence can be integrable enables Weyl to present a stronger justification for why we should believe that certain quantities are determined through adjustment:

Even if that is the case [that a persisting quantity is integrable], as, for instance, for the rotation of the top in Euclidean space, we should find that the two tops which start out from the same point with the same axial positions and encounter again after the lapse of a very long time would show arbitrary deviations of their axial positions, for they can never be completely isolated from every influence. Thus, although, for example, Maxwell’s equations demand the conservation equation \(\frac{de}{dt} = 0\) for the charge \(e\) of an electron, we are unable to understand from this fact why an electron, even after an indefinitely long time, always possesses an unaltered charge, and why the same charge \(e\) is associated with all electrons. (1921a, p. 261)

Even if a persisting quantity is integrable in the ideal case, such as an isolated gyroscope in Euclidean space, we will not actually observe it to be so, due to our inability to realize the ideal case fully. In other words, a persisting, integrable quantity should be \textit{perturbable}, which at first glance appears to be merely a rebranding of the earlier notion of arbitrary modifiability. A quantity determined through adjustment, on the other hand, though integrable, should be imperturbable. Maxwell’s equations imply the conservation of electric charge, which in turn implies that charge is integrable. But the charge of a macroscopic body will not generally remain constant as the body moves around, due to interactions with its environment; i.e., the charge will be perturbed. On the other hand,
the charge of an individual electron is always the same; it appears to be imperturbable. Therefore, it must be determined through adjustment. Likewise for the electron’s rest mass and for atomic spectra. Of course, such an argument says nothing about the underlying adjustment mechanism, and therefore nothing about why we should expect these quantities to be determined through adjustment in the first place. But it does give good reason to suspect that the charges of fundamental particles and macroscopic bodies are determined through fundamentally different mechanisms, i.e., that the distinction between persistence and adjustment, or something close to it, really is reflected in nature.

The 1921 papers do provide some ways of characterizing the features of systems that allow their properties to be determined through adjustment. Weyl writes this about a magnet needle:

Its direction is determined at each instant independently of the condition of the system at other instants by the fact that, in virtue of its constitution, the system adjusts itself in an unequivocally determined manner to the field in which it is situated. (1921a, p. 261)

It is the constitution of a system that causes its properties to be determined through adjustment. In order for an electron to prevent its charge from being determined through adjustment, it must cease being an electron. For Weyl, this explains why all electrons have the same charge and shows that there is some kind of unique “state of equilibrium of the negative electricity.” The term “equilibrium” is a refinement of the notion of fixed relationship discussed above; it suggests that the mechanism that determines the electron’s charge can be described as a dynamical balancing between two ontologically distinct entities or structures. Furthermore, equilibria tend to be settling points reached after a non-zero amount of time following displacement from an equilibrium point,
exactly in the way that a real magnet needle behaves. Weyl’s use of the term in reference to electric charge raises the possibility that he means to draw the analogy with the magnet needle quite literally—suggesting, perhaps, that an electron’s charge could in fact oscillate around an equilibrium point much as a magnet needle would. Unfortunately, he does not pursue the point here, although the 4th edition of Raum-Zeit-Materie contains indications that he might have been thinking in this way (see Section 3.3). On the other hand, he does recognize that talk of an object’s constitution is really a promissory note for a more fundamental description of some internal physics: “Such things as the behavior of charge under the influence of the electric field …must proceed as consequences of the developed [fundamental] theory” (1921b, p. 259). He does not provide a “developed theory” here, although he does hint at a proper framework for one in “Feld und Materie.”

The discussion of magnet needles and electrons is meant merely to introduce the notion of determination through adjustment; Weyl ultimately wants to show that the lengths of measuring rods are also determined in this way. As manifestly complex objects, measuring rods are quite different from electrons, however, and so he conspicuously shifts strategies, emphasizing the lack of arbitrary modifiability:

The length of a measuring-rod is obviously determined by adjustment, for I could not give this measuring-rod in this field-position any other length arbitrarily (say double or treble length) in place of the length that it now possesses, in the manner in which I can at will predetermine its direction. (1921b, p. 259)

The shift is needed because, unlike the charge of a fundamental particle, the length of a measuring rod seems to be perturbable. Even in a Riemannian spacetime, two congruent measuring rods will most likely not have the same length upon being reunited after a
separation, due to interactions with the environment (weathering, sublimation, etc.), just like the charge of a macroscopic body. Rod length and macroscopic charge do not appear to be arbitrarily modifiable, however, and so there seems to be a difference between perturbability and arbitrary modifiability. However, upon closer scrutiny, this difference becomes rather murky, especially when we consider questions of identity and constitution.

The charge on a macroscopic body can be perturbed only if it gains or loses particles; one could fairly wonder whether such a change alters the body’s identity, in which case it would no longer be the same body. If this were the case, then the charge of a macroscopic body would in fact be imperturbable—it could not be changed without the body ceasing to be itself. Only properties that do not depend in an essential way on a body’s constitution could be perturbable and hence determined through persistence. Weyl’s emphasis on “this measuring-rod” is evidence that he thought this way. However, it is not obvious that a rod’s length depends essentially on its constitution in the same way, since its length can change without the rod gaining or losing any particles (e.g., if it is heated or mechanically compressed). We might rule out mechanical influences by including the internal arrangement of a complex body’s particles—its shape—in its constitution. We might even go so far as to include temperature in a body’s identity; after all, it is merely the motion of the constituent particles, which must shift their internal arrangement somehow. However, the more particular we require the body’s state to be specified in order to fix its identity, the harder we make it for any property to persist; persistence requires that the same body undergo some kind of change. If any change causes the body to cease existing, then persistence will be impossible.
Suppose there is a physical process that changes a property of some body without changing its identity. The body is then perturbable with respect to that property. To arbitrarily modify the property, we need only control the process. Since the process is physically possible, there should be no reason in principle why we could not control it. Hence, any quantity that can be perturbed by physical causes should also be arbitrarily modifiable by humans harnessing those physical causes. The property might be restricted in a way that prevents the associated quantity from assuming certain values, but this would not remove the element of arbitrariness.

Weyl might have been conceiving of the rod as partly rigid, impervious to thermodynamic or other physical effects, although the rigidity could not have extended to interactions with the underlying geometry; otherwise the rod’s length would be determined by stipulation, not adjustment. It is clear that he did not think he was making any stipulations about the behavior of measuring rods, a mistake he frequently attributes to Einstein. In the course of the 1921 papers, particularly in “Über die physikalischen Grundlagen der erweiterten Relativitätstheorie,” Weyl elaborates this criticism of Einstein while attempting to provide a more principled and mathematical basis for rod-length adjustment. He makes clear what he thinks provides the basis for the adjustment mechanism: “As a result of its constitution, the measuring-rod assumes a length which possesses this or that value, in relation to the radius of curvature of the field.” (1921a, p. 262) The field he refers to here is the “guiding field,” or expanded world-metric, which now includes a linear differential form $\phi$ alongside the familiar metric tensor $g$ (see Chapter 2). The new world-curvature is a scalar invariant that arises from the expanded metric in a complicated way (see Section 2.3.3). At first glance, it seems that Weyl is
describing an adjustment mechanism similar to the subcharge described in the previous section, but he does not go into specifics about the construction of the rods. Instead, he tries to relate the behavior of measuring devices in general terms to the cosmological constant, which in 1921 Einstein had not yet labeled a “blunder.” In the action principles Weyl considers, the cosmological term appears naturally as a consequence of the metric’s gauge freedom; however, what appears as a constant scalar in Einstein’s version is now a field-like quantity that depends on the spacetime coordinates. By varying this quantity, he obtains a field equation that relates the gauge to the radius of curvature and the expanded metric. The cosmological factor is thus fixed naturally by the theory, and need not be fine-tuned to the total mass of the universe, as Weyl complains of Einstein’s version of the cosmological constant. This much is straightforward. The reasoning from here to the integrability of rod length is less so.

Weyl first argues that rod length is integrable when \( \phi \) vanishes—that is, when there is no electromagnetic field present. This comes about when the simplest action is used, the basic volume element scaled by the gauge factor. The gauge/cosmological factor becomes a true constant in this case, and the theory reduces more or less to Einstein’s. Although the transport of spacetime intervals (represented by vectors) would then be integrable, Weyl goes further, taking this to demonstrate that interval transport is determined through adjustment to the radius of curvature (1921d, p. 230). As we have seen above, however, integrability does not imply determination through adjustment.

Later, Weyl considers a more complicated action principle, one including a term corresponding solely to the electromagnetic field. To make the principle gauge invariant, a complicated additional term is needed. The gauge is no longer fixed in this case, and
we have freedom to vary it. If we choose what he calls the “natural gauge” \((\lambda = -1)\) everywhere, then the metric takes on units on the order of magnitude of the entire universe: “Our normalization means that we measure with cosmic measuring rods” (ibid., p. 231). We can use gauge freedom to rescale the metric so that distances are given in units of the order of magnitude of human bodies, in which case the complicating term needed for gauge-invariance becomes infinitesimally small. If we drop this term, we recover the Einstein-Maxwell theory; in other words, general relativity and classical electrodynamics should hold locally. Furthermore, if we base the transport of intervals on the natural gauge, then rod length and atomic clock frequency will be determined through adjustment to the curvature radius. Weyl recognizes that by advocating the use of two gauges, he is explicitly differentiating “aether-geometry” (that observed by the metric) and “body-geometry” (that observed by physical objects). He attempts to justify this by arguing that mass, and hence the notion of body itself, is only well-defined locally in a Riemannian geometry, where the Euclidean approximation can be used. Unfortunately, it becomes quite unclear what sense we are supposed to make of “cosmic measuring rods” in that case, since this phrase appears to refer to globally-defined structures.

The move to differentiate “body-geometry” from “aether-geometry” is not surprising and is even intuitively necessary; what is curious is Weyl’s assertion that the distinction follows naturally from the mathematics of the theory and does not represent a sort of stipulation. In a footnote following the claim that rod length and clock frequency is determined through adjustment to the curvature radius on the basis of the natural gauge, Weyl writes:
That was previously only a provisional assumption; now it is founded on physical theory. For this reason, it appears to me that the postulate pronounced by Einstein … has been dealt with sufficiently. (1921d, p. 232)

Whereas Einstein must postulate the behavior of measuring devices, Weyl indicates that he himself need not do so. By claiming that measuring devices adjust to the local curvature, Weyl thinks he is rearranging the concepts drawn from the basic phenomenology of the theory in their “natural order,” where Einstein had them “turned upside-down” (1921d, p. 230). However, Weyl’s basing of the behavior of human-sized measuring devices on the cosmic gauge does not appear to be motivated on theoretical grounds; his assertion that our measuring devices adjust to the cosmic gauge remains as ad hoc as Einstein’s claim that the devices read the metric.

To summarize, the 1921 papers advance the analysis of persistence and adjustment in several ways. The notion that a quantity’s values “carry on” is replaced by the more mathematically precise notion of integrability, which turns out to be a necessary but insufficient condition for determination through adjustment. Arbitrary modifiability turns out to be necessary and sufficient for determination through persistence. Conversely, if a quantity can take only one possible value at a given location, then it is imperturbable, and this is necessary and sufficient for determination through adjustment. A quantity determined through persistence can still be integrable, if contingent factors so conspire. Finally, Weyl attempts to give a very general derivation of the behavior of measuring devices on the basis of his unified field theory, though he is not he is not able to do so entirely free of ad hoc stipulation.
3.3. Dynamics of adjustment and persistence

The 4th edition of Raum, Zeit, Materie was published in its original German in 1921 and in English in 1923 (titled Space-Time-Matter). Although its introduction of the distinction between persistence and adjustment does not differ greatly from those in the 1921 papers, it does introduce some new elements that get preserved in later editions. Principally, Weyl backtracks on the claim that determination through adjustment for the properties of rods and clocks follows from the fundamentals of the unified field theory. Instead, he suggests that the behavior of measuring devices must be derived from the action principle, which he is now much more pessimistic about being able to specify uniquely. Whereas gauge invariance seemed to be the operative principle in the earlier accounts, the dynamics of the theory now take center stage.

Weyl’s new uncertainty appears toward the end of his discussion of persistence and adjustment. First, he notes that world-curvature provides the mere “theoretical possibility” (1921c, p. 281) of determination of length through adjustment. This is quite different from his claims in the 1921 papers to have shown that lengths adjust to the local world-curvature. He then expresses similar uncertainty about vectors, which he had previously characterized as pure geometric objects whose behavior can be known a priori:

The affine and metric connection determine a priori the manner in which vectors and lengths vary, if they purely follow the tendency to persist. But how much this is the case in nature and to what extent persistence and adjustment mutually modify one another, can only be ascertained on the basis of the prevailing laws of nature, i.e., of the action principle. (ibid.)

The earlier confidence that vector direction is determined through persistence and vector length is determined through adjustment is gone; either can be determined through
persistence if the dynamics so demands, in which case the effects on them of motion through spacetime is given by the local geometry, which Weyl takes to be determined a priori by the kinematics of the theory. But now persistence and adjustment are no longer mutually exclusive. The varying of a quantity is determined a priori by the geometry only if it exclusively follows a tendency to persist. It is now somehow possible for a quantity to be determined through some mixture of adjustment and persistence, the proportion to be determined by the dynamics. Unfortunately, Weyl does not say more about this here, so it is difficult to determine precisely what kind of mixing he had in mind.

Weyl also raises the possibility that the timekeeping properties of some clocks are not determined through adjustment:

Perhaps the period of rotation of a top is an example of a time interval that is determined through persistence; if what we assumed above [congruent transport] holds for direction, then the vector of rotation would experience a parallel displacement at each instant during the top’s motion. (1921c, p. 281)

This is a rather startling admission, given his general insistence that clock periods are determined through adjustment. A spinning top is essentially a clock, though not a very good one, thanks to friction, and most other mechanical clocks also operate on some kind of rotary or pendular motion. If the top’s period is non-integrable, then the periods of all mechanical clocks should be as well. Why should we expect the periods of electronic clocks to be different? The basic mechanisms of the quartz clock is electromechanical; a quartz tuning fork is made to vibrate, with energy supplied by a shifting electric field. Just like the basic rotary clock, it is the simple oscillatory motions of particles that is being counted. The unknown fundamental composition of the particles is irrelevant.
Only clocks where the fundamental composition plays an operative role, such as atomic clocks, will differ substantially from the spinning top clock. This explains why Weyl routinely uses the frequencies of light emitted by excited atoms when giving an example of a temporal quantity determined through adjustment. The nascent quantum theory had already made clear that these frequencies could not result from the simple orbital motions of electrons around the nucleus. If they did, then the atomic clock would be no different from a spinning top. What Weyl is arguing here is that no matter what dynamics turns out to govern the emission of light from atoms, the frequency should somehow adjust to the local world-curvature.

The argument seems difficult to extend to measuring rods, since their lengths are not obviously determined by motions of particles. Also, the length of a macroscopic object seems to be determined more by electrostatic forces than the underlying atomic dynamics. On the other hand, a macroscopic body would have no stable structure were it not for the stability of the bonds between molecules, which is certainly dependent on the fundamental physics of the constituent particles. In other words, without the stable states provided by quantum theory, we would not be able to define a measuring rod enduring in time at all. Quantum theory assigns a characteristic separation length to each kind of molecular bond, and the collective lengths of the bonds comprising a rod will determine its length. It is these characteristic lengths that Weyl must argue are determined through adjustment to the world-curvature. Of course, just like the atomic frequencies, these lengths will depend directly on the Planck constant, which must therefore also be determined through adjustment. Ironically, Weyl ultimately rejected his unified field theory on the grounds that the Planck constant (via the Bohr radius) introduces a natural
length scale and thus disconfirms the principle of the relativity of magnitude (see Section 2.4.2).

3.4. The celestial compass

In the 5th edition of Raum, Zeit, Materie, published in 1923, Weyl significantly expands his discussion of persistence and adjustment. He introduces the distinction in a new section, entitled, “Compasses and Rotation,” and later weaves discussion of it effectively into the technical presentation of his unified field theory. In the new section, he provides for the first time an example of a fully metrical adjustment mechanism. It is also an exactly adjusting mechanism, without a time delay or oscillation around an equilibrium point. This is the celestial compass, and it is contrasted with an “inertial compass,” the paradigmatic persistence mechanism.

First, we should clarify what Weyl means by this generalized use of the word “compass” (Kompaß). Given a worldline along which an observer travels, we can define at each point a local notion of “spatial direction” by considering all the line elements perpendicular to the worldline at that point, i.e., a spatial tangent space. If we pick two points, $O$ and $O'$, along the worldline, we can then consider how specific directions at the two spacetime events can be correlated. The particular mapping from one spatial tangent space to another will be the defining characteristic of a generalized compass; indeed, the compass is best thought of as the mapping itself. A physical object that traces

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46 The term “spatial tangent space” is intended to refer to a subspace of the four-dimensional tangent space at a point. At each point in the spacetime, there will be as many spatial tangent spaces as there are timelike vectors in the whole tangent space. If we begin, however, with a Riemannian manifold (as opposed to a pseudo-Riemannian manifold with a Lorentzian signature), then there is no timelike/spacelike distinction, and we have only one, unique tangent space at each point.
one of these mappings is a “realization” of the compass, rather than the compass itself. It is worth emphasizing that a generalized compass in this sense does not provide a definition of spatial direction at a point, but rather a notion of sameness of direction at distant points. The ontologically primary notion of spatial direction at a single spacetime event is given purely geometrically and is well-defined once a timelike vector is specified.

Weyl discusses two ways of mapping directions between two events. The first, the inertial compass, is the standard method for propagating direction: parallel transport. Simply parallel transport all of the vectors in the designated spatial tangent space at \( O \) along the observer’s worldline, then project them onto the relevant spatial tangent space at \( O' \), determined by the tangent vector to the observer’s worldline. The projection makes any rotation into the timelike direction (i.e., acceleration) irrelevant. He cites Foucault’s pendulum and gyroscopes as realizations of the inertial compass. The persistence properties are clear; we have great freedom in choosing the direction of the initial vector, and both this choice and the path taken will affect the direction of the final vector. However, some directional properties will remain path-independent, such as the angle between two vectors.

Like the inertial compass, the celestial compass is a mapping between directions at different spacetime events; however, the mapping procedure is entirely different. The intuitive idea is that direction should be correlated with light coming in from infinitely distant stars. That such a compass should differ from the inertial compass is easy to see. Suppose we orient a gyroscope so that it points toward a specific star, say, Vega. The alignment might fail to hold at a later time either because Vega moves relative to us, or
because the intervening, time-varying metric field alters the path of the light traveling to us. That the celestial compass is an adjusting compass is also easy to see. The direction from which light appears to arrive at $O$ is given by the projection of the four-dimensional tangent vector of the light path into the spatial tangent space defined by the observer’s worldline. Likewise at $O'$. These projections are determined solely by the light path and the choice of spatial tangent space. The path taken by the observer from $O$ to $O'$ plays no role. To be sure, the projection will proceed differently depending on the choice of spatial tangent space, and this is determined by the observer’s worldline at the point, but this is of little consequence. The choice of spatial tangent space is not a volitional choice; rather, it is a way of specifying which vectors in the four-dimensional tangent space will serve as directions. There is an identical arbitrariness for the inertial compass: once the original directional vectors have been parallel-transported, they must be projected into the new spatial tangent space, which will vary with the observer’s four-dimensional tangent vector, and this is quite independent of the arbitrariness that defines persisting quantities.

The system of incoming light rays thus meets the two original criteria for adjustment. It is independent of the path taken by an observer, and it cannot be modified arbitrarily. The latter point requires some clarification. There is a clear sense in which the initial direction of a gyroscope, a body realizing an inertial compass, can be modified arbitrarily. But with the celestial compass, it is more difficult to pinpoint the specific physical body that is performing the realization. This is best identified with the totality of actual incoming light rays at a given point. The analogue to the selection of an initial

gyroscope direction is the selection of a given ray of light (not merely a null geodesic), in which case we truly do not have the freedom to modify its direction.

With the basic structure thus specified, the next task is to define a mapping procedure, i.e., a way of uniquely associating directions at $O$ with those at $O'$. Unfortunately, Weyl’s presentation of this procedure is not particularly lucid and gets bogged down in a cumbersome geometrical proof. But the basic principle is easy enough to grasp. In a Minkowski spacetime, the mapping is rather simple. Given a null geodesic passing through $O$, there will be only one null geodesic passing through $O'$ parallel to it. Now, if a star’s worldline intersects one of these geodesics, it need not cross the other (given three dimensions of space, that is), meaning that light from finitely distant stars will not necessarily remain in the same direction. Conversely, an infinitely distant star cannot pass through one geodesic without passing through the other, lest it move at infinite speed; light from such a star thus will always be mapped to the same direction. Put another way, a celestial compass will always match up with an inertial compass in a flat spacetime, as one would expect.

Not surprisingly, the situation gets more interesting and more complicated in a general relativistic spacetime. Here, the past light cones at the two points need not have the same shape, and there is no distant parallelism to facilitate the mapping. If we assume, however, that the spacetime is asymptotically flat, then we can recover the previous result. For, at infinity, each null geodesic on one light cone can be said to be parallel, as judged according to the Minkowski spacetime there, with one and only one null geodesic on the other. All that we need to do is follow every pair of null geodesics that are parallel at infinity in toward $O$ and $O'$ and identify the directions found at the
two points. Conversely, we could start at \( O \), choose a direction, follow its null geodesic to infinity, find the matching geodesic—again, according to the notion of parallelism granted by the Minkowski spacetime at infinity—on the past light cone of \( O' \), and follow it back to \( O' \). The result is, apparently, a unique mapping of directions at the two points, one entirely independent of the path traveled by an observer passing through them.

Before investigating the technical points more closely, we should explore the consequences of using a celestial compass to gauge direction. To begin with, the mapping is well-defined for every direction, whether starlight arrives from it or not. To gauge directions in practice, we will need to receive distinguishable starlight from all of them. This situation is no different with an inertial compass, where gyroscopes or pendulums are needed to gauge direction over time. The lesson here is simply that we need to realize a compass with physical bodies in order to observe the geometric structures comprising it. In other words, the celestial compass is as much a pure geometrical structure as the inertial compass.

With a celestial compass, directions behave very differently than they do with an inertial compass. Because the mapping does not depend on the path of the observer, the celestial compass does in fact provide a method for distant comparisons of direction. On the other hand, the angle between two directions generally will not remain fixed. This is in contrast with the inertial compass, where angular separation is always fixed, regardless of the path taken. If we mapped the sky in equatorial coordinates,\(^{47}\) waited a while, and then mapped the sky again, we will generally find that infinitely distant stars appear in a

\(^{47}\) These are the earth’s latitude and longitude projected outward onto the sky, with the prime meridian located by convention where the sun’s path crosses the ecliptic at the start of the Northern Hemisphere’s spring (the vernal equinox).
different arrangement, due to the operation of the intervening, time-varying metric field. By “different arrangement,” I mean precisely that their angular separations will have changed. If we gauge direction by the inertial compass, then we will say that we have to point our telescopes in a different direction in order to locate the same star, i.e., that the star’s coordinates have changed. However, if we gauge direction by the celestial compass, then we will say not that the star’s coordinates have changed, but rather that the directions have shifted with respect to one another. In the latter case, the night sky appears to have been distorted; in the former, it is our sense of direction that has been modified.

We can easily imagine how a distorted night sky would look, but what does it mean to say that our sense of direction is modified? Weyl does not consider the question, and it is difficult to answer precisely. We could not rely on rigid bodies such as rulers and protractors to gauge the change, since these are realizations of inertial compasses. We would have to find a way to characterize the change purely geometrically, in coordinate-invariant terms. We can do this for the inertial compass: a nonzero angle between two initially coincident vectors reunited after being parallel-transported along different paths cannot be made to vanish through any permissible coordinate transformation. However, we will find that the celestial compass does not readily allow the formulation of coordinate-invariant facts.

To see this, consider an asymptotically flat spacetime with two spatial dimensions. In this world, the “night-sky” is simply a circle. Suppose at $O$ we use a protractor to measure the angular distance between two infinitely distant stars, $\infty_A$ and $\infty_B$, and find it to be exactly $90^\circ$. At $O'$ (i.e., at a later time), we measure the angle
again using the same protractor and find $45^\circ$. We conclude that the night sky has been distorted. Now, this change, as measured by the protractor, is coordinate-invariant, not just in being non-zero but in magnitude as well. Any re-coordinatization that shifts the apparent angle of incidence of the incoming null geodesics at a point will also shift the vectors in the tangent space there. In other words, we are free to deform the paths of the light rays by modifying the coordinates, but the gradations on the protractor will be similarly deformed. The coincidences of the light rays with the physical marks on the protractor are invariant under diffeomorphisms, as are all point-coincidences. As mentioned above, however, the protractor is a realization of an inertial compass, and we must discard it if we wish to rely solely on the celestial compass. In that case, it should be clear that we lose much of our ability to formulate coordinate-invariant statements about angles between light from $\infty_A$ and $\infty_B$. We will be able to say whether the angle is zero or nonzero, but nothing more than that. If we compare two angles, we will be able to say that one is greater than, equal to, or less than the other, though not by how much, and only if one is contained inside the other.

If we rely solely on a celestial compass for our sense of direction, and the mapping of directions between two points is a diffeomorphism, then we will not be able to discern any modification of that sense at all (keep in mind that to sense direction using only the celestial compass, we somehow need to make observations without using non-infinitesimal objects such as our eyes, which are realizations of inertial compasses). The infinitely distant stars will appear motionless, regardless of the action of the intervening metric field. The motion of a finitely distant object could still be gauged, because the points of intersection of its worldline with null geodesics passing through our points of
observation are coordinate invariants. We would see such an object moving across a truly fixed background.

If the mapping is allowed to be non-diffeomorphic, then we will be able to detect changes in the ordering properties of our sense of direction. Consider the previous example, but where light from two other infinitely distant stars, $\infty_c$ and $\infty_d$, is observed as well, coming from directions between $\infty_a$ and $\infty_b$, as indicated in Figure 3.1. If we later find the directions to have been rearranged, so that they fall as indicated in Figure 3.2, then we will have observed a change that cannot be reversed through any permissible coordinate transformation. There is no continuous mapping between the one-dimensional, circular “night skies” represented by these figures, and so we would be able to detect such a change, relying only on the incoming light. Any non-diffeomorphic mapping between two-dimensional, spherical night skies will be similarly detectable, although the way in which changes in the “ordering properties” of the directions constitute the modifications to our sense of direction cannot be illustrated as simply.

![Figure 3.1: 1-dimensional night sky with four infinitely-distant stars.](image-url)
Are non-diffeomorphic mappings for the celestial compass possible in a general relativistic spacetime? The answer is yes. Suppose an observer passes through two points $O$ and $O'$ in a spacetime with three spatial dimensions. Suppose $O$ is at the center of a uniform, spherical shell of dust, and that by the time the observer reaches $O'$, this dust has coalesced into a small sphere some distance away from him. Figure 3.3 shows a map of the night sky he sees at $O$ in equatorial coordinates, where shading indicates latitude. If the eventual point of coalescence is at the north pole and the dust does not collapse beyond its Schwarzschild radius, then the map at $O'$ (of the northern hemisphere) will be given by Figure 3.4, where the shading corresponds to the latitudes in Figure 3.3.
Figure 3.3: Northern hemisphere at $O$ (shading indicates latitude, darkest at north pole, lightest at the equator).

Figure 3.4: Northern hemisphere at $O'$. The north pole appears a second time as a circle (dark circle), while latitudes less than $\theta_{\text{max}}$ (the dotted lines) appear three times.
The north pole gets mapped both back to itself and to a circle, known as the Einstein ring. There will be a critical angle, $\theta_{\text{max}}$, dependent on the density of the dust, that represents the largest deflection of light made by the dust. The night sky between the north pole and this angle will be duplicated, with a longitudinally reversed image appearing inside the north pole circle. This image will appear a third time, at the very center of the sky and latitudinally-inverted from the second image. When $\theta_{\text{max}}$ grows large enough, the south pole will also be mapped to a circle and the image around it will appear in triplicate. When the dust reaches its Schwarzschild radius, a lightless region will appear around the new north pole, and the image inside a minimum angle will appear only in duplicate.

The mapping represented by Figure 3.4 is clearly not a diffeomorphism. To begin with, it is not one-to-one. The north pole, a single point, gets mapped to an infinite set of points, and many other points are duplicated. This means that the mapping has no inverse. This is true despite the fact that the movement and collapse of the dust can occur continuously. In fact, collapse is not even necessary, nor is it necessary for $O'$ to be outside of the dust. As soon as $O'$ is not at the center of a spherical mass distribution, then the spatial trajectories of light rays, incoming or outgoing, will be bent; we will then always be able to find at least two different rays that start out or end up parallel at infinity.

While the availability of non-diffeomorphic mappings might make the phenomenology of the celestial compass interesting, it actually complicates the definition of the compass itself. Weyl’s proof of its cogency depends critically on the existence of a one-to-one and onto mapping (i.e., one with a unique inverse) from one set of directions
to the other. If one direction gets mapped to many (or worse, to none at all), or if many
directions get mapped to one, then our sense of direction will not merely be distorted, it
will be useless; if we relied solely on the celestial compass for our sense of direction, we
would find ourselves rather confused as to which direction is which. However, we might
not find ourselves hopelessly confused. To be sure, if we mapped directions once, waited
for a time, then mapped directions again, then we would have no way of knowing how to
map directions inside the $\theta_{\text{max}}$ circle, since light coming from them would now appear in
triplicate. But if we mapped continuously, then the ordering properties of the directional
manifold—the spatial tangent space—would allow us to keep track of which were the
originals and which were the new copies. The north pole circle might still present a
problem, however, since its continuity has no counterpart in the original directional
manifold.

While the celestial compass cannot coherently define how directions evolve over
time, it is not itself incoherent, and can still shed some light on how Weyl understood
determination through adjustment. “Direction” is clearly not the right name, but
whatever it is that the compass defines, call it “quasi-direction,” is in fact imperturbable
in precisely the sense required above—it cannot be modified at all by anything acting
locally. Even if we deformed the local spacetime by placing a massive body at the point
of observation, it would not change the spatial trajectories of light rays coming or going
from that point at all (it would of course deflect all other light rays). The compass is
uniquely defined at each point in space, even if the sense of direction given by it is not.
Thus, it still seems appropriate to say that the properties of any realization of the celestial
compass are determined through adjustment.
But what exactly is a realization of the celestial compass adjusting to? It is not quite right to say that it is adjusting to the local metric curvature, as Weyl wants rods and atomic clocks to do, since spacetime curvature in distantly located regions clearly matters. But the properties of the compass are fixed at each point; as an observer moves through spacetime, he or she carries along his or her realization of the compass, and, at each point along the observer’s worldline, this realization adjusts to the local, unique definition of the compass. One can think of the compass definition at a point as defining a kind of curvature of quasi-directionality, to which the realization of the compass adjusts.

Though not quite in the way he intended, the celestial compass does provide a fully articulated, physically plausible example of determination through adjustment. On the other hand, its existence does not establish the determination of rod length or atomic clock frequency through adjustment. In fact, since the compass does not adjust to the local metric curvature, the example is not even a good analogue for the mechanism underlying rod or clock adjustment. But it does help to demonstrate the coherence of the distinction between persistence and adjustment, and its presence in 5th edition of Raum, Zeit, Materie reaffirms Weyl’s commitment to the distinction and to its usefulness in the defense of his field theory, at least as of 1923.  

48 Weyl’s celestial compass has not been entirely mothballed since 1923. Soffel, Schastok et al. (1985) use it in a discussion of deflection of starlight in the vicinity of large masses. Kovalevsky and Seidelmann (2004) give a brief treatment in an astrometry textbook.
3.5. Appendix to PMNS

In 1949, an English translation of Weyl’s 1928 epistemological treatise, *Philosophy of Mathematics and Natural Science*, was published, and Weyl took the opportunity to update the text. He did not wish to rewrite the book, or even to revise its content significantly, even though some of its analyses were clearly outdated. He chose to update his views on the themes of the book in a series of lengthy appendices, which, like the book itself, range in topic from the most abstract mathematics to the philosophical implications of the concrete sciences, such as chemistry and biology. Tucked away in the final appendix, in the middle of a discussion of the future of fundamental science, is the first newly crafted treatment of the distinction between persistence and adjustment to appear in print in the 26 years following the 5th edition of *Raum, Zeit, Materie*. The appearance is notable for several reasons. First, it occurs outside the context of the unified field theory, long since abandoned, which confirms that he thought the distinction to be of general validity and not merely as a device to be employed in defense of the theory. Second, it sheds a bit more light on his vision for the mechanism underlying the adjustment of rod length. Third, it appears in the context of considerations of fundamental constants, including the Planck constant, making his earlier repudiation of the unified field theory on the basis of quantum-determined natural length scale appear even more strange (see Section 2.4.2).

The introduction of persistence and adjustment in the 1949 appendix begins in a way similar to the previous treatments, although in a somewhat more elegant and concise fashion. Weyl wonders why there are many copies of particles all having the same mass and charge, when classical physics allows bodies of arbitrary charge and mass to exist.
From this he concludes that the fixity of the charge and mass of elementary particles results from some kind of adjustment, rather than “perseverance.” He then contrasts the rotating top with a magnetic needle, before introducing arbitrary modifiability and reaffirming its connection to perturbability:

If conservation of a quantity depends on inertia then its initial value may be chosen arbitrarily; but since perturbation can never entirely be eliminated, deviations are apt to occur in the course of time. Adjustment however enforces a definite value that is independent of past history and hence reasserts itself after any disturbances and any lapse of time as soon as the old conditions are restored. (1949, p. 288)

Quantities determined through adjustment may be perturbable, but only in the short term. As soon as disturbances have been removed, the quantity will return to its locally determined value. On the contrary, a quantity determined through persistence, presumably, would continue to bear some mark of the disturbance. This seems to answer the question of whether adjustment was intended to be instantaneous or not—here the magnet needle appears to be an explicit paradigm. It is also possible that a quantity that adjusts only when undisturbed is an example of persistence and adjustment modifying one another, a possibility raised in the 4th edition of Raum, Zeit, Materie (and discussed above in Section 3.3).

Weyl then complains again of Einstein’s treatment of rods and clocks. These preserve their lengths and periods when transported, but only because the charges and masses of the constituent elementary particles are preserved, which must be postulated. A more comprehensive approach would work differently:

49 The appendix appears to have been written by Weyl directly in English. It is unclear why he chose the term “perseverance” over “persistence.” “Persistence” had been the choice in an earlier English publication (Weyl 1921a).
The systematic theory, however, proceeds in the opposite direction; it starts with a
metric ground form and thus introduces a primitive field quantity, to which the
Compton wave length $m^{-1}$ of the particle adjusts itself in a definite proportion.
(1949, p. 288)

By “systematic theory,” Weyl means a true theory of everything—one that postulates
nothing, or at least nothing that need be ascertained phenomenologically. The
“primitive field quantity” is surely some scalar curvature defined in terms of the
fundamental metric, however complicated and different from the general relativistic that
might be. But now his confidence in quantum theory enables him to state more precisely
what inside the rod should be doing the adjusting, or rather what quantity is determined
through adjustment. It is not some length characteristic of molecular bonds, as
speculated above, but the characteristic length associated with each fundamental particle,
the Compton wavelength, which depends only on the particle’s mass and the Planck
constant. Unfortunately, he does not repeat the distinction between mechanical clocks
and atomic clocks, so it is not clear whether he intends the periods of mechanical clocks
to adjust as the lengths of rods do.

Finally, Weyl mentions attempts to modify Maxwell’s equations so that only one
or some small number of spherically symmetric solutions is possible. Of these, he writes:

Had one succeeded, then adjustment would have been explained in the framework
of classical field physics. But so far this idea has led nowhere. Quantum theory
on the other hand solves the riddle, at least to a certain extent, by the quantization
of field equations… Equality is accounted for, yet the particular values of charge
and mass remain as unexplained as before. (1949, p. 288)

50 See Section 5.2 for more on Weyl’s views of the end-goal of physics.
Somewhat curiously, he claims that *adjustment* would have been explained, when really the need for it as an explanatory tool would have been eliminated. The facts that adjustment was intended to explain, that all elementary particles have the same charge and that the charge has a particular value, would have been explained by other means. The attempts that he refers to, in particular Mie’s electrodynamics, in no way tried to fix the charge of the electron to some other value, certainly not local metric curvature; they were not even metric theories at all. Likewise, the quantization of the field equations that occurs in QED does not reference any kind of adjustment mechanism. He simply overstates the case here.

The 1949 Appendix does not on its own advance our understanding of persistence and adjustment much further, but it does clarify some points of confusion. Most important, it shows that Weyl thought the distinction to be important and useful in crafting and understanding physical theory on its own, outside the context of his defense of his unified field theory, and continued to advocate it more or less until the end of his life.

3.6. Conclusion

Weyl introduces and elaborates the distinction between persistence and adjustment for two distinct purposes: 1) to provide the beginnings of an argument for why certain properties of identical particles take the same exact values in all places and at all times, and 2) to show how his unified field theory could be compatible with the

\[51\] A useful discussion of Mie’s approach and of other electromagnetic programs in the early 20th century can be found in Vizgin (1994, chapter 1).
observed behavior of measuring rods and clocks. The second use did not last beyond the 1920s, as he abandoned his field theory due to a perceived conflict with the new quantum mechanics; but he found the first use compelling until the end of his life. The distinction survives close scrutiny, although some of the examples Weyl offers in hopes of clarifying it, in particular the celestial compass in the 5th edition of *Raum, Zeit, Materie*, do not quite operate in the way he intended. Still, if the properties of fundamental particles are ever to submit to explanation, the distinction should prove useful; that there are fundamental particles that have the same basic properties everywhere and at all times does call out for some kind of explanation, and adjustment is certainly preferable to simply accepting brute fact. Indeed, the best modern explanation for the masses of the fundamental particles of the Standard Model, known as the Higgs mechanism, involves a local interaction between those particles and a scalar field, i.e., an adjustment to the local properties of the Higgs field.\(^{52}\) However, as a response to Einstein’s objection, the invocation of determination through adjustment remains a promissory note; Weyl argues that there is some mechanism that solves the problem of apparent empirical disconfirmation, but, as he acknowledges, he is unable for the moment to provide the necessary detail about that mechanism.

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\(^{52}\) While the Higgs is an example of an adjustment mechanism, the adjustment itself is not actually what explains the common masses of identical fundamental particles in the Standard Model. Rather, this is explained by the fact that the Higgs field must assume the same vacuum state throughout a continuous region of space, one that in our region happens to encompass the entire visible universe, a decidedly non-local property. The Higgs might assume different vacuum states in finitely separated regions (much as magnetization can vary between regions of a magnet), but then the masses of the particles (and possibly their identities) will be different in the two regions as well, even though they adjust to the Higgs. I thank Ikaros Bigi for pointing this out to me.
In the previous chapter, I examined Weyl’s response to Einstein’s claim that the 1918 unified field theory could be empirically disconfirmed. In this chapter, I will consider a different aspect of Einstein’s objection, one that was seemingly more epistemological in character. In correspondence between the two prior to the publication of Weyl’s theory, and in an appendix to that publication, Einstein argued that Weyl’s handling of the measurement process and its relation to the metric robbed the theory of its empirical basis, rendering it mere mathematical or formal speculation. In response, Weyl launched a counterattack: general relativity, when handled with the appropriate epistemological care, suffers a similar or even worse difficulty. In a truly comprehensive and fundamental theory, he argued, the behavior of measuring devices as measurers should be derived, not stipulated. Einstein would ultimately agree with Weyl on this point, creating an apparent tension with his repeated insistence that measuring rods and clocks play a privileged role in giving empirical content to both special and general relativity, a point of view strongly advocated by the logical empiricists Schlick and Reichenbach.

I will argue here that we can reconcile Einstein’s views by connecting them to his methodological distinction between principle and constructive theories, and that doing so places his objection to Weyl in a different light; it should be read less as a complaint
about Weyl’s theory itself than about the lack of empirical support for the principle that
guided its construction, the relativity of magnitude. This reading of Einstein creates
room for agreement between the two on the proper treatment of measuring devices in a
complete, systematic theory; in particular, both thought that the behavior of measuring
devices should be derived from the first principles of the theory. I will eventually, in
Chapter 6, subject this common reasoning to an extended analysis, in order both to show
how it should be implemented and to address a shortcoming that will be indicated at the
conclusion of this chapter, namely, that the observability of measuring devices must also
be accounted for within the theory, if stipulations are to be avoided in its endowment with
empirical content.

4.1. The (apparently) epistemological facet of Einstein’s objection

Early in their brief correspondence about Weyl’s new theory, Einstein indicated
his concern that the project had a speculative character, in that it grew out of purely
formal considerations, not physical ones. In his first written response to Weyl, Einstein
praises some of the work’s technical aspects before offering a rather qualified
compliment: “At any rate, apart from agreement with reality, it is a terrific achievement
of the mind” (Einstein to Weyl, 8 April 1918 [CPAE 8, Doc. 499]). The following week,
Einstein sent another letter expressing the complaint in more specific form:

Though your idea is beautiful, I must nevertheless frankly say that it is in my
opinion impossible that the theory corresponds to nature. Namely, the $ds$ itself
has real significance… If one abandons the connection of the $ds$ to measurements
made with measuring rods and clocks, then the theory of relativity loses its
empirical basis entirely. (Einstein to Weyl, 15 April 1918 [CPAE 8, Doc. 507])
In the case of relativity theory, he appears to be arguing, the empirical basis must be provided in a particular way: 1) Because the infinitesimal metric interval has intrinsic physical significance, it must be directly correlated with some kind of measurement, and 2) rulers and clocks are the appropriate media for such measurements. Weyl would eventually dispute both of these claims.

As discussed above in Section 2.4.1, the Prussian Academy published a two-paragraph objection by Einstein as an appendix to the article, along with a rejoinder from Weyl. There, Einstein addresses a potential response that light rays could also trace local metrical properties and thus provide a suitable (and even preferable) means to give the theory empirical content.\(^{53}\) Einstein tries to rule this out while reiterating the points made above:

If light rays were the unique means of empirically ascertaining the metrical relations in the neighborhood of a world point, then a factor in the interval \(ds\) (as in the \(g_{ik}\)) would indeed remain indeterminate. However, this indeterminacy does not exist if one defines the \(ds\) in terms of measurement outcomes that are to be obtained with (infinitely small) rigid bodies (measuring rods) and clocks. A timelike \(ds\) can then be measured directly with a unit clock whose worldline contains \(ds\).

Such a definition of the elementary interval \(ds\) would become illusory only if the concepts “unit measuring rod” and “unit clock” were based on an in principle false supposition. (Einstein 1918, p. 40)

Here again, he asserts that use of rods and clocks is the right way to measure the metric. He does recognize that this would not work if proper rulers and clocks could not be physically realized, as would be the case if physical lengths were not integrable, but he

\(^{53}\) This approach was later expanded to include free-falling test particles and given a rigorous axiomatization. See fn. 98.
clearly does not think this is the case. While he will eventually grow more circumspect about the uncritical use of rods and clocks, his stance toward Weyl in 1918 appears to be that they are indispensable to the formation and interpretation of theories based on a spatiotemporal metric.

In his rejoinder, Weyl accuses Einstein of having assumed too much about the behavior of rigid bodies. He agrees that in special relativity a rigid ruler at rest will maintain the same length, as will a clock the same period. But, he writes,

that is not at all to say that a clock undergoing arbitrarily turbulent movement measures the proper time, \( \int ds \) (as little as in thermodynamics an arbitrarily quickly and unevenly heated gas passes through only equilibrium states); even more so if the clock (the atom) is exposed to the effects of a strong, changing electromagnetic field. In general relativity, one can at most claim: a clock at rest in a static gravitational field measures the integral \( \int ds \) in the absence of an electromagnetic field. (Weyl 1918, p. 41)

Weyl is chiding Einstein for applying the theories beyond their domains of validity. Special relativity allows us to infer the behavior of objects moving uniformly relative to local inertial frames from those objects’ behavior in a given rest frame; but this gives us no resources to make inferences about the objects’ behavior under arbitrary accelerations. Likewise, general relativity describes the behavior of objects in gravitational fields; since electromagnetic fields are not accounted for there (nor in special relativity), one cannot know how the rigidity of bodies will be affected by them. Weyl continues:

How a clock behaves during arbitrary movement under the combined effect of an arbitrary gravitational and electromagnetic field can be learned only through the implementation of a dynamics based on the physical laws. Because of this problematic behavior of measuring rods and clocks, I have in my book Space, Time, Matter relied on the observation of the arrival of light signals alone for in principle measurement of the \( g_{ik} \). (ibid.)
In other words, the behavior of rods and clocks should not be taken as given, but rather should be derived from the fundamental laws of the theory, an open task in Einstein’s as well as Weyl’s theory. Electromagnetic radiation, a simpler and more primitive phenomenon, more readily allows such a derivation, and hence is a more appropriate medium for measurement. Weyl goes on to claim that the components of the infinitesimal metric, not just their ratios, can be determined exactly through the use of light rays alone.  

Later in the response, Weyl introduces a line of argument that seems to support Einstein’s worry that the theory has abandoned contact with reality:

> It should be noted that the mathematical ideal process of vector displacement, which is to form the basis for the mathematical construction of geometry, has nothing to do [nichts zu schaffen] with the real process of the motion of a clock, whose course is determined by the laws of nature. (Weyl 1918, p. 42)

Weyl overstates his point with characteristic rhetorical flourish. Read literally, he seems to be saying that the geometric features of a theory can have no impact whatsoever on the physical features. However, in this case, Weyl’s actions speak louder than his words. His pure infinitesimal geometry does, in fact, constrain the possible laws of nature through the requirement of gauge invariance (to the point of uniqueness, Weyl hoped); there can be no question whether geometry matters for physics in Weyl’s scheme. What he intends in the above passage is that the impact of geometry on physics is not simple or direct, but mediated through the laws of nature. The point is that the specifics of those intervening laws are essential to determining the behavior of physical objects. 

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54 This claim depends on the adoption of a universal gauge. In Raum, Zeit, Materie, Weyl derives such a gauge (the “natural gauge of the world”) by assuming that the universe is static and that its contents are uniformly distributed. See Section 3.2 for further discussion.
Weyl’s response is really a promissory note; the laws of nature are needed to answer Einstein’s objection, but they are not yet known. The subtlety in Weyl’s response, easily overlooked, is that the situation is no different for general relativity; if his own theory suffers from indefiniteness in connecting the laws of nature to observable objects, then so should Einstein’s. In line with the overall negative reception of the theory, this point was overlooked, and Weyl’s theory was generally dismissed as easily disconfirmable speculation. The sedimentation of Einstein’s point of view is visible in Pauli’s 1921 appraisal:

Weyl’s present attitude to this problem is the following: The ideal process of the congruent transference of world lengths... has nothing to do with the real behaviour of measuring rods and clocks; the metric field must not be defined by means of information taken from these measuring instruments... While there now no longer exists a direct contradiction with experiment, the theory appears nevertheless to have been robbed of its inherent convincing power, from a physical point of view... [T]here is no longer an immediate connection between electromagnetic phenomena and the behaviour of measuring rods and clocks... [T]here exists only formal, and not physical, evidence for a connection between the world metric and electricity. (Pauli 1921, p. 196)

While perhaps an interesting formal exercise in mathematics, Pauli argues, the theory is not meaningful from a physical standpoint, because it tells us nothing about the behavior of rods and clocks. The stress on the lack of an “immediate connection” between rods and clocks and the theory indicates Pauli’s agreement that these measuring devices play a special, epistemologically privileged role, such that a theory that does not employ them in the physical definition of its central concepts cannot be properly tied to experiment. As far as Pauli can tell, Weyl accepts such a conclusion by placing his geometric innovation squarely in the realm of the ideal, which is opposed to the “real.” Whether Pauli saw this defect as inherent to Weyl’s approach, as would follow from the literal reading of Weyl’s
comments above, or as provisional on the specification of the laws of nature is not clear. For his part, Weyl was not deterred by the negative reaction, and his attempt to turn the tables on Einstein would grow more sophisticated over the following decade, as I will show in the next section.

4.2. Weyl’s response elaborated

Between 1918 and 1923, Weyl published several works on his unified field theory, and each included some discussion of the epistemological facet of Einstein’s objection. Together they provide a fuller account of Weyl’s stance than is apparent in the appendix to the original 1918 paper. It is quite clear that he did not think his theory lacked empirical content and that he thought Einstein’s position on the necessity of using rods and clocks could not have been more wrong. As I will discuss in Section 4.4, Einstein softened somewhat on the latter point in this time period, enough that Weyl quotes him in agreement on a related matter in his 1927 *Philosophy of Mathematics and Natural Science*.

As noted above, Weyl demands that no assumptions be made about the behavior of material bodies; instead, their behavior should be derived from postulated laws of nature. By arguing that this is just as true in general relativity as in his field theory, he makes a tacit methodological claim concerning the connection between theoretical entities and observables. Consider the formulation of the point in “Feld und Materie:”

If Einstein *defines* measure-determination in the ether with help from measuring rods and clocks, then one can allow that to hold only as a preliminary connection to experience, something like the definition of the electric field strength as ponderomotive force on the unit charge. Subsequently, such things as the behavior of charge under the influence of the electric field and the behavior of
lattice intervals in a crystalline medium under the influence of the metric field must proceed as consequences of the developed theory. (Weyl 1921b, p. 259-60)

Two somewhat distinct points can be teased apart in this passage. The first is a general point about representations of composite structures. A rigid body is just a kind of crystal, which is clearly composite; its rigidity results from the fixity of the spacing between its constituent atoms, maintained by some physical force arising between them. Our theory, which presumably tells us how the crystal can remain stable in the first place when at rest in static gravitational and electromagnetic fields, should also be able to tell us how its constituent primitives behave when moved around in arbitrary fields. The point is that the observable behavior of composite structures must follow from the theory, not the other way around. We might observe that lattice intervals appear to remain fixed when a crystal is moved in an arbitrary field, and hence assume that as a feature of our theoretical representations of crystalline bodies on a preliminary basis, but we must eventually derive the fixity of the intervals from first principles. This general sentiment is relatively uncontroversial; Weyl is simply demanding that only ontological primitives be represented as primitives within a fundamental theory.  

The second point is related but of deeper philosophical import; the phenomenologically-based definitions that we use initially to connect a theory to experiment must eventually be replaced by derivations of the same formal relationships from the fundamental principles of the theory. For example, we might begin with a phenomenological definition of the electric field as the force on a charged particle per

55 In Chapter 6, I reconceptualize this sentiment as a completeness requirement, the demand that theoretical representations describe their subject matters completely. See Section 6.2.
unit charge, but we later redefine the field as a component of the electromagnetic field tensor \( \mathbf{F} \), taken as primitive. The formal relationship between force, charge, and electric field, \( \mathbf{F} = q \mathbf{E} \), then follows from the Lorentz force law, a fundamental postulate of electrodynamics. Likewise, Einstein’s phenomenological definition of the metric tensor in terms of rod and clock properties must eventually be replaced by derivations in which the tensor’s properties are postulated as fundamental and those of the measuring devices defined in terms of them. Thus, with rods and clocks, the need for a more detailed representation is two-fold: 1) because they are composite objects, and 2) because they are supposed to be measurers of the theory’s primitive entity, the spacetime metric.

We can formulate Weyl’s tacit methodological appeal as a principle: all rules and concepts concerning observable phenomena used in understanding, interpreting, or evaluating a theory must derive from the basic precepts of the theory. His advocacy of this idea can be seen even more clearly in the final edition of *Raum, Zeit, Materie*:

> It is necessary to close the circle; once the physical action laws are established, one must prove in accordance with the action laws that charged bodies under the influence of an electromagnetic field exhibit those behaviors that we initially used for the physical definition of the field strengths, and likewise for measuring rods under the influence of the metric field. (Weyl 1923, p. 298)

The question is one of proving, as opposed to stipulating, the behavior of observables. A sound theoretical basis must be provided for the empirical. The physical definition of the field should ultimately come not from an immediate connection to observables, but from a formal, mathematical statement, the action principle (the laws of nature).

Weyl’s view of the epistemological importance of this methodological principle is made apparent in an explicit comparison he drew of himself to Einstein. In a companion piece to “Feld und Materie”, “Über die physikalischen Grundlagen der erweiterten
Relativitätstheorie”, Weyl attempts to derive the integrability of measuring rod length. First, he argues that the length of a rod is determined through adjustment to the local curvature. Then, he argues that a universal gauge arises naturally from his theory, given only the assumptions that the universe is static and is filled uniformly with matter, both commonly-held beliefs about the universe at the time. If we take the matter distribution to be perfectly uniform (i.e., a homogenous dust cloud), then the radius of curvature turns out to be constant everywhere and directly related to the cosmological term, which is also constant and arises naturally from the requirement of gauge invariance (and not by stipulation, as in Einstein’s theory). The curvature can then be used as a standard of length, what Weyl refers to as the “natural gauge of the world,” which we can use to fix the heretofore-indeterminate size of the infinitesimal metric interval. Thus, Weyl argues, the observable behavior of measuring rods and the properties of the infinitesimal metric can be derived from first principles. Einstein, on the other hand, by claiming that rods and clocks “read off” the metric, stipulates the behavior of measuring rods in order to fix the determinant of the metric; only after such a normalization does the radius of curvature turn out to be constant (a requirement for the desired uniformity of mass distribution in the universe). Einstein has things conceptually backwards, according to Weyl:

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56 See Chapter 3 for further discussion of determination through adjustment. The gauge-fixing technique discussed here is examined in Section 3.2.

57 Indeed, it was largely to account for these supposed facts that Einstein had made two more-or-less ad hoc additions to general relativity: the cosmological term and the requirement that spacetime be closed.
While the laws agree exactly with Einstein’s, one sees here how the concepts, turned upside-down by Einstein’s point-of-view, arrange themselves in their natural order. (Weyl 1921d, p. 230)

Einstein stipulates what should be derived. As a bonus, Weyl adds, the cosmological term arises of necessity in his theory, as does a correlation between the size and mass content of the universe, both of which Einstein must stipulate ad hoc. One can question whether Weyl’s derivation is successful or not, but his view of the necessary epistemological-methodological task is clear.  

What Weyl is doing is implicitly drawing a distinction between what should be considered primitive or fundamental in a theory and what should be considered observable, to the end that what is observable is likely to be separated from the primitive by several layers of complexity. The appeal is inherently reductionistic—the behavior of a composite object should be accounted for through a characterization of its parts, and since the objects we can observe directly happen to be composed of extremely small parts, we have no choice but to construct theories whose primitives are not directly observable. This could be viewed as a contingent fact; had we been constructed on a (much) smaller scale, perhaps we would be able to observe the primitive ontological entities directly. Indeed, as I will argue in Chapter 6, if stipulations are to be avoided altogether in the description of how measuring devices do their jobs as measurers, the physical constitution of the observer must be taken into account. However, Weyl did not

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58 As it happens, the central assumption in his derivation, that matter is distributed homogeneously throughout the universe, is not only ad hoc but clearly false. The visible matter distribution is very lumpy, with clusters of galaxies separated by huge voids. Even if the matter distribution were (relatively) smooth at the largest length scales, this would not help Weyl, since it is the local curvature to which the objects are supposedly adjusting.
ask what makes rods and clocks observable; rather, he was content to point out that those objects are manifestly composite, and that their behavior as measurers should be derived.

The division between observability and fundamentality that Weyl has in mind is made precise in the genetic account of theory construction he gives in *Philosophy of Mathematics and Natural Science*. There he argues that physical theories undergo successive steps of abstraction over the course of their development, moving from what is immediately accessible to observation, i.e., the phenomenological, to the formal mathematical structure of the underlying objective reality. The end-result is a purely formal, axiomatic structure, in which all entities and concepts are implicitly defined by their structural relationship with one another. The concepts and entities of the “intermediate” stages operate like scaffolding during the construction of a building—transitionally helpful or even crucial, but ultimately superfluous and dispensable.⁵⁹

Tellingly, in this very different context, Weyl once again invokes the example of the electric field to demonstrate this (Weyl 1949, p. 114). The notion of the electric field, he writes, was created by abstracting from forces, originally taken as given. The relationship was eventually turned around, so that the field was taken as given and existing regardless of whether a charged body was present to experience a force. Finally, in Maxwell’s theory, the field enters merely as a structure with a specific formal character (i.e., a vector) that is related by a set of formal equations to other structures (i.e., via the Maxwell equations).⁶⁰ For Weyl, that structure and those relations are the

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⁵⁹ See Section 5.2 for greater detail on Weyl’s scientific methodology.

⁶⁰ Interestingly, Einstein described the development of the concept of the electric field in a similar fashion in lectures to students during the winter of 1910-11 (CPAE 3, Doc. 3). There he was advocating a Duhem-style confirmation holism (see Section 4.4), and he intended to show that the theoretical notion of
objective essence of the electric field; its relation to what is accessible to observation is secondary.

4.3. Weyl and the logical empiricists on how a theory gets empirical content

Weyl’s view of how scientific concepts in an advanced theory gain empirical meaning represents an extreme opposition to the verificationism advocated by some of his notable contemporaries, Moritz Schlick and Hans Reichenbach in particular. Weyl argues that theoretical concepts are defined implicitly by their formal relationship to other concepts, while for verificationists such concepts are meaningful only insofar as they can be reduced to concepts directly involving observation. Because he omits reference to observation in the definitions of these concepts, Weyl must give an account of precisely how scientific theories get connected to observation, i.e., how they gain empirical content. The question is of central importance to the Einstein objection; recall that Einstein and Pauli both accuse Weyl of creating a theory without any empirical content. For his part, Einstein expressed some complicated views on the issue, some of which seem to match up fairly well with Weyl’s genetic account of theory construction, others which seem quite opposed. These will be explored in Sections 4.4 and 4.5.

Weyl’s broader scientific methodology will be examined in greater detail in Section 5.2. What is important for the present purpose is that he viewed fully mature

the electric field could not somehow be verified in isolation from the remainder of the theory. The fact that electromagnetism can be used to ask empirically meaningless questions (such as whether there is an electric field inside an electron) does not mean that all claims of electromagnetism are empirically meaningless. As I discuss below, Howard (2005) argues that Einstein explicitly endorses a more comprehensive semantic holism, in that a concept such as the electric field has no meaning whatsoever outside of the theoretical context in which it is situated.
theories as initially uninterpreted axiomatic structures that have to be deliberately connected to observable experience. One consequence of this is that individual scientific statements cannot be meaningfully evaluated in isolation; a theory can only be evaluated empirically as a whole. Another consequence is that the way in which the theory is connected to observation needs to be specified explicitly; theories cannot do so on their own.

Weyl drew these conclusions explicitly. After illustrating the progression of electrodynamics through the constructive phase up to its apotheosis in the Maxwell-Lorentz theory, he describes the very last step: “If forces are considered observable, the link between our symbols and experience will thus have been established” (Weyl 1949, p. 113). The conditional phrasing of the statement indicates that we are not required to consider forces to be the observables. Once we do, however, the connection is made, and the theory (if consistent) will make univocal predictions about what will and will not be observed. He does not say so explicitly, but another choice might have been made, and hence a different mathematical structure might have been deemed observable. The importance of this conventional moment will take on greater significance when we consider the competing views of the logical empiricists below.

Weyl is explicit about his holism: “Individual scientific statements cannot be ascribed an intuitively verifiable meaning, but truth forms a system that can be tested only in its entirety” (Weyl 1949, p. 151). Elsewhere, he addresses the issue in connection with the behavior of measuring devices:

Against the argument that an attempted experimental test of geometry always involves physical statements about the behavior of rigid bodies and light rays it may be pointed out that the individual laws of physics no more than those of geometry admit of an experiential check if each is considered by itself, but that a
constructive theory can only be put to the test as a whole. (Weyl 1949, p. 134, his emphasis)

Rods and clocks cannot be essential for the measurement of local metrical properties, he argues, because nothing is, not even his preferred medium, rays of light. Given an abstract, logical-mathematical structure, we have significant freedom in designating some symbols and not others to correlate to directly observable phenomena. No experiment can compel us to adopt a particular interpretation. Thus, we can only test the combination of the whole theory and a chosen interpretation together at once.

The stated target of these remarks is not Einstein, but rather “empiricists.” Weyl describes empiricists as the “enemies” of the a priori; they attempt to do without the theoretical, and to grasp reality directly. He provides several historical examples to illuminate the intended opposition:

[A]ny aprioristic construction is a thorn in their flesh; they fondly imagine it to be possible to grasp reality as a thing of one stratum, as it were, without aprioristic ingredients, by a purely descriptive approach (Bacon versus Galileo, Hume versus Kant, Mach versus Einstein). (Weyl 1927, p. 95; 1949, p. 132)

What stands out immediately here is the side of the ledger on which Weyl places Einstein—he is listed as an ally of the a priori. Though it is doubtful that Weyl saw Einstein as having joined him in neo-Kantianism, this demonstrates that, by 1927 at least, Weyl no longer viewed Einstein as a strict opponent on the question of how rods and clocks should be used to give empirical content to theories. At the very least, he saw correctly that Einstein’s views (to be examined in the next section) were not in line with those of his contemporaries then carrying the empiricist banner, Schlick and
Reichenbach, in particular (a fact that they themselves were somewhat slower to understand).

Schlick and Reichenbach were instrumental in crafting the logical empiricism that came to prominence in the 1920s and 1930s, by way of the Vienna Circle. The goal for Schlick and Reichenbach was to understand the philosophical consequences of the revolution in physics prompted by general relativity. Though the views of both men underwent some evolution in the late 1910s and early 1920s, they both ultimately concluded that one of the most important of those consequences was the repudiation of the Kantian project in physics, that of identifying synthetic a priori elements of scientific cognition. In this they were opposed to Weyl and in agreement with Einstein. As we will see below, however, Einstein’s reasons for rejecting neo-Kantianism were rather different.

Before situating Einstein, we should see how the two empiricists stand in opposition to Weyl. As mentioned, Schlick’s views changed somewhat during the 1920s, in a way that Weyl could not have deemed favorable. In the first edition of *Allgemeine Erkenntnislehre* (1918), Schlick endorses the axiomatic method and the implicit-definition of theoretical terms, and also the idea that convention plays a significant role in their endowment with empirical content. Weyl even quotes him to that effect earlier in *Philosophy of Mathematics and Natural Science* (Weyl 1949, p. 26) and also lists the book as a reference for the section (§18) from which the above passage is drawn. However, by the second edition of *Allgemeine Erkenntnislehre* (1925), Schlick had converted to the verificationist view that would ultimately sit at the core of logical empiricism, that theoretical terms are defined only by their relation to observation.
statements. Whereas Weyl viewed confirmation holism as something of a virtue (as did Einstein), Schlick saw it is a problem that could be eliminated only by basing the meaning of theoretical terms in the bare facts of observation.\footnote{For further discussion of the difference between the two editions of Schlick’s Allgemeine Erkenntnislehre, see Howard (1994).}

In *Axiomatization of the Theory of Relativity* (1924), Reichenbach lays out his own axiomatic method for endowing physical theory with empirical content. An axiomatic structure contains “conceptual definitions” that are connected to the real world via convention, according to principles he calls “coordinative definitions” (Reichenbach 1924, p. 8). The most important difference with Weyl concerns how to identify the portion of a theory to be coordinated. For Weyl, this is purely conventional. For Reichenbach, there are elements of a theory that require coordinative definitions (e.g., distant simultaneity in special relativity, and spatiotemporal intervals in general relativity); statements involving them are synthetic, while the remainder are either analytic or meaningless. The synthetic elements can thus be identified as empirical, even before coordination, and different theories that share the same synthetic/empirical content will actually be the same theory; the difference would be identical to that between the labels different languages apply to simple concepts (e.g., “egg” and “das Ei”), or between units of measurement (e.g., hour vs. second or meter vs. yard).\footnote{These helpful examples are drawn from Howard (2007b).} For Reichenbach (and Schlick), empirical content can be identified independently of its particular characterization, and the exact choice of presentation is inconsequential.

Schlick and Reichenbach were both principally concerned with showing how experiment can reveal the true geometry of the world; put differently, they wanted to
show that geometric claims were intrinsically empirical. In that regard, they were arguing both against neo-Kantians, for whom geometry was at least in part a priori (and hence not an empirical matter), and against extreme conventionalism, in which any desired geometry could be maintained merely by adopting different physical definitions. To combat both of these, the logical empiricists needed to argue that the immediate objects of perception, material bodies, could reliably probe the spacetime geometry, and hence that the determination of geometry was an empirical task. They did this by coordinating the infinitesimal metric with the lengths of measuring rods (and the rates of clock advancement), and by stipulating that rods of equal length will remain so after arbitrary separation, i.e., that they will be rigid. The general relativistic metric is thus identified as an inherently empirical structure (i.e., it requires coordination), and the method of its coordination is uniquely specified by the empirically confirmable fact that measuring rods retain their lengths over time and space. As we have seen, Weyl disagreed with both of these claims, in practice and in principle.63

4.4. Two Einsteins?

Though the logical empiricists claimed Einstein as an adherent, his stated views on the conventionality of rods and clocks are not straightforward, and at times even appear inconsistent. As noted above, in his initial reactions to Weyl’s theory, Einstein seems to advocate a necessary role for rigid bodies in empirical interpretations of theory. 63

63 Ryckman states the opposition between Weyl and Reichenbach on the interpretation of relativity theory concisely: “Weyl and Reichenbach accordingly stand on opposite sides in an epistemological debate that ostensibly turns on whether measuring rods and clocks do or should play an epistemologically fundamental role in the new theory” (Ryckman 2005, p. 79). For more, see also Ryckman (1995; 1996).
Yet, in both earlier and later writings, he also endorses a very clear confirmation holism and unambiguously separates himself from the verificationism and positivism of the logical empiricists. In a well-known lecture, “Geometry and Experience” (1922), he seems to do both on neighboring pages, at times espousing a provisional role for rods and clocks along the lines that Weyl proposed, and at others reiterating his objection against Weyl’s unified field theory. A further complication is the fact that Einstein eventually followed Weyl’s lead, pursuing his own unified field theory and at times even abandoning the metric as a fundamental concept.⁶⁴ These seemingly multifarious views are not impossible to reconcile; below I address a recent attempt at doing so before offering a novel alternative in the following section.

In “Geometry and Experience,” Einstein contrasts the “old” way of understanding geometry with the modern, axiomatic interpretation. In the old way of doing things, we have a direct, intuitive understanding of basic concepts such as point and line, and axioms such as “there is only one straight line through any two points” express self-evident, a priori knowledge. The modern point of view is axiomatic, as Weyl also maintained: the terms “point” and “line” are mere symbols, empty of any intuitive meaning. Einstein then describes the method for endowing this axiomatic system with meaning:

> It is clear that the system of concepts of axiomatic geometry alone cannot make any assertions as to the relations of real objects of this kind, which we will call practically-rigid bodies. To be able to make such assertions, geometry must be stripped of its merely logical-formal character by the co-ordination of real objects of experience with the empty conceptual frame-work of axiomatic geometry. (Einstein 1922, p. 31-2)

⁶⁴ In an appendix added to the 15th edition of his popular exposition, *Relativity: The Special and the General Theory*, Einstein claims that he has finally found the “most natural” generalization of the gravitational field, one in which the field tensor is no longer symmetric (1955, p. 177-8).
He adds that we need only specify the type of geometry that the practically-rigid bodies are to arrange themselves according to, and we thus produce a “practical geometry,” in contrast with the “purely axiomatic geometry.” A practical geometry contains statements about the real world that can be confirmed or disconfirmed through observation, thereby allowing us to determine whether the world conforms to a specific geometry or not. He then notes that he attaches “special importance” to practical geometry, because he would not have discovered general relativity without it.65

To this point, Einstein sounds very much like a logical empiricist. However, soon after the above passage, he admits that rigid bodies do not actually exist in nature, and that we should not employ them in interpreting our theories. His reasoning is directly reminiscent of Weyl’s:

It is also clear that the solid body and the clock do not in the conceptual edifice of physics play the part of irreducible elements, but that of composite structures, which may not play any independent part in theoretical physics. But it is my conviction that in the present stage of development of theoretical physics these ideas must still be employed as independent ideas; for we are still far from possessing such certain knowledge of theoretical principles as to be able to give exact theoretical constructions of solid bodies and clocks. (Einstein 1922, p. 36)

Einstein apparently had come to agree with the main elements of Weyl’s criticism of the use of rods and clocks in general relativity; he is now clear that they are composite structures whose behavior should be derived from first principles, and their use in the construction of physical theory should be provisional, until such first principles can be determined. Practical geometry, then, is a preliminary stage on the path to a systematic

65 Einstein is referring here to the “rotating disk” thought experiment, which led him to conclude that the general relativistic spacetime could not be Euclidean. Stachel (1989) provides a clear and thorough excavation of the role of this thought experiment in the formation of general relativity.
theory of space and time, precisely as was the ponderomotive definition of the electric field on the path to the Maxwell-Lorentz theory. The special importance Einstein placed on practical geometry in the formulation of general relativity fits perfectly into Weyl’s genetic account of the construction of scientific theory; it was temporary scaffolding used during construction, and it need play no part in the finished structure.

Lest one get the impression that he agrees wholeheartedly with Weyl, Einstein quickly and greatly complicates the picture. Even though there are no perfectly rigid bodies in nature, he argues, it is possible to determine the state of a body accurately enough that it can be considered rigid for all practical purposes. He then articulates a principle of congruence on which he says both Riemannian and Euclidean practical geometry rest: if two practically-rigid rods are found to be the same length at one time, they will remain so after any arbitrary separation. Furthermore, this principle of congruence can be demonstrated empirically, as he argues in a reprise of his objection to Weyl’s field theory:

The existence of sharp spectral lines is convincing experimental proof of the above-mentioned principle of practical geometry. This is the ultimate foundation in fact which enables us to speak with meaning of the mensuration, in Riemann’s sense of the word, of the four-dimensional continuum of space-time. (Einstein 1922, p. 38-9)

Given the apparent capitulation to Weyl expressed in the previous paragraph, this represents a stunning reiteration of the original objection. It also appears to bolster the logical empiricist position in more than one way. First, he asserts that one cannot even speak of the measurement (“mensuration”) of spacetime structure unless the congruence of measuring rods is preserved throughout time and space, thus giving them a privileged position in the relation between theory and world. And second, congruence is presented
as a principle that admits of proof from the basic facts of experience, apart from any other theoretical structure in which it might be embedded. Both points serve to earmark the metric as an intrinsically empirical concept and to support the principled, not provisional, use of rods and clocks to measure it.

Can these two Einsteins, separated by mere pages in the same lecture, be reconciled? There is some division in the literature on this point. Ryckman (2005) takes the standpoint that Einstein shifts positions over the course of the 1920s, beginning the decade as a proponent of a privileged role for rods and clocks and ending it as “virtually the only practitioner” of the geometric unification program initiated by Weyl; the apparent equivocation in “Geometry and Experience” thus represents a moment of transition and perhaps inconsistency. On this point, Ryckman concludes: “Reichenbach and Weyl can both appear as champions of Einstein, but indeed of different Einsteins” (Ryckman 2005, p. 79). Conversely, Stachel (1989) argues that there is good evidence that Einstein maintained to the end of his life a belief that one had to assume the existence of rigid bodies that could directly measure the properties of the local spacetime. Howard (2005; 2007b) argues that Einstein neither changes his position nor contradicts himself on the role of rigidity assumptions in giving empirical content to relativity theory. I will examine Howard’s argument in the remainder of this section, before offering an amendment of my own in Section 4.5.66

66 Yet another point of view is offered by Friedman (2002). Einstein’s insistence on rigid bodies playing an important role in his discovery of general relativity is, Friedman argues, purely heuristic. For example, the rotating disk thought experiment indicates only that space might be curved, not spacetime; it is Einstein’s creative genius that made the leap to the spatiotemporal curvature at the heart of general relativity. Any hint from Einstein that rigid bodies are necessary for giving geometrical theories empirical content is a vestigial remnant of the Helmholtzian stress on free mobility that dominated late 19th century philosophical reflections on geometry: “Einstein’s own appeal to rigid bodies, in the spirit of Helmholtz, therefore provides an especially striking example of how an older perspective on geometrical constitutive
Howard attempts to explain the apparent tension in “Geometry and Experience” by first showing that Einstein was himself a deeply committed confirmation holist, not just in response to Weyl, but from the early stages of his scientific career. Arguing that Einstein had been greatly influenced by Duhem, Howard finds evidence in lectures given by Einstein in 1910-11: “[The electric field] is a part of a theoretical construction that is true or false, i.e., corresponding or not corresponding to experience, only as a whole” (Einstein 1910-11, p. 325). As Howard notes, this holism is evident in “Geometry and Experience”; symbolizing geometry and physics by the letters G and P, respectively, Einstein remarks:

[W]e may say that only the sum of (G) + (P) is subject to the control of experience. Thus (G) may be chosen arbitrarily, and also parts of (P); all these laws are conventions. All that is necessary to avoid contradictions is to choose the remainder of (P) so that (G) and the whole of (P) are together in accord with experience. (Einstein 1922, p. 35)

Finally, Howard quotes a review written by Einstein of a book by Alfred Elsbach (1924) on a neo-Kantian interpretation of general relativity. In the review, Einstein argues that the division of a theory into a priori and empirical segments is entirely arbitrary, enabling him to formulate the holist sentiment in even more general terms and to draw strong conclusions against a verificationist understanding of metrical properties:

Only a complete scientific conceptual system comes to be univocally coordinated with sensory experience. On my view, Kant has influenced the development of our thinking in an unfavorable way, in that he has ascribed a special status to spatio-temporal concepts and their relations in contrast to other concepts….

Concerning the metrical determination of space, nothing can then be made out

principles can be subtly transformed into a radically new perspective on such principles that is actually inconsistent with the old” (2002, p. 218).
empirically, but not “because [it] is not real,” but because, on this choice of a standpoint, geometry is not a complete physical conceptual system, but only a part of one such. (Einstein 1924, p. 1690-91)

The passage is striking not only for its unambiguous endorsement of confirmation holism, but also because it very clearly rejects the attachment of any special intrinsic significance to metrical or spatio-temporal concepts, thus refreshing the apparent tension with his earlier remarks concerning the metric as the primary empirical structure within general relativity.

Howard argues that Einstein’s confirmation holism, as present in 1918 and 1921 as in 1924, makes it impossible for him to have endorsed an epistemologically vital role for rigid rods and clocks. Indeed, Howard argues this holism is so complete as to anticipate the more comprehensive semantic holism later advocated by Quine.\(^{67}\) The key to reconciling this view with his repeated emphasis on rigid bodies and the principle of congruence, according to Howard, is to emphasize the provisional aspect of their employment. Howard calls this an “interestingly subtle amendment” to the holist picture:

The demand for a stipulative coordinating definition for the notion of the line segment is perfectly reasonable as a kind of stop-gap, given the still immature state of our fundamental physics. We expect a future fundamental physics to yield structures like rods and clocks as solutions of our theories basic equations. A holist picture of empirical interpretation would be right, under those circumstances. But we aren’t there yet. At present we have to put in such structures by hand, as it were, from outside of the theory, and precisely because our theories don’t yet constrain our account of structures like rods and clocks, those structures have to be coordinated with theoretical primitives such as the line segment by conventional stipulation. (Howard 2005, p. 18)

\(^{67}\) As given, famously, in Quine (1951). Howard has reported being accused of creating a mythical “Quinestein.”
The suggested picture of Einstein’s position is consistent with Weyl’s reasoning that the behavior of measuring devices should ultimately be derived within a fundamental theory. Until such a derivation is possible, however, simplifying assumptions should be made, merely for pragmatic reasons. These assumptions will allow an empirically meaningful portion of the logical space of the theory to be identified. Whereas Schlick and Reichenbach thought this identification was not arbitrary, Einstein believed it was entirely so. The Elsbach review supports this reading: “The choice of geometrical concepts and relations is, indeed, determined only on the grounds of simplicity and instrumental utility” (Einstein 1924, p. 1691). Howard also notes that this serves as an effective response to the neo-Kantians, who argued, inversely from the logical empiricists, that the identification of the a priori portion of a theory is not arbitrary.

While accurate, Howard’s account of Einstein’s seeming inconsistency in “Geometry and Experience” cannot be quite the whole story. The picture he paints is of an extremely pragmatic, “anything goes” Einstein, one who is willing to choose any division of a priori and a posteriori, any means of endowing theory with empirical content, so long as the result is maximally simple and maximally useful. Howard might not intend such an extreme reading of Einstein, but a rhetorical purpose is served by pointing it out—more needs to be said. Einstein’s practice as a physicist does not fit the “anything goes” characterization; Einstein did, in fact, have a strong affinity for rods and clocks, which, as Stachel notes, he maintained to the end of his life. And the ultra-pragmatist picture also ignores Einstein’s repeated complaints that Weyl’s theory is too speculative; there should be no such thing if “anything goes.” Howard is right to focus on methodology in reconciling Einstein’s apparently conflicting views, but, as I will
argue in the following section, the reconciliation can be made along more principled lines, without such a thorough retreat to pragmatism.

4.5. One Einstein

What is needed for a more satisfactory account of Einstein’s thinking is a principled justification for the explicitly provisional use of rigid bodies and the congruence assumption. The distinction between principle and constructive theories introduced by Einstein in a 1919 *London Times* article can help provide such a justification. Though there is much to say about the distinction, we need only consider one aspect of it here (see Section 5.1.2 for a fuller treatment). Constructive theories are inherently hypothetical, postulating some relatively simple set of entities and using them to model more complex phenomena. Principle theories, on the other hand, begin with a set of higher-level principles discovered through and well-grounded in experiment. Constraints can then be derived from those principles on both the processes described by the theory and, by extension, on future theory intended to describe those processes in more detail. Though constructive theories might be the ultimate goal, he explains, attempts to build them are premature and overly speculative in the absence of well-established empirical phenomena.\(^6^8\) It is the regulatory role that principle theories play, or rather of the principles that comprise them, that will provide the key for understanding “Geometry and Experience.”

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\(^6^8\) String theory is probably the best recent example of this kind of premature speculation. String theorists now find themselves somewhat lost among the extraordinary maze of possibilities within their framework. See Smolin (2006) for a credible critique of the string theory program, and Polchinski (2007) for a critical response.
Because the principles that form the basis of a principle theory are each individually empirically well-grounded, each can play a regulative role on its own; we can thus refer to them as regulative principles. In the search for special relativity, the light and relativity postulates served as regulatory principles for Einstein. Likewise, the equivalence principle joined an expanded relativity postulate to regulate the search for general relativity.

The notion of the regulative principle is the key to resolving Einstein’s apparently mixed sentiments about the use of rigid bodies. The principle of congruence that he articulates is: “If two tracts are found to be equal once and anywhere, they are equal always and everywhere” (Einstein 1922, p. 37). As quoted above, after stating this principle, he then cites the observation of stellar spectra as “convincing experimental proof” of this principle, which becomes a “foundation in fact” (ibid, p. 38). Because Weyl’s theory does not observe this congruence principle, even approximately, it is not workable. Thus, congruence serves as a regulative principle: it is suggested by experience, it is empirically well-grounded, and it guides the development of future physics.

Because regulative principles are drawn from experiment, or “empirically discovered” (Einstein 1919, p. 228), they operate much differently from both the conventional coordinating definitions of the logical empiricists and the synthetic a priori of the Kantians. It is not that congruence is required in order to give any theory empirical content, or that spatiotemporal concepts have some special status in the constitution of cognition, but rather that we are still working at the level of a principle theory, where empirical content forms the starting point of the theoretical construction, unlike a
constructive theory, where we work from hypothetical structures back to the observable level. It is in this precise methodological sense that the use of rigid rods and regular clocks is preliminary. Einstein’s repeated insistence on their use is principled in the sense that the congruence principle is empirically well-grounded (i.e., drawn from a wide array of experiences and experiments), not that rigid rods and regular clocks are needed to make any theory testable. This further explains his stress on the measuring rod being “practically” rigid; he means less to emphasize that the rod is “almost perfectly” rigid, and hence suitable for idealization, than that rods are found to be rigid “in practice.” The provisionality of the congruence assumption is thus empirical; it derives from the conditionality of the interpretation of observation data, i.e., the fact that future observations always carry the potential to circumscribe the validity of previously accepted facts.

One might worry that by considering the role of regulative principles in isolation, we are violating at least the sense of the confirmation holism to which Einstein is clearly committed. For example, if the congruence principle or the light postulate can be discovered empirically, does this not mean that each can be confirmed empirically? The answer, of course, is that neither their discovery nor their justification happens in the absence of background theoretical structure. Such background operates at two distinct levels. The more primitive level concerns the general theory-ladenness of perceptual experience; the light postulate does not report sense data, but rather is the product of a complicated consolidation of a wide variety of sensory experiences involving a great number of interpretive steps. For example, the recognition that seeing a light bulb and seeing the sun involve the same physical phenomenon is a highly nontrivial interpretive
maneuver. At a higher level, the use of various instruments to perform measurements requires that one assume as background some kind of understanding of their functioning; since we do not measure the speed of light directly, but infer it from the use of rather complicated measuring devices, we could always adjust our understanding of the physics of the devices in some complicated fashion in order to deny the light postulate.

What marks the regulative principle is its apparent validity across a wide domain of application, not its infallibility. Since it is drawn from experience, its justification will necessarily be inductive and hence fallible, which only further emphasizes the provisional nature of its employment. Regulative principles constrain the search for future physics, but not strictly. Whatever new physics comes about, be it in the form of a constructive theory or principle theory, it is almost certain to expose the limits of the domain of applicability of those same regulative principles or principle theories used to guide its construction. There can be little question that Einstein saw it this way; classical thermodynamics regulated the development of statistical thermodynamics, which showed that the basic principles of the classical theory, such as the law of increasing entropy, need not hold strictly. Special relativity regulated the development of general relativity, in which the light principle no longer holds globally (at least not in the straightforward sense it does in the special theory). Likewise, Einstein’s later willingness to generalize metrical theories shows that he viewed the congruence principle as a guide, not a strict constraint.

To summarize, Einstein’s insistence on the use of rigid rods and ideal clocks in giving empirical content to general relativity was principled after all, but not in the way the logical empiricists advocated. Their motivation was epistemological, whereas
Einstein’s was methodological. It was not that physical theory could not be made
testable without the existence of rigid bodies, as Schlick and Reichenbach argued, but
rather that there was good empirical reason to believe that physical objects could in fact
be rigid in practice. Einstein was perfectly open to the possibility of evidence to the
contrary, but until such was found, he thought physics should not be guided by
speculation otherwise.

4.6. Conclusion—A common reasoning between Weyl and Einstein

Seen through the lens of this new, methodology-driven understanding of the
apparent tension in Einstein’s thinking about the use of rigid rods and clocks, his
objection to Weyl’s unified field theory comes into a slightly different focus. As a
principle-based extension of general relativity, the theory is best thought of as a principle
theory. But the principle used to generate the extension, the relativity of magnitude, is
motivated not by empirical discovery but by epistemological considerations. To justify
the principle, Weyl does not point to experiments indicating that distant comparison of
length does not work, but rather he appeals to the nature of the ideal observer and the
elegance of the geometry constructed from it: “It would be curious if, instead of this true
[wahren] geometry, an inconsequent and half-infinitesimal one were realized in nature”
(Weyl 1918, p. 42). Weyl is thus theorizing prematurely, without the guidance of
empirically well-grounded principles, and hence speculating on merely formal grounds.
Perhaps it is no accident that Einstein articulated the distinction between constructive
theories and principle theories in 1919, while still grappling with the consequences of the
challenge that Weyl’s theory posed.
Thus, the facet of Einstein’s objection considered in this chapter resolves into a complaint that is more methodological than epistemological. When Einstein objects that Weyl’s theory loses its “empirical basis,” we should read this not as a charge that the theory lacks empirical content, but that its core principles are no longer drawn from experience—they are not empirically well-grounded.

Such a shift creates significant room for agreement between the two, which Weyl would quickly exploit by introducing his own quasi-methodological distinction, that between persistence and adjustment (see Chapter 3). He could and did agree that the congruence principle was empirically well-grounded, although only in the measurement contexts we had so far found access to, namely, weak gravitational fields; by arguing that the lengths of measuring rods are determined through adjustment, he implicitly concedes that the apparent validity of the principle must be accounted for, i.e., that it should play a regulative role in the search for new physics. More importantly, both Weyl and Einstein agreed that the optimal situation would be for measuring devices to arise as solutions of the basic equations, i.e., that the behavior of measuring devices should be derived from first principles. Weyl would claim that he had taken a step toward doing this by articulating the method of adjustment, and he was free to claim, or speculate, that his theory showed the limits of applicability of the regulative principle of congruence, with which Einstein would not agree.

Their common reasoning, that the behavior of measuring devices as measurers should be derived within fundamental theory, pits Weyl and Einstein against Schlick and Reichenbach on the question of how physical theories gain empirical content. The logical empiricists argued that stipulation is unavoidable there, because theoretical
concepts are intrinsically empty and gain content only by being assigned to empirical phenomena, which are to be taken as given; e.g., the metric tensor is physically meaningless until it is deemed to be measurable by objects directly accessible to observation, rods and clocks. Weyl and Einstein thought that stipulation should be avoided, and that the relationship between theoretical and empirical concepts should be reversed; the theoretical entities are to be taken as given and content-ful on their own, and what is observable must be reproduced from them. In a systematic, non-provisional theory based on a metric, observable phenomena should depend on the metric tensor, not the other way around. Left unanswered in this scheme is the important question of why some phenomena should be considered observable and others not; as discussed briefly in Section 4.2, answering this question requires a more careful analysis of the process of observation than either Weyl or Einstein provide. I will carry out such an analysis in Chapter 6 and will ultimately conclude that, if the endowment of physical theory with empirical content is to be free of stipulation, the physical constitution of the theorizer as observer must be taken into account.
In Chapter 4, I argued that Weyl and Einstein were in greater agreement in the dispute over Weyl’s field theory than is apparent at first glance. They agreed that laws governing the behavior of complex objects such as measuring rods and clocks should be used only provisionally, and that, as physical theory progresses, such laws, to the extent that they have empirical validity, should be replaced with derivations from first principles, even if only in some approximate limit. The clear implication is that the goal of physics—and of science generally—should be to produce a theory in which all observable phenomena can be derived from first principles. The strength of such a claim depends on just what is meant by “first principles,” and while Weyl and Einstein were not in perfect agreement on this, both did believe that the foundation of a mature theory should be a simple and well-integrated whole, not an aggregate of disparate parts.

In this chapter, I will examine the scientific methodologies of Weyl and Einstein in order to determine with greater precision their views on the ultimate goal of physics, and of science more generally. Both tell a story in which science progresses in iterated steps from sense experience toward logical-mathematical systems of greater and greater abstractness. Both argue quite strongly that the physical world is inherently unified and that there are deep interconnections among all the sciences, doctrines that today fall under
the general heading “unity of science.” The importance of a second, more novel feature emerges under close examination, especially for the later stages of development when a theory becomes particularly abstract; the internal resistance to modification, or “rigidity,” of the overall mathematical structure becomes a critical epistemic virtue. In the first section, I will develop Einstein’s overall scientific methodology and show how he stresses theoretical rigidity directly. In the second section, I will present Weyl’s somewhat more detailed methodology and show that rigidity appears there in a more nuanced way, tied in detail to his more specific vision of the future of physics.

5.1. Einstein’s methodology of science

In Chapter 4, I examined Einstein’s views on scientific methodology in order to reconcile his seemingly contradictory claims about the use of rigid rods and clocks in the construction of physical theory. Here I will provide a fuller treatment with an eye toward Einstein’s vision of the ultimate goal of physics. As discussed in Section 4.4, he argues in “Geometry and Experience” that independent assumptions about the rigidity of measuring rods and the regularity of clocks should be employed only because at present we do not yet know enough about the theoretical principles underlying the construction of these bodies. The use of rigid bodies is therefore provisional and would be improper and unnecessary in a fully mature physics. What, precisely, did he think such a mature physical theory would look like, and what would be the right method to discover it?

69 Though I am not concerned here with critiquing the various doctrines falling under the banner of the “unity of science,” it is worth noting that they have come under much critical scrutiny in recent times. See, for example, Galison and Stump (1996) and Dupré (1993).
5.1.1. Advancement of science and the role of the a priori

In discussing the goal of physics, Einstein generally does not refer to the production of theories per se, but rather to “systems” or “theoretical bases,” an indication of the broad scope of his general outlook. The task of science begins for him with the organization and categorization of basic sensory experiences, and proceeds through the reorganization of those experiences according to simpler and simpler schemes. The more phenomena that are accounted for by fewer principles, the better the science. The notions of unification and simplicity are thus at the heart of his idea of scientific advancement. In “The Fundaments of Theoretical Physics,” after describing in general terms the technological success enabled by the segregation of physics research into specialized branches, he continues:

On the other hand, from the very beginning there has always been present the attempt to find a unifying theoretical basis for all these single sciences, consisting of a minimum of concepts and fundamental relationships, from which all the concepts and relationships of the single disciplines might be derived by logical process. This is what we mean by the search for a foundation of the whole of physics. The confident belief that this ultimate goal may be reached is the chief source of the passionate devotion which has always animated the researcher. (Einstein 1940, p. 324)

The aim of physics is to find a “foundation” for all physical theory, one that consists of the simplest possible set of fundamental postulates.

Einstein explicitly endorses a hypothetico-deductive model of justification. In “On the method of theoretical physics,” he argues that the theoretical bases of theories are not deduced from the phenomena, but rather are “free inventions of the human intellect” (Einstein 1933, p. 272). Current empirical knowledge should be represented “in the conclusions of the theory,” and the “sole value and justification of the whole
system” lies in its ability to do so. The set of fundamental concepts and postulates defining a theory “cannot be justified either by the nature of that intellect [that invented them] or in any other fashion a priori.” This latter comment is presumably aimed at neo-Kantians and their attempts to derive the laws of nature from the characteristic features of cognition, but the brushstroke appears very broad, eliminating any a priori consideration whatsoever. “[R]eason cannot touch” these concepts or postulates, he continues, before repeating the unificatory aim: “it is the grand object of all theory to make these irreducible elements as simple and as few in number as possible.” He goes on to emphasize the “fictitious” character of the theoretical system; it is almost as if he wants to envision the theoretical basis as a free-floating entity, picked out by the intellect through a fanciful and untraceable leap.

It is unlikely, however, that Einstein intended a claim quite this strong; later in the same lecture he offers the seemingly opposite view:

Our experience hitherto justifies us in believing that nature is the realization of the simplest conceivable mathematical ideas. I am convinced that we can discover by means of purely mathematical constructions the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena … Experience remains, of course, the sole criterion of the physical utility of a mathematical construction. But the creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed. (Einstein 1933, p. 274)

The last claim, in particular, suggests not only that a priori considerations are relevant, but that they might be sufficient for the generation of physical theory, in obvious contradiction with the sentiments expressed above. Once again, we are faced with two apparently different Einsteins, drawn from neighboring pages of the same text (see Section 4.4).
We can reconcile these two Einsteins by sharply distinguishing the context of justification from that of discovery. We should read the latter Einstein as arguing that a priori constraints, such as simplicity, play guiding roles in the discovery or formulation of physical theory, not its justification. Historical precedent—“our experience”—gives us reason to think that simplicity will be a useful guide in the task of discovery, but the theory can only be justified—shown to have “physical utility”—by having its deductive consequences match up with experiment—“sense experience.” With its enormous range of possible logical structures, mathematics provides a rich field of opportunity for the construction of connections between and within the quantitative sciences, one reason he locates the creative moment of discovery there. When the earlier Einstein claims that “reason cannot touch” the postulated structure, we should read him as merely adding a rhetorical flourish to the claim that the theoretical basis cannot be justified a priori. There is ultimately just one Einstein; he argues that reason can guide us in our search for the correct “foundation of the whole of physics,” but that reason alone cannot tell us whether we have in fact found it or not. He calls the postulated structure “fictitious” to emphasize that the structure is initially imagined, not derived, not that we should hesitate to think of it as real once its physical utility has been demonstrated.

This middle road that Einstein walks is expressed most clearly in a 1950 Scientific American article discussing his latest attempts at a unified field theory. There he describes physicists as “tamed metaphysicists,” characterizing as metaphysics the “illusion” that the world can be reproduced through pure thought, without any basis in empirical observation. The “metaphysicist,” he argues, believes that the simple must be the real. The physicist has such inclinations, but he or she “tames” them:
The tamed metaphysicist believes that not all that is logically simple is embodied in experienced reality, but that the totality of all sensory experience can be “comprehended” on the basis of a conceptual system built on premises of great simplicity. (Einstein 1950, p. 342)

The scare quotes around “comprehended” (begreifen) are a bit peculiar, but perhaps telling. Just before the quoted passage, he defines “comprehension” as the deductive reduction of phenomena to known or well-understood concepts, and then proceeds to use forms of the word several times without quotation marks. We can read Einstein here as emphasizing that there are limits on the kind of understanding that scientific reasoning can provide, a rejection of the idea that such reasoning tells us something about the nature of cognition, as might be argued by transcendental epistemologists (i.e., the neo-Kantians, and, less stringently, Weyl). If scientific theory were not limited in this way, we might be able to reverse the direction of implication, using transcendental arguments to deduce physical theory from thought alone. For Einstein, his domestication has the effect of reinforcing his essential realism: scientific theories are about the objective, external world, not us.

5.1.2. Layers of science and principle vs. constructive theories

In a 1936 article, “Physics and Reality,” Einstein gives an account of how the desired logical economy is achieved and improved. He argues that physics begins with a layer of concepts drawn directly from the “complexes of sense experience,” and then proceeds by generating a secondary layer of concepts and postulates from which those of the primary layer can be deduced. The secondary layer will have greater “logical unity” than the first but will no longer be directly connected to the complexes of sense
experience. A tertiary layer is then generated, from which the concepts of the secondary layer can be deduced, and so on:

Thus the story goes on until we have arrived at a system of the greatest conceivable unity, and of the greatest poverty of concepts of the logical foundations, which is still compatible with the observations made by our senses. We do not know whether or not this ambition will ever result in a definitive system. If one is asked for his opinion, he is inclined to answer no…The multitude of layers discussed above corresponds to the several stages of progress which have resulted from the struggle for unity in the course of development. As regards the final aim, intermediary layers are only of a temporary nature. They must eventually disappear as irrelevant. (Einstein 1936, p. 294-5)

Here is a clearly articulated role for the provisional use of certain concepts, such as the rigidity of measuring rods; we recognize the temporary nature of such concepts by the fact that we can imagine a theoretical basis with fewer fundamental concepts that accounts equally well for our sensory experiences. Once we have found a way to reproduce the apparent behavior of measuring rods from postulates concerning only their fundamental ontological components, the rods and clocks postulate will disappear as irrelevant. Note also the expression of pessimism, as early (or late) as 1936, that such a maximally economic basis will ever be found.

Einstein’s distinction between principle and constructive theories, which was discussed in Chapter 4 in the context of his objection to Weyl’s field theory, can be incorporated directly into this picture of how theories progress. Recall that Einstein distinguishes “principle theories” from “constructive theories” largely by how their theoretical bases are assembled and motivated. Constructive theories employ a “synthetic” method, in that they “attempt to build up a picture of the more complex phenomena out of the materials of a relatively simple formal scheme from which they start out” (Einstein 1919, p. 228). They are thus inherently both hypothetical and
reductive, postulating unobservable structure and attempting to describe completely complex phenomena (either observable or unobservable) in terms of it. He cites as a paradigm case the kinetic theory of gases, with its attempted reduction of thermodynamic phenomena to molecular motion. A principle theory, on the other hand, employs an “analytic” method, and the “elements which form their basis and starting-point are not hypothetically constructed but empirically discovered ones, general characteristics of natural processes” (ibid.). It is built from experience, whereas constructive theories are built from the hypothetical back toward experience, via reconstruction. A principle theory is designed more to consolidate experience than to explain it in terms of what is unobserved. He cites classical thermodynamics as a paradigm principle theory.

Each kind of theory is useful in its own way. Einstein describes constructive theories as more important because of their explanatory power: “When we say that we have succeeded in understanding a group of natural processes, we invariably mean that a constructive theory has been found which covers the processes in question” (ibid.). On the other hand, he emphasizes the regulatory role that principle theories play. We use the empirically well-grounded principles forming the basis of a principle theory to deduce mathematical constraints on the natural processes described by the theory, constraints that must be accounted for by any future theory, constructive or principle, attempting to describe the same processes. The importance of the guidance provided by principle theories should not be underestimated; without it, constructive theories risk being wildly and hopelessly speculative. Classical thermodynamics is the best historical case in point; its principle of conservation of energy has been employed throughout physics, and entropic considerations, by way of statistical mechanics, have found their way even into
quantum mechanics. Einstein sums up the advantages of the two kinds of theories:

“The advantages of the constructive theory are completeness, adaptability, and clearness, those of the principle theory are logical perfection and security of the foundations.”

The principle vs. constructive theory distinction has recently gained some attention in the literature. Yuri Balashov and Michel Janssen describe the difference in rather simple terms: “Whereas theories of principle are about the phenomena, constructive theories aim to get at the underlying reality” (Balashov and Janssen 2003, p. 331, their emphases). Both kinds of theories explain, they argue, but principle theories do so by pointing to regularities, whereas constructive theories present real models of nature. Harvey Brown and Oliver Pooley (2001; 2006) disagree, arguing that principle theories are explanatorily “deficient” (Brown and Pooley 2006, p. 6). They argue that Einstein’s main purpose in articulating the distinction was precisely to highlight the explanatory inferiority of principle theories. Like Balashov and Janssen, Brown and Pooley regard principle theories as inherently phenomenological; both sets of authors are concerned with giving a constructive-theory account of special relativity, in order to get at the “reality behind the phenomenon” (Balashov and Janssen 2003, p. 331).

It is, I believe, a mistake to consider principle theories to be inherently phenomenological in nature. Kepler’s laws, at least in the form in which they are considered today, are “about the phenomena” in that together they form a consolidated description of observation, a (remarkably) concise set of empirical regularities. It would be a mistake to consider relativity (special or general) to be merely phenomenological in

the same way, since its connection to actual data is significantly more remote; yet
Einstein was quite clear that relativity should be considered a principle theory. Brown
and Pooley recognize that Einstein did not draw the light postulate from experiment at
all, despite the then-recent Michelson and Morley experiments, but rather from the
Lorentz symmetry of Maxwellian electromagnetism, a theory with strong empirical
support. Brown and Pooley try to work around this by describing the postulate as a
“phenomenological aspect of all ether theories of electromagnetism” (Brown and Pooley
2001, p. 262). By doing so, however, and by focusing on the differences in explanatory
power, these authors miss the important relational aspect of the distinction, that principle
theories constrain or regulate the development of future constructive theories. The
difference between the two kinds of theories is simply that between different levels in the
progression of science described by Einstein in “Physics and Reality”; principle theories
are indeed closer to the complexes of sense experience than the constructive theories they
regulate, but they are by no means restricted to the primary, mostly phenomenological
level. That is, principle theories exist on levels lower than those containing subsequent
constructive theories, but they need not be the lowest levels in the hierarchy. The
distinction is thus better thought of as a relation between theories than an intrinsic
property of any theory considered in isolation.

Don Howard (2007a) illustrates the distinction through an analogy to formal
semantics. An axiomatic system is a formal language in which a set of statements are
designated as axioms. The set of all statements derivable from those axioms according to
the rules of the language are considered theorems. A model of an axiomatic system is
any assignment of meanings to the uninterpreted formal symbols that renders true the
axioms (and, by implication, the theorems); the assignment is typically understood as a mapping from those symbols to some set of objects, properties, and relations, themselves given either by direct experience, formal construction, or some other means. The relevant notion of truth is given with the model, and it need not be any more transparent than the means by which the entities themselves are given. If the model is explicitly constructed through formal operations, an opportunity presents itself: the statements specifying the model can be used as the uninterpreted axioms of a new formal system, one with its own models. Conversely, an axiomatic system can be shown to be a model of another axiomatic system (or itself, trivially).

Howard’s suggestion is that principle theories relate to constructive theories similarly to the way axiomatic systems relate to their models. The principles of a principle theory operate like axioms in a formal system, and principle theories constrain the development of future constructive theories just as axiomatic systems constrain the set of models that satisfy them. Furthermore, constructive theories can serve as principles theories, constraining the development of future physics. Indeed, Howard’s analogy becomes rather literal in this case; when a physical theory is suitably formalized, the principle/constructive theories distinction reduces to that of whether the system is viewed as a model of another or as an axiomatic system in its own right. Howard’s analysis fits well with that given above, in that he emphasizes the relational character of the distinction, with one caveat. The progression of theory that Einstein describes is one toward greater logical simplicity and abstractness. If we view constructive theories as further removed from experience than principle theories are, as I have suggested, then constructive theories should in general be simpler and more abstract than principle
theories. Yet, models need not be simpler and more abstract than the axiom systems they satisfy. If Howard’s analogy is to serve as a more precise model for the principle/constructive theories distinction, which I think it has great potential to do, it will need to incorporate this progressive aspect.

5.1.3. Theoretical rigidity

In a 1934 essay, “The problem of space, ether, and the field in physics,”, Einstein presents the theory of relativity as a characteristic example of the deeply abstract nature of modern physics, but also as a truly fundamental advance:

The initial hypotheses become steadily more abstract and remote from experience. On the other hand, it gets nearer to the grand aim of all science, which is to cover the greatest possible number of empirical facts by logical deduction from the smallest possible number of hypotheses or axioms. (Einstein 1934, p. 282)

The abstractness is unavoidable, he goes on to argue, and the physicist working at the fundamental level has no choice but to be guided by purely formal, mathematical considerations. Although agreement with experience will be the final arbiter, experiment alone cannot mark the path to the logically simplest theory, and the theorist must give “free reign to his fancy.” Einstein’s immediate purpose is to defend his then-current approach to unified field theory, which was based on a non-symmetric metric tensor (see Sections 2.1.3 and 2.2.3 for a discussion of metricity). Empirical considerations were useful in guiding the theorist to general relativity, he seems to be arguing, but beyond that the theory is so abstract as to allow guidance only from mathematical notions of simplicity. Notably absent is any suggestion that the limitation on the usefulness of
experiment is contingent on the current state of technology or experimental ingenuity, leaving open the possibility that he found the limitation at least partly a fundamental one.

In a 1950 *Scientific American* article, Einstein offers a general account of his unified field theory. In this case, the theory is so abstract that he was not as yet able to find any observable consequences capable of being measured. Instead, he cites an internal, structural virtue:

In favor of this theory are, at this point, its logical simplicity and its “rigidity.” Rigidity means here that the theory is either true or false, but not modifiable. (Einstein 1950, p. 350)

The theory resists attempts to mold it to a particular purpose—it “pushes back” against the theoretician’s desires. In the 1919 *London Times* article, he attributes a similar character to general relativity:

The chief attraction of the theory lies in its logical completeness [Geschlossenheit]. If a single one of the conclusions drawn from it proves wrong, it must be given up; to modify it without destroying the whole structure seems impossible. (Einstein 1919, p. 232)

Interestingly, this immediately follows a description of the three empirically-accessible points of divergence from Newtonian theory known at the time, the advance of Mercury’s perihelion, the deflection of light paths by massive objects, and the redshifting of light emitted by massive objects. That the “chief attraction” (*Hauptreiz*) of the theory should be for him an internal, structural feature rather than its empirical confirmation shows just how strong his “metaphysicist” streak ran.

In his self-styled “obituary,” *Autobiographical Notes*, written in 1946-7, Einstein addresses the distinction between external and internal criteria for evaluation of theories.
He first recognizes the problem of underdetermination; empirical disconfirmation of theories is “quite delicate” in practice, because “artificial”—i.e., ad hoc—assumptions can almost always be appended to a theory to maintain its core principles (Einstein 1949, p. 23). The solution, he continues, is to appeal to internal characteristics of the theory under consideration, its “naturalness” and “simplicity,” which he calls the “second point of view:”

The second point of view may briefly be characterized as concerning itself with the “inner perfection” of the theory, whereas the first point of view refers to the “external confirmation.” The following I reckon as also belonging to the “inner perfection” of a theory: We prize a theory more highly if, from the logical standpoint, it is not the result of an arbitrary choice among theories which, among themselves, are of equal value and analogously constructed. (ibid.)

Later, he makes clear that this second point of view is what guided him to general relativity and further toward his unified field theory:

Equations of such complexity as are the equations of the gravitational field can be found only through the discovery of a logically simple mathematical condition which determines the equations completely or [at least] almost completely. Once one has those sufficiently strong formal conditions, one requires only little knowledge of facts for the setting up of a theory. (Einstein 1949, p. 89)

For general relativity, the sufficient formal conditions are the dimensionality of the spacetime manifold, the symmetricity of the metric tensor, and the requirement of diffeomorphism invariance. For the unified field theory, the metric tensor is allowed to be nonsymmetric, but the other two conditions are maintained.

The theoretical features Einstein invokes under the headings “rigidity” and “inner perfection” are, if not identical, intimately related. The London Times article provides a direct textual link. There, general relativity is presented as a principle theory, and the
advantages of principle theories are said to include logical perfection (*Vollkommenheit*). He does not call it “inner perfection,” but there is little question he is referring to an internal characteristic of principle theories. And as quoted above, he writes in the article that the chief attraction of general relativity is its lack of modifiability—its rigidity.

The conceptual common ground between rigidity and inner perfection can be helpfully clarified by way of a distinction offered by Ian Hacking (1996), and in the process be related directly to the goal of unification. Hacking distinguishes two ways that unity in science can be valued, which he terms singleness and harmonious integration. Singleness is the property of being the only thing satisfying some set of criteria (being the god of a monotheistic religion, for example), whereas harmonious integration is the property of having parts that fit well together, or interconnectedness. Hacking argues that philosophers have tended to care more about singleness and scientists more about harmonious integration; furthermore, the scientists’ efforts have been “incredibly rewarding” for it, while the philosophers have searched “in vain” (Hacking 1996, p. 57).

Harmonious integration, or interconnectedness, is a defining feature of theoretical rigidity. Consider musical harmonies; if an instrument is just a bit out of tune, or if the timing is just a little off, the aesthetic effect is ruined, usually glaringly so. Or consider the way that two pieces of a jigsaw puzzle fit together; as the interlocking shapes become more complicated, their joining becomes more rigid in that slight modifications to the shapes will make the pieces incompatible, and the successful joining is a feature of the “inner perfection” of the way the pieces relate to each other. We could form subunits of the completed puzzle by fitting groups of pieces together, but only if there is little room
to jiggle the pieces—little “room for play,” to borrow Weyl’s phrase (see Section 5.2.4 below). The analogy to physical theory is almost literal; the pieces that come together to form general relativity do so harmoniously and in complex interlocking fashion, and if there were room for play in the joining, the theory would look like parts stuck together rather than a unified whole, as if one had thrown some denizens of a junk drawer together and called it a jigsaw puzzle. Einstein himself argues that nature has the character of a “well-formulated puzzle” (Einstein 1936, p. 295). Rigidity (and with it inner perfection) and unifying power thus appeal to a distaste for adhocness, but with slightly different emphases. If we take “ad hoc” to mean “made up for a specific purpose,” then rigidity and inner perfection make a theory seem less “made up,” while unificatory power makes a theory seem less “for a specific purpose.”

Theoretical rigidity does not map neatly onto the principle vs. constructive theories distinction. Einstein clearly identifies general relativity as both rigid and a principle theory, but one need not look far to see that principle theories can fail to be rigid; as Harvey Brown shows in great detail, special relativity can be modified in numerous ways while still maintaining the light and relativity principles (Brown 2005, chapter 2, in particular). It can even be disputed whether general relativity is a rigid theory. Indeed, Einstein himself modified the theory, adding the cosmological term,

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71 It is worth noting that Brown also argues that special relativity is better thought of as a constructive theory, as does Janssen (2002, p. 506), if, as has become the custom, one begins by postulating a Minkowski spacetime, rather than the light and relativity principles.

72 Heisenberg, for example, thought general relativity was not merely malleable, but actually incomplete. In a recent comparison of Heisenberg and Dirac on methodology, Alisa Bokulich describes Heisenberg’s notion of a “closed” theory, which overlaps considerably with Einstein’s notion of rigidity: “The elements of a closed theory exhibit such a tight interconnectedness that not a single concept can be changed without destroying the whole system” (Bokulich 2004, p. 378). A notable difference, however, is that a closed theory for Heisenberg is also a completely finished one, empirically accurate not just to
without “destroying the whole structure.” Yet, he had already done this by the time he wrote the *London Times* article, an indication that he could not have viewed the cosmological term as a true modification, instead more a convenient way to limit the admissible models. In any case, he shortly came to regret the addition of the term, in part because of its ad hoc character.

It is not clear whether constructive theories can be rigid or not. As quoted above, Einstein identifies the advantages of constructive theories as “completeness [*Vollständigkeit*], adaptability, and clearness” (Einstein 1919, p. 228); the question hinges on just what is meant by “adaptability.” If we take it to mean “flexible” or “easily changeable,” then rigidity is clearly out of the question. We might, instead, view “adaptability” as indicating that the theory can be applied to a wide variety of domains. Conversely, principle theories, since their core principles are drawn from specific sets of phenomena, could be viewed as limited to the domains of those phenomena. These two senses of adaptability can be divided roughly as internal vs. external. A constructive theory might offer internal resistance to being molded to any particular application, but still might be capable of many different applications, and hence be rigid but adaptable. However, Einstein is not clear one way or the other, and his choice of term, *Anpassungsfähigkeit*, is ambiguous between the two readings. Furthermore, if the present technological limitations but for all time. Also missing in Heisenberg’s treatment is a sense of the internal logical perfection that Einstein associates with rigid theories; it should not be surprising, then, that Heisenberg considered general relativity, Einstein’s paradigmatic rigid theory, to be open. The difference leads the overall methodologies to different endpoints; Bokulich argues that Heisenberg’s global vision of science was pluralistic, in that distinct, closed theories could and did coexist, in contrast to Einstein’s strong belief in the unity of science. As for Dirac, Bokulich shows that he argued that scientific theories are always open to modification, so that he would almost certainly have rejected any notion of theoretical rigidity as a methodological virtue.
principle vs. constructive theory distinction is to be read as a relative one, as I have argued above, then the intrinsic property of rigidity would be entirely independent.

It is clear from the *Scientific American* article that Einstein continued to view theoretical rigidity as a methodological virtue throughout his life, and that he thought it a major virtue of his unified field theory. Did he view that theory as a principle or constructive theory? Since the theory was motivated by a desire to unify electromagnetism and gravitation, not by any empirically well-grounded principle—or even a principle backed by specific physical intuition—it would seem that it could not be a principle theory. Yet its supposed rigidity would seem to speak against its being a constructive theory. On the other hand, it does look like an attempt to “build up a picture of the more complex phenomena out of the materials of a relatively simple formal scheme.” The best answer might be that Einstein thought the theory of the universe would not fit neatly in either category, but would have to combine features from both, being a logically perfect but still hypothetical construction.

To summarize, Einstein viewed the development of scientific theory as a progression, by means of generalization, away from the data of sense experience. At each level, theory exists as a set of hypotheses that are more unified and logically simpler than the level below, but that are able to reproduce all of the lower levels through deduction. He thought the grand aim of physics, and further of all science, was to produce a unified foundation with maximal logical simplicity. Experiment could not determine the path to such a theory of the universe, or even provide much guidance past a certain point. Instead, one had to rely on a new methodological virtue—theoretical
rigidity. Finally, his later attempts to locate such a theory draw on features of both principle and constructive theories.

5.2. Weyl’s methodology of science

Weyl’s treatment of scientific reasoning and of the progression of theory construction was significantly more detailed than Einstein’s. His *Philosophy of Mathematics and Natural Science* (*PMNS*), originally published in German in 1928 and then in English in 1949, was a bold attempt to find unity in mathematics and science. Developments in the twenty years between the publication and the translation, Gödel’s incompleteness theorems in particular, caused Weyl to be more circumspect about the imminent availability of such a unified treatment, yet not of its feasibility. He opted to make only some limited revisions of the original text for the translation, and instead to develop a series of lengthy appendices. His intent might have been merely to update the details of the main text and to moderate some of its youthful excitement, but the appendices form a concise and comprehensive monograph in their own right, reaffirming the expansive ambitions of the earlier effort as well as introducing some new themes. In particular, he includes extensive discussion of chemistry and biology (only physics was treated in any detail in the original work) and argues that the future of science lies in the utilization of discrete, combinatorial structure.

5.2.1. Unity and disunity

Weyl’s deeply-held belief in the ontological unity of the natural world, at least insofar as it is accessible to experience, is apparent throughout his writings. Though his
unified field theory was motivated initially by purely epistemological concerns, in the end its chief virtue for him was the conceptual advantage of the ontological unification it represented. Following general relativity’s successful reduction of gravitational phenomena to purely geometric structure, the goal for Weyl was clear, to attempt “to geometrize all of physics, for the sake of the uniformity of the physical picture of the world” (Weyl 1931, p. 338). In his extension of general relativity, electromagnetic phenomena arise from an expanded world-metric in a natural way alongside gravitational phenomena, providing “insight into the essence of the forces of nature” (Weyl 1920, p. 142). Because the theory predicted deviations from general relativity too small to be observed, this insight was the theory’s primary advantage. The rise of quantum mechanics dampened his enthusiasm for the theory, but not for the goal of unification. By 1929, he had come to believe that the quantum mechanical wavefunction represented a matter field that could not be considered a manifestation of an underlying, unified electromagnetic-gravitational field, as he had previously hoped; rather, electromagnetism, gravity, and the matter field had to appear as equals from the perspective of some underlying structure: “Not two, but rather three things are to be brought under one hat” (Weyl 1931, p. 338). Ironically, he came to believe that the failure of the principle of the relativity of magnitude to be observed in nature at the fundamental level actually demonstrated that physics could never be reduced to geometry (Weyl 1949, p. 83).  

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73 This argument does not appear in the 1928 edition. It is included in a section that was significantly expanded for the 1949 edition.
In PMNS, in a passage appearing in the 1928 edition but updated and expressed more eloquently in 1949, Weyl strongly endorses a comprehensive unity of science, both methodological and ontological:

The fact that in nature “all is woven into one whole,” that space, matter, gravitation, the forces arising from the electromagnetic field, the animate and inanimate are all indissolubly connected, strongly supports the belief in the unity of nature and hence in the unity of scientific method. There are no reasons to distrust it. (Weyl 1949, p. 214)

Biology, he continues, might currently operate with its own set of laws, but the situation is only temporary, a consequence of being at a relatively early stage of development. The “comparative” methods of passive observation and classification still prevail over “experimental” methods in biology only as a matter of practice, not principle. Seemingly irreducible concepts such as “wholeness” and purposiveness in organic systems are not explanatory in and of themselves, according to Weyl, rather they help frame the scientific questions that must be answered with the same methodology used in physics—appeal to and derivation from the fundamental laws governing all of the natural world. That is not to say that Weyl thought holistic concepts had no place in physical explanation; even in 1928 he was well aware that quantum mechanical accounts of atomic and molecular structure involved some kind of holism. Rather, he saw no reason to treat the holism in physics any differently from that found in biology: “The solution must be sought in the same methodical manner which we apply to wholeness concepts in physics” (Weyl 1949, p. 215). He extends the argument as far as the “body-soul problem,” so that psychology and the social sciences should someday be united with physics.
The Appendices to *PMNS* reiterate Weyl’s belief in a comprehensive unity of science, but they also extend and strengthen his claim, emphasizing a privileged, fundamental status for physics:

The elementary laws of matter that physics reveals and chemistry is ruled by are no doubt also binding on living matter. As long as progress from simple to more complicated configurations remains the methodologically sound way of science, biology will rest on physics, and not the other way around. (Weyl 1949, p. 277)

Biology and chemistry are not merely to be unified with physics, they will be reduced to physics. Weyl even argues that such a reduction has already occurred in the case of chemistry; he devotes an entire appendix to a lengthy discussion of the way in which quantum mechanics explains the features of the periodic chart as well as the basic mechanics of chemical bonds. Indeed, with characteristic breadth of vision, he goes on to argue that the critical structure in these explanations, the Pauli exclusion principle, can be understood purely combinatorically, ultimately as just the difference between even and odd permutations. In the face of such a tightly-integrated view of the world, it is unlikely that Weyl would have been swayed by the recent arguments in the philosophical literature against the reduction of chemistry to quantum mechanics.74

While acknowledging that biology is still much further behind chemistry on the path toward unification with physics, Weyl notes in 1949 the dramatic steps made in that direction since 1928. These are principally in genetics, where theories about the nature

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74 Scerri (2000; 2004) gives a detailed account of the seemingly ad hoc assumptions needed to derive the exact features of the periodic chart, rather than merely its gross features. He argues that chemistry is “autonomous” from physics, in that methodological unification is impossible, even if ontological unification is. Friedrich (2004) responds to Scerri in a way that Weyl would likely have endorsed. He argues that scientists are not concerned with exact reduction, but rather with reproducing structure to ever greater approximation. If gross features are reproduced, then that is evidence enough that they are on the right track.
and structure of genes were finally being subjected to experiment, to the end that they appeared to be large molecules with an aperiodic crystalline structure. The discovery of the structure of DNA just a few years later would undoubtedly have struck Weyl as perfectly consonant with the picture he paints in the Appendices. He recognizes explicitly that, even if the structure of genes could be determined precisely, we would still be a long way from knowing how they control developmental processes; yet he sees no fundamental impediment to figuring this out. He considers and rejects Bohr’s hypothesis that complementarity extends to self-sustaining biological systems, i.e., that there are limits to our ability to probe biological systems without destroying them.

Weyl thought the scientific method was preferable to other ways of generating knowledge, but for a somewhat surprising reason—not because it is more reliable or more useful, but because it allows more room for creativity. He warns scientists not to ignore an alternative method, what he calls “understanding from within” (Weyl 1949, p. 283). By this he means direct introspection of self, of perceptions, thoughts, acts of will, etc., the sort of experience that allows communication and empathy with others (even to a degree with animals). It is a mistake, he says, to dismiss understanding gained through introspection altogether in favor of objective theory, since it can serve as a useful guide to problems, even if it cannot provide objective solutions. But, he concludes, one method opens more possibilities than the other:

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75 For this Weyl relies primarily on a seminal article in molecular genetics by Nicolai Timoféeff-Ressovsky, Karl Zimmer, and Max Delbrück (1935), generally known now as the “Three Man Paper”, and on Schrödinger’s influential popular treatise, *What is Life?* (1945), itself heavily influenced by that paper.

76 See, for example, Bohr’s “Light and Life” (1933).
The way of constructive theory, during the last three centuries, has proved to be a method that is capable of progressive development of seemingly unlimited width and depth; here each problem solved poses new ones for which the coordinated effort of thought and experiment can find precise and universally convincing solutions. In contrast the scope of the understanding from within appears practically fixed by human nature once for all, and may at most be widened a little by the refinement of language, especially of language in the mouth of the poets. Understanding, for the very reason that it is concrete and full, lacks the freedom of the ‘hollow symbol.’ (Weyl 1949, p. 284)

Contrary to what most would expect, it is not investigation of our internal world that holds limitless possibilities, but that of our shared external world. The abstract language of mathematics, precisely because it fails to keep the scientist securely grounded in concrete experience, allows him or her to float away and perhaps land in some previously undiscovered realm.

5.2.2. Stages of science—constructive vs. systematic theories

For Weyl, scientific reasoning, by which the world comes to be represented by “hollow symbols,” proceeds through the iterated construction of concepts abstracted from direct experience, becoming more abstract with each iteration. In that respect, the process he describes bears some similarity to Einstein’s, although Weyl places greater emphasis on the genetic connection between the different stages. Weyl also articulates with greater precision the details and end-goal of the whole process.

In Weyl’s methodology, a particular science passes through three phases: pre-scientific, constructive, and axiomatic. The pre-scientific phase is mainly one of classification; phenomena that might be relevant to one another are identified, isolated, and catalogued. A critical part of this stage is to recognize the ways in which aspects of the phenomena might vary. For example, in examining the different ways solid objects
appear to us, we can recognize that the objects might have different shapes, that they can be oriented in different ways, and that we as observers could have been situated differently. Weyl uses the structure of vector spaces to illustrate the process; in the pre-scientific phase, one recognizes that coordinates (the phenomena) can have a range of values and that the basis (i.e., the observer) can be situated differently.

The constructive phase begins when concepts are abstracted from the phenomena and represented symbolically. Weyl reconstructs an historical example: from observations of particles in free fall, Galileo formed a notion of velocity by measuring change in location over change in time (gauged by the weight of water flowing out of a large tank), and then a notion of “same mass,” which gets attributed to two objects repelled equally after a head-on collision. In the case of solid objects, the constructive phase involves the creation of the concept of “shape” and the articulation of a geometry, which allows us to reconstruct our perceptions by situating a particular shape at a particular location in a three-dimensional spatial manifold and then projecting its image by tracing straight lines. In the vector space example, the constructive phase involves the passage to an algebraic model; one conceives a “vector,” a single object capable of representation in multiple bases, that can partake in various operations (vector addition and scalar multiplication) and can undergo coordinate transformations, which play the role of laws of nature.

The initial mathematical representation marks only the beginning of the constructive phase:

The distillation of this objective world, capable only of representation by symbols, from what is immediately given to my intuition, takes place in different steps, the progression from level to level being enforced by the fact that what
exists on one level will reveal itself as the mere apparition of a higher reality, the reality of the next level. (Weyl 1927, p. 80; 1949, p. 113)

The conception of mass enables one to construct a notion of force, from which the notion of field can be further abstracted, an entity no longer tied to pairs of objects. The concept of the electric field is abstracted from observations of objects subjected to ponderomotive forces. Mass can become generalized to mass-energy, the electric field becomes mingled with magnetism, pH becomes a feature of electrostatic molecular interactions, and so on. The key point is that the concepts used at one stage are necessarily linked, if only genetically, to those that follow. As with Einstein, the intermediate layers are disposable, although Weyl characterizes them with greater flourish, each intermediate level being a “mere apparition of a higher reality.”

The axiomatic phase is the telos of the constructive process. A theory becomes mature and “systematic” when it moves from being a collection of abstracted concepts to a tightly integrated whole. The integration is not merely logical but also semantic; a mature scientific theory for Weyl is an axiom system whose elements are defined only implicitly, by the logical relations holding between the parts of the symbolic structure they inhabit. The status of the theory is fundamentally different from the precursors that served as scaffolding in its construction:

A systematic scientific explanation, however, will reverse the order; it will erect a world of symbols as a realm by itself and then, skipping all intermediate levels, attempt to describe the relation that holds between the symbols representing objective conditions on the one hand and the corresponding data of consciousness on the other. (Weyl 1927, p. 80; 1949, p. 113)

Hilbert’s axiomatization of geometry is a paradigm case. In looking at a purely formal listing of the axioms, it is not even clear that they are “about” geometry; for Weyl, this
reduction of content lays bare the essence of the concepts used in geometry. More importantly, the axiomatization allows the removal of any notion of observer or perspective, so that the theory “transcends” subjectivity. For example, in the constructive phase, the reality of solid objects is described by the provisional assumption of a particular, even if arbitrary, perspective and orientation; in the axiomatic phase, no such assumption is needed. Don Howard (2005) characterizes the difference succinctly as the “view from anywhere” versus the “view from nowhere.” In the vector space example, the view from nowhere is attained by a coordinate-free axiomatization, where the operations of vector addition, scalar multiplication, and linear transformation are represented as abstract algebraic operations. The equivalence of all perspectives (i.e., basis decompositions) no longer need be asserted; the system incorporates it essentially. Using Howard’s terminology, we no longer need to declare that all views are equally valid, since there are no longer any views in the theory at all.

Weyl’s method is largely hypothetico-deductive, but with some important differences. In typical accounts of the hypothetico-deductive method, the means of theory generation (the context of discovery) is regarded as irrelevant to matters of justification, a view Einstein largely endorses by emphasizing “free inventions of the intellect” that “reason cannot touch.” Not so with Weyl. Axiomatization is just the last step in a traceable, constructive process based initially in direct observation. A strong genetic relationship remains between the systematic theory and its empirical roots. If the connection between concepts at different levels is purely analogical, there might only be a limiting relationship (as between classical and relativistic mechanics, for example). However, the relationship would exist nonetheless. It is for Weyl a necessary one; if the
science is to be empirical, then there must be a traceable path from the concepts we employ to the sense data we perceive in intuition.

5.2.3. The role of the a priori

Any essentially hypothetico-deductive methodology must contend with the problems associated with underdetermination, and Weyl’s is no different. His philosophical disposition prevented him from adopting an entirely conventionalist position, even as the growing complexity exposed by the new experimental methods of nuclear physics caused him to moderate his epistemological claims. He did, however, consistently and unambiguously embrace a confirmation holism: “Individual statements cannot be ascribed an intuitively verifiable meaning, but truth forms a system that can be tested only in its entirety” (Weyl 1927, p. 111; 1949, p. 151). When confronted with an anomaly, he argues, the scientist has discretion in choosing which part of the theory to modify, and only very general rules can be employed for guidance. In identifying which facts are to be given special weight, he says simply, “The genius walks under his own power” (Weyl 1927, p. 113, my translation). This led Weyl to a rather flexible view of the distinction between a priori and a posteriori:

At any given stage of the theoretical construction, there exists a hierarchy of laws, inasmuch as different degrees of fixity are ascribed to different laws. Certain ones among them are clung to with great tenacity as principles. … It is certain that a considerable portion of the theoretical system can be maintained in the face of any experiences as long as modifications to the remainder are permitted. Thus in the practice of scientific research the clear-cut division into a priori and a posteriori in the Kantian sense is absent, and in its place we have a rich scale of gradations of fixity. It is the simple form and instinctively convincing character of a law, together with its decisive significance for an extensive domain of facts, which gives it the rank of a principle. (Weyl 1927, p. 114, my translation)
At first glance, this looks very conventionalist in tone. The distinction between the a priori and a posteriori elements of a theory is malleable and merely one of ascription. Certain elements are deemed foundational principles for psychological reasons—their convincing power. However, Weyl’s own practice as a physicist speaks against such a strictly conventionalist reading. Although his early enthusiasm for imposing epistemological requirements on physical theory did wane somewhat, it did not disappear entirely.

Consider Weyl’s treatment of geometrical structure in relativity theory, where he distinguishes a priori from a posteriori elements by differentiating a metric’s nature from its orientation. The nature of a metric is supposed to be the same everywhere, independent of the matter distribution. On the pseudo-Riemannian manifolds of general relativity, it is the fact that the metric is a second-rank tensor with Lorentzian signature. Weyl calls metrics of this sort “Euclidean-Pythagorean,” since the spacelike tangent space at every point on the manifold is a Euclidean vector space (in which the Pythagorean theorem always holds). Weyl’s preferred way of characterizing this structure is group-theoretic; the isometries of the tangent space at a given point have the structure of the Lorentz group, an entity “incapable of any variation” (Weyl 1931, p. 340). The orientation of the metric, on other hand, varies with location, depends on the matter distribution, and is knowable only a posteriori. The variability of the orientation is most clearly expressed in terms of the affine connection, which can be thought of as encoding the way a given coordinate system must be transformed in order to make the

77 Isometries are mappings from the tangent space to itself that are one-to-one and onto and that preserve metrical relations.
metric Minkowskian at a given point in the manifold (see Sections 2.1.2 and 2.2.2). The fact that such a transformation is available at every point is due to the fixed nature of the metric; the fact that no single coordinate system can achieve this at all points is due to the variable orientation of the metric. Also a priori for Weyl are the basic topological connectivity of the manifold, as well as its dimension. Interestingly, he makes a further division within the a posteriori, between the contingent distribution of matter and “what is necessitated by natural law” (Weyl 1927, p. 98; 1949, p. 135), by which he appears to mean the influence of the field laws on the metric. Less clear is whether the field laws themselves are to be considered a posteriori.

Weyl offers several fairly technical arguments for why we should consider the particular features he identifies as the nature of the metric to be knowable a priori. The Lorentzian signature, he argues, is mandated by the basic features of causality; nothing would happen if there were no timelike dimensions, and past and future would be confused if there were more than one. He gives a few simplicity-based arguments for why there should only be 3 spatial dimensions, the most important being that Maxwell’s equations are only gauge-invariant in a four-dimensional spacetime. As for why the metric should have a Euclidean-Pythagorean nature, Weyl argues that the Euclidean group is the only one that allows a coherent notion of parallel transport to be defined between tangent spaces on a Riemannian manifold (and, correspondingly, the Lorentz group for pseudo-Riemannian manifolds).78

78 For more on Weyl’s arguments concerning the “problem of space”—how the basic topological features of physical space are to be explained—see Scholz (2004) and Coleman and Korté (2001, sections 4.5 to 4.7).
The question is, how a priori are these features? What “degree of fixity” did Weyl attach to them? Quite a lot, it seems, but not so much that he would maintain them at all costs. In 1918, Weyl writes of the geometry behind his unified field, theory, “It would be remarkable if, instead of this true one, a halfway and inconsequent infinitesimal geometry with an electromagnetic field stuck on were realized in nature” (Weyl 1918, p. 40). Here the brash physicist is suggesting it would be nature’s problem, not his, if the theory were disconfirmed. By 1929, he was significantly chastened, enough that he justifies his principle of gauge invariance, now reinterpreted as a feature of the quantum mechanical matter field and not the metric, on empirical grounds: “The new principle grows out of experience and summarizes an enormous wealth of spectroscopic evidence” (Weyl 1931, p. 344). Indeed, as mentioned above, the fact that this principle found a home outside of the metrical structure convinced him that the geometrization program was untenable. On the other hand, in the same 1931 text, he reaffirms his commitment to “infinitesimal geometry,” articulated now very precisely as the identification of the space of intuition with the tangent space at the location of the observer. But, he argues, this only commits him to the claim that the local structure of a manifold is what is probed by intuition, whatever that structure might turn out to be. The a priori elements of that structure will be whatever is the same at all points in the manifold.

Weyl was convinced that the distinction between fixed nature and variable orientation—and hence between a priori and a posteriori—was intimately bound to the distinction between discrete and continuous. He hints at such a connection in his discussions of the metric in the 1920s and in the 1931 lecture; the nature of the metric “does not participate in the irradicable vagueness of that which assumes a variable place.
on a continuous scale” (Weyl 1927, p. 97; 1949, p. 134), while the orientation does (Weyl 1931, p. 340).\textsuperscript{79}

In the appendices to \textit{PMNS}, he develops this connection further, through a group-theoretic analysis of the symmetries of crystalline structures. A Euclidean space maps back to itself—is invariant—under arbitrary combinations of three kinds of transformations: translations, rotations about an axis, and reflections through a plane; these define the translation and orthogonal groups of transformations, which together form the Euclidean group. All of these groups are continuous and hence uncountably infinite. If we restrict our attention to subsets of the Euclidean space and attempt to see whether they are invariant under any subgroups of the Euclidean group of transformations, a somewhat surprising fact emerges; if any translations at all are allowed, then only a finite number of combinations of subsets and subgroups are possible, depending on the dimension of the space. In three dimensions, there are only 230 distinct subgroup/subset combinations, known as the space groups. The subsets turn out to be discrete (i.e., countably infinite), comprising a lattice stretching infinitely far in all directions; the subgroup of translations in each space group is countably infinite as well, but the subgroup of allowed rotations and reflections is finite. For purely geometric reasons, then, there are only 230 distinct ways to construct a regular lattice in three dimensions.

Weyl argues, very subtly, that each of these 230 space groups is compatible with its own continuous range of possible metrics—of second-rank tensors that can be used to

\textsuperscript{79} The language is nearly identical in the two texts, with this property denied of the nature in one text and attributed to the orientation in the other.
gauge the lengths of vectors in the local tangent spaces. The full specification of the symmetry of a crystal, then, involves the selection of one from a discrete number of space groups, and one from a continuous range of metrics compatible with the first selection. The selection of the space group is completely determined by the nature of the atoms comprising the crystal; but the selection of the metric properties involves a negotiation between the nature of the atoms and the environment:

The morphological laws are today understood in terms of atomic dynamics: if equal atoms exert forces upon each other that make possible a definite stable state of equilibrium for the atomic ensemble, then the atoms will of necessity arrange themselves in a regular system of points in our strict sense. The nature of the atoms composing the crystal determines, under given external conditions, their metric disposition, for which the purely morphological investigation had still left a continuous range of possibilities. (Weyl 1949, p. 292)

The nature of the crystal is fixed and is exhausted by the space group and by the specification of the range of possible metric dispositions. At each point in space, the “orientation” of the crystal, i.e., the selection of a specific metric disposition, is variable and contingent, within a range determined by the fixed nature. This variability allows the crystal to have macroscopic organization that departs from the strict regularity of its space group nature, so that, for example, two snowflakes can differ, just as the variability of the spacetime metric allows spacetimes of arbitrary curvature. If one examines a snowflake (or ice cube) very closely, the exact regularity of the underlying dihedral symmetry is revealed, just as, if one performs localized measurements on the inertial properties of test particles, the local Minkowskian character of general relativistic

80 Very similar language can also be found in Weyl’s last major work, Symmetry (Weyl 1952, p. 126).
spacetimes is revealed. But both of these local symmetries might be imperceptible in the
global view, obscured by the variable orientation of the individual constituents.

The distinction between a priori and a posteriori is thus understood for Weyl in
terms of fixity and variability, which are themselves explicated in terms of discreteness
and continuity. Internal, fixed natures are knowable a priori and are either discrete or the
sharply defined boundaries of continuous ranges, and external, changeable orientations
are knowable only a posteriori and involve variation within those continuous ranges.

5.2.4. Uniqueness?

The status of the laws of nature themselves in the scheme outlined in the previous
section is uncertain. In the case of the spacetime metric, Weyl seems to have considered
the field equations to be distinct from the nature of the metric. After determining the a
priori nature of the subject of study, the task was to find the correct field equation that
was compatible with that nature, preferably by variation of an action magnitude (see
Section 2.3.4 for discussion of Weyl’s attempts to do this for his unified field theory).
Criteria such as simplicity might provide a useful guide to finding the correct law, but he
conceded they would not lead the theoretician to it algorithmically.

Natures are typically the domain of ontology, and if we understand Weyl’s use of
the term “nature” in an ontological sense, then his standpoint becomes counterintuitive in
the sense that ontology seems to be constraining theory, not the other way around. A
better approach would be to consider “nature” in a more transcendent way, where
metaphysics and epistemology could be said to merge. In that case, attributing an a priori
nature to the metric becomes a statement as much about the structure of the universe as
about our ability to cognize about that structure. The requirement that an a priori nature
of the subject under investigation be determined before formulating its governing laws
then becomes a methodological prescription. This viewpoint helps to justify Weyl’s
demand for an infinitesimal geometry. Physical theories have the task of connecting the
spaces of intuition of different subjects; if a cognizing subject’s space of intuition is
identified with a tangent space on a manifold, then, in order for a physical theory to be
coherent and to have objective consequences, it must unambiguously determine how to
connect different tangent spaces—how to allow intersubjective communication. If this
requires the symmetry of the Lorentz group at every point on the manifold, then this must
be the a priori nature of the metric; any alternative should begin by finding another nature
that allows the tangent spaces—different subjects’ spaces of intuition—to be connected.
When, in a 1951 retrospective of relativity theory, Weyl rebukes Einstein and others for
attempting to construct unified field theories by generalizing the formal symmetries of
the spacetime metric, he makes clear that he thinks they are committing just this
methodological error:

The symmetry of the $g_{ij}$ and the $\Gamma^i_{kl}$ has an importance far in excess of the formal
one, namely, that the nature of the metric and of the affine connection is one and
everywhere the same. Instead of jiggering the symmetry, one should search for a
different, richer structure whose nature would likewise have to be the same
everywhere. (Weyl 1951, p. 82)

In other words, the search for the unified theory should be guided by a priori
considerations concerning what could possibly serve as a representation of the space of
intuition.

Weyl is clear that one component of these considerations must be the principle of
sufficient reason (Weyl 1927, p. 118; 1949, p. 159). In some very simple cases, he
argues, the principle is even sufficient to establish specific laws. That claim is motivated by basic symmetry considerations; if a physical state is invariant under some transformation, then the portions of the state mapped onto one another by that transformation cannot evolve differently relative to whatever symmetries it might have. If they do, then there must be some hidden difference—a reason for the divergence. Weyl does argue, however, that this method only applies to very simple situations and in general was overemphasized by Leibniz as path to knowledge.

Despite this reprimand, Weyl uses the principle to deduce a requirement on a final, systematic theory:

The exact laws of nature must not contain any material constants; the latter should be derived from those laws on the basis of the atomic structure of the material under investigation. (Weyl 1927, p. 121; 1949, p. 162)

When the laws of the intermediate, constructive phases are shown to be “mere apparitions” of the reality of the systematic level, the constants appearing in them should be derived from first principles; they should not require determination through measurement. For example, the dielectric constant, the ratio of electric polarization to local electric field strength, is taken to be fixed in Maxwellian dynamics, but shows a dependence on frequency when the field oscillates rapidly, a fact which is readily explained by attributing an atomic constitution to the medium.

In the final appendix to PMNS, Weyl reiterates the injunction against absolute constants with more sophistication. Two constants, he argues, the speed of light and the quantum of action, have been given “radical understanding” by modern theory (Weyl 1949, p. 287). Both are artifacts of our choice of measurement standards, and relativity theory and quantum mechanics show us how their values can be reduced almost purely to
convention (the only restriction being that they cannot be made to vanish). But no similar move is available for the charge of the electron. He relates this value to the fine structure constant, a pure number equal to approximately $\frac{1}{137}$, which he argues must be derived from first principles:

A complete theory ought to account for this value [of the fine structure constant] by mathematical reasons—just as geometry predicts the value $\pi = 3.1415\ldots$ of the ratio between circumference and diameter of the circle. Whatever Eddington may have thought, no such theory is available today. (Weyl 1949, p. 287)

The masses of the elementary particles require similar explanation, although the fact that they differ for each particle indicates they are of less fundamental character. He separates that problem into two pieces; first, one has to account for the fact that every instance of a given elementary particle has the same mass and charge, and second, why those quantities have the values they do. The first problem is explained by the existence of some (unknown) adjustment mechanism (see Chapter 3). The second is partly explained by quantum mechanics, in that the quantization of the field equations limits the possible solutions to a discrete set. But the particular values remain unexplained.

A theory with unspecified scalar constants is one that can be modified in a continuous fashion, something Weyl associated with a posteriori orientations, not a priori natures. His prohibition of scalar constants in laws suggests that he thought the laws defining the “complete” or “exact” theory belonged on the a priori side of the ledger. However, laws can be incapable of continuous variation and yet still not be uniquely determined; they might be specified only up to unknown integer parameters, or up to selection from a discrete set of symmetry groups. In those cases, the ultimate theory
would be what Steven Weinberg calls “logically isolated,” but not unique. Appeal to experience might still be required to know which among a discrete set of possibilities is the correct theory. One might worry that this amounts to a violation of the principle of sufficient reason—e.g., nothing prevents us from asking, “What reason is there for \( n = 3 \) instead of \( n = 62 \)?”. Weyl is cagey in *PMNS*, both in 1928 and in 1949, about whether the laws of nature should be uniquely determined a priori, but in “Geometrie und Physik” he offers a very clear statement:

> One will be content, definitively, only with a theory that leaves no room for play, and in which instead the action magnitude governing the processes of nature arises on purely mathematical grounds as the only possible one. (Weyl 1931, p. 343)

A theory that “leaves no room for play” cannot be varied, continuously or discretely, but is rigidly fixed, precisely the kind of logical completeness that Einstein lauds in principle theories (see Section 5.1.3 above). Whether or not Weyl believed that such a theory would be attainable in practice, we can conclude that he thought a physicist could not be satisfied without it, i.e., that such a theory was the grand aim of physics, and of science in general.

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81 In *Dreams of a Final Theory*, Weinberg, a co-discoverer of electroweak theory, writes this about the search for a theory of everything: “In my view, our best hope along this line is to show that the final theory, though not logically inevitable, is logically isolated. … In a logically isolated theory every constant of nature could be calculated from first principles; a small change in the value of any constant would destroy the consistency of the theory. … In this case, although we may still not know why the final theory is true, we would know on the basis of pure mathematics and logic why the truth is not slightly different” (Weinberg 1992, p. 236).
5.3. Conclusion

In the investigation of Einstein’s and Weyl’s scientific methodologies presented above, particularly concerning the ultimate aim of science, two common themes have emerged; both believed that scientific reasoning is inherently unificatory, and that as science becomes more unified the logical structures representing phenomena tend to become more rigid, less adaptable to particular theoretical needs. This reflects a common worldview in which nature is seen as an objectively existing, coherent unit, one that can be made sensible through distillation into logical-mathematical structure, but also one that resists efforts to be made to conform to idiosyncratic beliefs and predilections. The first of these sentiments, the rationality of the physical world, has a long history, but the second, the theoretical rigidity of the world, is somewhat more novel. Both Einstein and Weyl stress rigidity as an important methodological guide, especially as physical theory becomes more abstract and covers more and more of the natural world. It captures well their common belief that fundamental truth possesses an inner harmony that cannot be manufactured, only discovered and revealed.
In Chapter 4, I showed that a common line of reasoning can be distilled from the debate between Weyl and Einstein over Weyl’s 1918 unified field theory; both argued that assumptions about the behavior of measuring devices should be employed only provisionally, eventually to be discarded in favor of derivations from first principles. The general idea is that the connection between theory and experience should not be arbitrary, but should be justified on the basis of the theory itself. However, as I pointed out in Chapter 4, Weyl and Einstein’s demand concerning the behavior of measuring devices leaves the task half-finished. One must also justify why a given measuring device can be used at all; that is, one must account for its observability. In the spirit of Weyl and Einstein, I will demand that this justification be derived from the first principles of the theory at hand. I will argue that this cannot be done without taking into account the physical constitution of the theorizer considering and evaluating the theory.

The argument will proceed in four parts. First, I will outline the task more carefully; the “problem of empirical content” will be articulated and then shown to divide into two distinct problems, only one of which is addressed by the Weyl-Einstein reasoning. Second, I will clarify one of the motivations of the Weyl-Einstein reasoning, namely that theoretical representations describe their subject matters completely; in order to do this, I will utilize a concept that is common in physics but that has so far not been
given adequate treatment in the philosophical literature, the degree of freedom, which will then prove useful in the remainder of the analysis. Third, I will examine in detail how the Weyl-Einstein reasoning can be implemented in practice; this requires a particular relationship between a theory’s primary degrees of freedom and those accessible to observation. Finally, I will examine how the degrees of freedom accessible to observation might be derived from a theory’s first principles; I conclude that this can be accomplished free of arbitrary stipulation by including subject-matter-complete representations of the theorizer’s sense-organs within those representations used to test the theory.

6.1. The problems of empirical content

6.1.1. Representations and kinds of content

There are many important and interesting questions that can be asked about how theories relate to the world. Some are ontological: What is theory? What is world? Some are more broadly metaphysical: What exactly determines the relation between a theory and its domain? How does it come to be? Others are epistemological: How do we come to know about this relation? How do other beliefs affect our understanding of or access to that relation? Still others or socio-linguistic: How is the relation expressed? Can it be understood by individuals, or is it somehow dependent on interactions within epistemic communities? The question I will focus on here is largely empirical: How is the relation between theory and world made capable of test through observation?
To begin to answer this question, skeletal answers to some of the preceding questions about the theory-world relation are needed; in order to maximize the generality of the subsequent conclusions, I will attempt to keep these preliminary assumptions as minimal as possible. The first task is to distinguish theory and world; here I will do no more than recognize that theories are constructed and mind-dependent, and the world given and, in at least some regard, mind-independent. While this choice of terminology is decidedly realist, I do not think the antirealist or even the neo-Kantian should be too bothered by it, at least not here. Only the solipsist will contend that there is no given, mind-independent world whatsoever. The realist, antirealist, and neo-Kantian will, of course, disagree about what aspects of the given, mind-independent world can be justifiably represented through theoretical construction; this is simply one axis of debate about the theory-world relation, though not the one I will focus on here. I will adopt a broadly realist point of view, and, although I think that much of what follows could be reconfigured from a neo-Kantian perspective, this is not essential to the analysis. I will indicate shortly where I think the antirealist would object.

The second task is to be more precise about the nature of theory and its use in scientific practice. Since I am concerned here with an aspect of the representational capacity of theoretical constructs, I will focus specifically on representations; I take a theoretical representation to be roughly, in part or in whole, a solution or class of solutions of the fundamental equations of a physical theory. Given a suitable axiomatization of the theory, representations might also be characterized as aspects of models or classes of models in the logician’s sense. Whether such representations or models should be understood as purely extra-linguistic entities, as the proponents of the
so-called “semantic approach” would have it, or as retaining some language-dependence (as I am inclined to believe) should not impact the analysis presented here. However, I will assume that representations, models, and physical theories are not entirely linguistic, and that their content is not exhausted by the physical mode used to express and communicate them (e.g., written marks, verbal utterances, hand motions, etc.). I take the question of the nature of that excess, “semantic” content to be little different in mathematics than in physics.\(^{82}\)

In short, I take theories to be tools for the construction of representations; theorizers construct scientific representations of natural phenomena by means of theories. How exactly this construction occurs, what the end-product consists of, and how it is that we as theorizers can understand the end-product to bear a relation to an independently-existing world (that is, to represent), are questions that require for answers detailed philosophies of mind, mathematics, and language. I will take for granted that such

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\(^{82}\) Questions concerning the nature of physical theories, of scientific representations, and of scientific models have been increasingly active ones in the philosophy of science literature in recent years, as the descendants of the semantic approach have gone forth and multiplied. That approach, conceived initially as a reaction to the purely syntactic view attributed to the later logical empiricists (to Carnap, in particular, whether fairly or not) and usually credited to Patrick Suppes (1960), is known most widely through the works of van Fraassen (1980; 1989) and Fred Suppe (1989). The basic idea, that theories should be identified with classes of models satisfying a set of axioms, rather than with the axioms and their theorems, has been developed in different directions by several different programs. The most prominent, at least in English-speaking circles, is the partial structures view championed by Newton Da Costa, Steven French, and James Ladyman, in which theories are held to assert the existence of relations defined over only part of their potential domains (see Da Costa and French 2003; French and Ladyman 1999). A rather different program, one self-consciously aimed at a formal metatheory of physics, has developed out of the work of Joseph Sneed, Wolfgang Stegmüller, and Erhard Scheibe; for a review, see Schmidt (2002). A strong criticism of the semantic approach in general can also be found in the literature, led by Mauricio Suarez and Nancy Cartwright; they argue that the focus on the content of seemingly fully-formed scientific theories is misplaced, and that the process of discovery and development of theory is more interesting and more important for understanding the content of science (see Cartwright, Shomar and Suárez 1995; Suárez and Cartwright 2007).

The term “model” has many uses in science beyond that employed here; for an excellent review, see Frigg and Hartman (2006). For a very cogent discussion of the epistemological problems concerning scientific representation (as well as an argument for why we should not worry too much about them), see Callender and Cohen (2006). And finally, for a remarkably comprehensive and lucid discussion of the many technical ambiguities surrounding the notion of structure in mathematics, see Reck and Price (2000).
answers are possible, though without assuming much about the particularities. I will do this by taking as given that scientific representations have *semantic content*:

**semantic content**: what a representation says is true about the world.

I will assume that we can in fact construct theoretical representations that have semantic content and that we can understand them as such. I will not analyze the notion of semantic content further, except to clarify its specification in physics (as a collection of degrees of freedom; see Section 6.2.2).

Nothing in the definition of semantic content refers to observation. A theorizer with no epistemic access to the world is perfectly capable of constructing representations and believing in (or at least to contemplating) their accuracy, even if such representations are bound to be wildly inaccurate. It is when the theorizer wishes to test the accuracy of the representations he or she creates that observation is required. It is by no means obvious, and even highly doubtful, that every aspect of a representation will be capable of test through observation. We should thus distinguish a second kind of content, one that allows properties postulated as part of a representation to be compared with or determined through observation:

**empirical content**: what can be determined about a representation through observation.

The empirical content of a representation is clearly a subset of its semantic content. Whether the semantic content is exhausted by the empirical content is a question that cannot be answered directly; doing so will require a more detailed discussion of the nature of observation. This is where the antirealist will disembark; the antirealist believes
that scientific representations have no content beyond the empirical, being nothing more than consolidated expressions of data. In contrast, I take representations to come into being with a different kind of content—semantic content—by virtue of being constructed and being understood as representations. They have empirical content only secondarily.

The central question that this chapter is intended to address can now be refined. The initial, intuitive idea was to ask how something we construct inside our minds can make claims about external things that are capable of test through observation. In light of the distinction between the two kinds of content, this question can be made more precise: How is the empirical content of a theoretical representation to be identified within its semantic content? If we consider that representations are constructed initially with only semantic content, then the question can be put more dramatically: How do representations gain empirical content? This is the problem of empirical content.

As discussed in Chapter 4, this was a problem that concerned Weyl, Einstein, and especially the logical empiricists. All of them viewed the problem as attendant upon the axiomatic approach, which begins with an uninterpreted formal system. The logical empiricist solution was to stipulate the connection between the formal system and observation via coordinative definitions; one simply (and ineluctably) designates that elements of a representation constructed within the system correspond to objects or phenomena that can be directly observed. Empirical content then flows into the formal system by way of the coordinative definitions. For example, in general relativity, measuring rods and clocks are assigned to the spacetime metric in solutions of the Einstein field equation; it is supposed to be intuitively clear what can serve as a measuring rod or clock and how to pick out such objects in practice. Both Weyl and
Einstein rejected this solution, arguing that stipulations should be employed only provisionally, until more detailed accounts are available. Ultimately, Weyl and Einstein both argued, the ability of measuring devices to behave as measurers should be derived from the first principles of the theory. However, as I argued briefly in Chapter 4 and will further clarify in the next section, this can provide only half the needed solution.

6.1.2. Two problems

That the problem of empirical content divides into two distinct problems can be gleaned from the definition of empirical content given above. To determine the empirical content of a representation, one must figure out both what counts as “observation” and how what is observed goes about its “determining.” The latter was the locus of Weyl’s response to Einstein (see Section 4.2); Weyl argued, and Einstein ultimately agreed, that the ability of measuring devices to perform their duties as measurers must be derived from the first principles of physical theory. Left unaddressed, however, is the question of why the particular measuring devices should be the focus of attention in the first place. Why should we use measuring rods and clocks to test general relativity? There is an easy and mundane answer: we use these devices because we can and because they work. That is, they have properties to which we have access—properties accessible to observation—which can be used to determine the properties of the underlying metric field.

If we wish to give a complete description of how a measuring device can be used to give a theory empirical content, then we must do two things:

1) Identify properties of the measuring device that are accessible to observation,

2) Show how those properties determine the fundamental properties we wish to measure.
The Weyl-Einstein reasoning addresses the second task, demanding that it be solved through derivation from the primary principles of the theory at hand; in Section 6.3, I will examine how this can be accomplished. The first task, left open by Weyl and Einstein, will be the subject of Section 6.4. In the spirit of their common reasoning, I will demand that it be accomplished without stipulation. That is, given some theoretical representation, the aspects of the representation that are accessible to observation should be identified as such through derivation from the first principles of the theory under consideration.

The twofold nature of the problem of empirical content is directly related to an ambiguity in the use of the term “measurement.” In one sense, “measurement” can take place entirely independently of theoretical considerations. If the general relativistic metric field really does exist, and clocks really do interact with the local metric in the way envisaged by theorists of general relativity, then clocks can indeed measure an aspect of the spacetime metric (the proper time of their worldlines), regardless of whether a physicist is around to read the clock or to theorize about its behavior. If these objects, the clock and the spacetime metric, exist independently of minds, then they can certainly interact with one another independently of minds. In another sense, however, “measurement” requires a theoretical representation; a particular interaction can be said to result in a determination of the properties of the general relativistic metric field only by theorists of general relativity. It is only after the properties of the local metric field are postulated as part of a theoretical representation that those properties can be said to be measured. Measurement in the first sense, independent of theorizers, can be said to happen only within an already-given theoretical framework; that is, to make the
measurement claim sensible, one must presuppose that such and such theoretical entity
really does exist, and this requires a theoretical context. Thus, it is measurement in the
second sense, dependent on theoretical representation, that is the more fundamental.

Consider that clocks are capable of measuring both the general relativistic metric
and Newtonian absolute time. A computer could record clock readings just as easily in
either case. Since simply recording numbers is the same activity in both cases, the
recording itself cannot be what distinguishes measurement of the general relativistic
metric from measurement of Newtonian absolute time. It is not until a theorizer of either
general relativity or Newtonian mechanics constructs a representation of the
measurement interaction that a measurement of some particular property can really be
said to have occurred; prior to the construction of the representation, no one has yet
postulated a property that could understood as being measured. Note that it is the
theorizer’s ability to construct representations by means of the theory under consideration
that is crucial, not the ability to theorize in general. An untrained but sentient assistant
could play the role of the computer, recording clock readings without comprehension;
only an expert could justifiably claim that a measurement of the desired property had
been performed.

If we focused on only the first, more restricted sense of measurement, we would
conclude that, in order to give theoretical representations empirical content, we need only
accomplish the second task above, that of describing the behavior of measuring devices.
But consideration of measurement in the fuller sense reinforces the need to accomplish
the first task as well. Furthermore, it points the way to a clarification of the notion of
“observation” invoked there; the properties from which the empirical content of a
representation originates are those accessible to observation by the theorizer constructing the representation. This requires that the theorizer interact with the objects/phenomena being observed, which in turn requires that the theorizer be representable, in part if not in whole, by means of the theory; a derivation of the observability of measuring devices from first principles will thus require a representation of that portion of the theorizer that exists within the domain of the theory. At the very least, this will include the theorizer’s sensory devices, or sense-organs.

From these last reflections, we can distill a kind of credo: there is no such thing as observable simpliciter. To say that an element of a theoretical representation is observable is to say that there is some physically realizable scenario in which the theorizer constructing the representation, understood to have some specific physical constitution, can know the properties of that element. A similar clarification is made by van Fraassen in the Scientific Image; there, he argues that “observable is observable-to-us” (van Fraassen 1980, p. 19). He, too, thinks that observability should ultimately be derived:

The human organism is, from the point of view of physics, a certain kind of measuring apparatus. As such it has certain limitations—which will be described in detail in the final physics and biology. It is these limitations to which the ‘able’ in ‘observable’ refers—our limitations, qua human beings. (van Fraassen 1980, p. 17)

What van Fraassen is missing is mention of why human beings are relevant to the discussion of the content of physical theories in the first place; as my analysis makes
clear, it is because (and only because) we are the constructors and evaluators of those theories.\(^83\)

Here I have shown that the problem of empirical content can be divided into two distinct problems, one concerning the ability of measuring devices to behave as measurers, and the other concerning their suitability for use by theorizers constructing representations of the entities to be measured. I have demanded, in the spirit of Weyl and Einstein, that these problems be solved through derivation from first principles. What remains now is to show how such derivations are possible. For that, a bit more needs to be said about how representations are constructed and evaluated in physics.

6.2. Completeness and degrees of freedom

A straightforward motivation for the Weyl-Einstein reasoning is the desire to have theoretical descriptions that account for all of the features of the phenomena they are taken to represent (see Section 4.2). For example, the assumption that measuring rods are rigid should be avoided because actual measuring rods simply fail to be rigid. A vector in a vector space, though perhaps a decent first approximation, is ultimately unsatisfying

\(^83\) Other notable accounts of the nature of observability include Kosso (1989) and Radder (2006), who both also provide useful historical reviews of the development of observability concept. Kosso draws explicitly on van Fraassen in forming his “interaction-information account,” in which observation involves a flow of information from observed to observer through a physical interaction (or chain of interaction). This amounts to “a precise method for realizing the suggestion to allow physical theory to adjudicate matters of observability” (Kosso 1989, p. 4). However, as with van Fraassen, Kosso does not emphasize the role of the theorizer as I do here. More recently, Radder stresses that observation requires both “material realization” and “conceptual interpretation.” He describes humans as “self-interpreting observational instruments” (Radder 2006, p. 87). Although Radder’s account is similar to that offered here in its emphasis on the connection between theorization and observation, his ultimate goal is rather different; he is concerned with the origin and necessity of the concepts used in science, whereas I take the semantic content of scientific representations as given.
because one can easily discern complexities in real, physical rods that are not captured in that particular mathematical structure. It is an *incomplete* representation.

In attempting to give a precise characterization of this completeness requirement, I will leave aside the interesting epistemological question of how we come to know whether a representation is complete or not; instead, I will take for granted that we *can* do so, and take the measuring rod as a paradigm case. It is simply clear that *something is missing* when a vector is used to represent a measuring rod.

### 6.2.1. Intuitive motivation—Something is missing

There are two ways in which the vector representation\(^{84}\) of a measuring rod could be said to be missing something. On the one hand, it is too primitive; actual rods are easily seen to be composite objects, whereas vectors are not. On the other hand, actual rods take part in interactions that seemingly cannot be represented even by a more complicated arrangement of vectors. In the first case, one could say that there is a deficiency in the number of degrees of freedom in the representation; in the second, that there is a deficiency in the kinds of degrees of freedom available to the representation. As we will see below, this distinction is not as sharp as it might seem, but it does have some intuitive appeal. It maps roughly on to the distinction between the number of elements in a solution and the kinds of forces operating within it.

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\(^{84}\) One might quibble that, at least in special relativity, spatial intervals are the appropriate representation of measuring rods, not vectors. But the spatial portion of a Minkowski spacetime, since it is Euclidean, can be construed as a collection of vector spaces, each formed by a set of spatial intervals with a common endpoint (shrunk infinitesimally and allowed to reorient relative to one another, these become the tangent spaces of general relativistic spacetimes).
The idea that a representation has an insufficient number of degrees of freedom is relatively straightforward. The measuring rod can be broken into constituent parts, whereas a vector cannot. One can, of course, decompose a vector into a sum of other vectors, but the thus-decomposed vector is not less primitive than the others for it; all vectors enter into the mathematical apparatus on the same footing. Another obvious deficiency in the representation is that it is one-dimensional, whereas actual measuring rods are three-dimensional. Both of these assumptions, primitiveness and one-dimensionality, are useful idealizations in many contexts, but this is just another way to make the point. Degrees of freedom apparent in the phenomena of measuring rods are being deliberately ignored.

The idea that a representation should contain the right kinds of degrees of freedom is one that Weyl used in responding to Einstein; measuring rods are held together by electromagnetic forces, he argued, and thus a theory that does not account for those forces, such as general relativity, cannot provide the complete solution needed to represent the rods properly. Even if we attempted to model a measuring rod in general relativity by a linear arrangement of localized mass distributions, the argument goes, we could never recover the observable behavior of actual rods, since we know that matter contains important electromagnetic degrees of freedom, not just gravitational ones. Gravitational interactions are always attractive, for example, whereas electromagnetic interactions are sometimes repulsive. However, one should be very careful with this sort of reasoning; it requires a demonstration that electromagnetism could never be shown to arise as an effective feature of general relativity, i.e., that some (perhaps exquisitely) complex combination of gravitational degrees of freedom could not mimic
electromagnetic degrees of freedom. There could, for example, be some bizarre spacetimes in which matter appears to repel. Far-fetched, to be sure, but then solutions like the Gödel spacetimes were largely unfathomable before their discovery. Similarly, the gravito-magnetic forces in general relativity generated by rotating masses were likely equally difficult to imagine on the basis of Newtonian gravity. And it is impossible to rule out, however unlikely, the possibility that a successor theory to general relativity, one that might appear to describe only gravity, could have the resources to produce electromagnetism or even the other forces in some effective regime.

Whether or not a clean separation between the number and kinds of degrees of freedom is possible, together they constitute the “something” that is missing in the theoretical representation of measuring devices invoked in the Weyl-Einstein reasoning. In the next section, I will attempt to clarify the degree of freedom concept, so that the desired notion of incompleteness can be stated more precisely.

6.2.2. Degrees of freedom

As mentioned above, the degree of freedom concept is an important tool in the theoretical physicist’s toolkit, one that has so far received little direct attention in the

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85 In a Gödel spacetime, the entire universe appears somehow to rotate and to allow time travel into the past in a self-consistent manner; see Hawking and Ellis (1973).

86 Although it is worth noting that one could read this suggestion into Mach’s famous critique of Newton’s bucket thought-experiment (Mach 1893, p. 232).

87 Indeed, the five-dimensional versions of general relativity developed by Kaluza (1921) and Klein (1928) in the 1920s have precisely the needed resources to describe electromagnetism. For more on Kaluza-Klein, see Penrose (2004, Section 31.4).

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philosophical literature. I will not attempt a complete or comprehensive characterization here, rather an informal discussion followed by a semi-technical definition. The notion of a degree of freedom originates in classical mechanics, takes prominence in statistical mechanics, and remains a core feature of modern gauge theory. In all of these cases, the degrees of freedom of a system are typically understood as the number of scalar quantities needed to characterize its state.

In classical mechanics, a particle moving in a three-dimensional space requires six quantities, and the most straightforward way to provide them is with one position and one momentum vector. Once these six quantities are determined, all other quantities in the system are fixed; there is no further freedom. If each of them can vary independently, then the system has six degrees of freedom. If there are constraints on the system, so that some quantities either cannot vary at all or cannot do so independently of others, then the system is said to lose degrees of freedom. For example, one can model a pendulum by restricting the motion of a single particle to a circle, leaving it with only one position and one momentum degree of freedom. In a system of many particles, the total number of degrees of freedom are calculated by taking six degrees of freedom per particle and then subtracting the number of constraints.

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88 Thalos (1999) is a notable exception, although her explication is unsatisfactory for its reliance on a somewhat obscure notion of state “shaping.” Batterman (2000, p. 127) offers a simple parenthetical clarification: “microscopic details.”

89 This follows directly from the fact that the equations of motion are stated as second-order derivatives of position with respect to time.

90 There are subtleties involved in enumerating constraints. Due to the laws governing the system (Newton’s laws), restrictions placed on the position degrees of freedom often imply restrictions on the corresponding momentum degrees of freedom, a fact usually seen most easily in the Hamiltonian approach. Also, the constraints might not be independent of one another.
In statistical mechanics, the degree of freedom concept becomes more fundamental. One first postulates a composite system as a large collection of identical subsystems, each constructed in analogy with some relatively simple mechanical system, and then examines the behavior of the subsystems in aggregate. The mechanical systems on which the subsystems are modeled might be mere point particles, or they might be capable of more complicated motions, perhaps with parts connected by springs or able to rotate. One of the novelties of statistical mechanics is that these mechanical details are ultimately insignificant; it is only the fact that the subsystem is capable of variation in certain ways—that it has certain degrees of freedom—that is important. One is free to imagine the subsystems composed of springs and solid bodies if one so chooses, but the theoretical description, made in terms of abstract degrees of freedom, does not require it. These unobservable, or “microscopic,” degrees of freedom are then combined to generate observable, or “macroscopic,” degrees of freedom, such as temperature or total energy. An important point, to which I will return below, is that one can impose constraints on the macroscopic degrees of freedom that do not reduce the degrees of freedom of any of the subsystems taken individually.

In classical field theory as well as its modern descendant, gauge theory, a number of subtleties must be taken into account when counting degrees of freedom. Describing the field is easy enough; one simply postulates a number of quantities characterizing the field at any given point in space (or spacetime). It is the field equations that introduce the difficulties. They imply constraints that remove degrees of freedom, but they also link degrees of freedom at different points in a way that is easily overlooked. All of the field quantities at a given point can usually be varied independently of one another, even if the
field is heavily constrained, by borrowing some of the freedom available at other points. For example, in classical electrodynamics, the electromagnetic field has six components at each point, three each in the electric and magnetic field vectors. In a vacuum, Maxwell’s equations remove four degrees of freedom per point, leaving the field with just two per point; these correspond to the amplitude and polarization of light waves. Yet, at a given point, one is free to construct solutions with any desired electric and magnetic field values, simply by adding the basic light wave solutions together; there seems, then, to be six degrees of freedom at that point, not two. The catch is that one does not have the freedom to do this at all points simultaneously. For the system as a whole (which could potentially include the entire universe), there are only two degrees of freedom per point in the electromagnetic field, even if it can vary six different ways at any given point considered in isolation. An equally important subtlety is that some of the degrees of freedom left intact by the field equations will reflect symmetries of the equations themselves, and will have no impact on observation; these are referred to as “unphysical” or “gauge” degrees of freedom, and are a central feature of modern gauge theory, both quantum and classical.\footnote{The usefulness of the degree of freedom concept in quantum field theory is rather more complicated; the term is used frequently, but usually in reference to aspects of the classical fields prior to the quantization procedure. After quantization, the degrees of freedom of the field correspond to indices on the set of operators that act on the quantum state, not the state itself, which has its own degrees of freedom. The two sets of degrees of freedom are related, but they are not the same. The topic is interesting and worthy of further study.}

One can identify two distinct ways of using the term “degree of freedom” in the preceding discussion: 1) to refer to all the ways a system might vary, and 2) to indicate how the different aspects of a system might vary independently of one another. A system might have a large number of apparent degrees of freedom, but only a few independent...
ones. For example a solid body in classical mechanics is composed of an uncountable infinity of particles, each with its own position and momentum; the system as a whole therefore has an uncountable infinity of apparent degrees of freedom. However, most of those degrees of freedom are perfectly correlated, and the system as a whole has only nine independent degrees of freedom. The three rotational degrees of freedom of the solid body are in fact the remnants of the lost positional and momentum degrees of freedom of the constituent particles.

A second distinction can also be drawn from the above discussion: some degrees of freedom seem to be fundamental, comprising or describing the basic constituents of the theory, and others seem to be constructed by combining those degrees of freedom together through well-defined mathematical procedures. The first are postulated, and the second are constructed.

With these distinctions in mind, I offer the following definitions:

- **degree of freedom**: a feature of a system that can vary, or undergo change.
- **primary degree of freedom**: a degree of freedom postulated in a theory’s first principles.
- **secondary degree of freedom**: a degree of freedom constructed from the primary degrees of freedom of a theory.
- **independent degrees of freedom**: a set of degrees of freedom that can vary independently of one another for a given system in a given situation.

For example, the electromagnetic field has six primary degrees of freedom per point, but no more than two independent degrees of freedom per point overall. The solid body has six primary degrees of freedom per point on or within its surface, but no more than nine independent degrees of freedom total. When a physicist says that some degrees of freedom are coupled, we should take him or her to say that there are fewer independent
than primary degrees of freedom. In statistical mechanics, the microscopic degrees of freedom are primary and the macroscopic are secondary.\(^92\)

Whether or not a set of degrees of freedom is independent will be determined by the theory and the particularities of the solution or representation in which they appear. If they fail to be independent, then the exact manner of their dependence on one another will be determined by the theory. This is one way to understand how the laws of nature work; they determine how degrees of freedom vary relative to one another—their *manner of dependence*. This variation need not be understood temporally. Temporal variability can be viewed as a particular degree of freedom—not necessarily a primary one—that can be coupled to other degrees of freedom. For example, in classical mechanics, time enters as a primary degree of freedom common to all systems, and it gets coupled to the others by the equations of motion. In general relativity, one can construct a solution by positing a manifold with 10 primary degrees of freedom at each point (the components of the metric tensor); the field equation then governs how they vary relative to one another, and a time coordinate is constructed from them, following the adoption of a coordinate system. The appropriateness of calling the coordinate a temporal one is another matter entirely; this will be discussed in Section 6.3.

The above definition of the basic degree of freedom concept is very general, more so than is common in physics, where the term is typically used to refer only to quantities that range over the real or complex numbers. However, there is nothing inherent in the notion of “freedom” invoked here that requires continuous variability. Whole number

\(^92\) Note that the term “primary” is chosen here for its sense of being first or initial, not of being more important. Similarly for “secondary.”
quantities that characterize properties of systems, such as the energy of a quantum
harmonic oscillator, seem perfectly respectable as degrees of freedom—ways that the
system might vary. All that is needed, once the capacity for variation is recognized, is
that the type of variability also be recognized. The most straightforward way to
understand this is set-theoretic: the degree of freedom ranges over the elements of a well-
defined set. But the possible values might also be specified algorithmically or by other
means. In any case, the analysis presented here does not depend on how the degree of
freedom is understood beyond the simple notion of variability. It is in this sense that the
notion of degree of freedom is more primitive than that of “structure,” at least in the set-
theoretic understanding of the term. One could view degrees of freedom as means of
characterizing structures (for example, the parameters identifying members of a Lie
group), but I find it more helpful to see things the other way around, to view algebraic (or
other) structures as useful for characterizing and identifying the degrees of freedom of a
physical system—the ways in which it might be different.

Finally, there typically will be enormous flexibility in how the independent
degrees of freedom of a system can be characterized. Equations can be manipulated and
new variables can be introduced so that different aspects of a given set of constraints get
simplified or isolated. In statistical mechanics, the macroscopic degrees of freedom of a
system of particles are generated in this way, by defining new quantities—secondary
degrees of freedom—as combinations of the primary ones. For example, the total energy
of a system is the sum of the energy of all the constituent subsystems (the energy of each
particle itself being a secondary degree of freedom, if position and momentum are taken
as primary), and one could take the total energy as an independent degree of freedom.
Likewise, with the solid body, one could take the momentum of a constituent particle (not at the center of mass) as an independent degree of freedom instead of one or more of the components of the total angular momentum, however inconvenient that might be. The point is principally a logical-mathematical one, a consequence of the fact that a set of theorems can usually be axiomatized in a variety of ways, but, as we will see, it is one with wide-ranging consequences for physical theory.\(^93\)

6.2.3. Completenesses

We are now in position to state more carefully the notion of completeness invoked in the Weyl-Einstein reasoning and to distinguish it from the more general completeness required of a theory of everything.\(^94\) The first should be read as a requirement on the relation between a given theoretical representation and its subject-matter, whereas the second is a condition on whole theories. If we take “subject-matter” to mean a class of objects or phenomena, then the following definitions follow readily:

**subject-matter-completeness:** A theoretical representation is subject-matter-complete if there are no degrees of freedom in the objects or phenomena it aims to describe that do not have counterparts in the representation, nor any differences in their manner of dependence.

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\(^93\) One might also argue that the primary degrees of freedom of a given theory can be specified in many different ways. However, this assumes a particular way of individuating theories, one that does not involve the identification of the theories’ ontological primitives (and would for that reason be attractive to the antirealist). The idea does have some intuitive appeal; one would not want to be required to think of electromagnetism based on electric and magnetic fields as completely different from electromagnetism based on the 4-vector potential. On the other hand, for the realist, ontology does matter. The question does not significantly impact the analysis presented here, but it is worth further consideration.

\(^94\) The completeness I consider here is *descriptive*, not *explanatory*. One could require that a theory of everything leave no feature of the world unexplained, including itself; such a theory would have a single, unique form and would be logically necessary (or at least its falsity would be impossible). The notion I work with here is somewhat less stringent, merely that every natural phenomena be representable by means of the theory. Whether either goal is actually attainable is an open question.
**everything-completeness**: a physical theory is *everything-complete* if every observable object or phenomenon has a subject-matter-complete representation in the theory.

Again, I do not concern myself here with the epistemological question of how a theoretical representation is found to be subject-matter-incomplete; rather, I take as given that it can be done. I think there is sufficient looseness in the above definitions that an exact isomorphism between every object/phenomenon and representation need not be expected. In the cases we are concerned with here, the evaluation is more or less clear. A vector in a Euclidean vector space is a subject-matter-incomplete representation of a measuring rod; general relativity is an everything-incomplete theory (as far as can be told).

Note that subject-matter-incomplete representations are possible in an everything-complete theory. One need only take a simple solution of the fundamental equations (or, if there are no simple solutions, then some simple aspect of a solution) and consider it a representation of an object with obvious complexity. The representation will likely be a poor one, but that is just the point. Considerably more difficult is the question of whether a subject-matter-complete representation might be possible in an everything-incomplete theory. One might think it would be, since various degrees of freedom can often be ignored in the use of measuring devices. But if all of nature is deeply interconnected, as many suspect, then one would not be able to capture all of the degrees of freedom of an object or phenomenon without capturing all kinds of physically allowed degrees of freedom. This is the thinking behind total unification, the idea that all fundamental forces should appear as a single, unified force at some energy scale. If, on the other hand, phenomena existed that did not take part in all possible kinds of interaction, as would be
the case if, for example, the photon were purely electromagnetic and did not engage in weak or other interactions, then a subject-matter-complete representation would be possible in a theory with a restricted domain.

I intend to leave unanalyzed here exactly how the evaluation of subject-matter-completeness is made. Describing this would require an understanding of how representations are compared to what is given through observation, i.e., an understanding of the cognitive aspects of theorizing and observing—an interesting question, to be sure, but one best left for philosophers of mind.

6.3. Measuring devices as measurers

In the previous section, I considered measuring devices, the locus of the Weyl-Einstein reasoning, to be no different from any other object. In this section, I will consider them as measurers—objects designed and employed to determine the values of degrees of freedom of the entities they interact with. The Weyl-Einstein reasoning requires that an object’s ability to function as a measurer be derived from within the theory, not stipulated from without. That is, an object’s usefulness as a measuring device should be demonstrated from a theory’s own first principles, not from some intuitive or stipulative designation. As discussed in Section 6.1, there are two aspects to this usefulness; objects are chosen as measuring devices because they have properties accessible to observation and because those properties can be used to determine properties not accessible to observation. In this section, I will show how a theory can demonstrate the latter, using the characterization of representations in terms of degrees of
freedom. This will allow a further refinement of the notion of empirical content, again in terms of degrees of freedom.

6.3.1. The Brown-Butterfield picture

The most notable recent endorsement of the idea that the behavior of measuring devices should be derived and not stipulated comes from Harvey Brown (2005). The idea forms a key part of his critique of the traditional understanding of the causal powers of spacetime geometry. He argues that the local validity of special relativity (as seen in phenomena such as length contraction) is due not to a mysterious interaction between physical objects and the spacetime geometry, but rather to the particular form of the interactions among those objects, expressed as the Lorentz covariance of the equations characterizing them. Put another way, we ascribe a particular chrono-geometrical character to the world around us not because an independently-existing spacetime geometry causes our rods and clocks to behave in a certain way, but rather because the laws of nature do. In the language of general relativity, this means that the 2nd-rank tensor, $g$, at the heart of the theory is not inherently chrono-geometrical, but rather becomes so because, thanks to the laws of nature, it can be measured by rods and clocks, which are considered chrono-geometrical only by tradition. One might say (though Brown does not) that, instead of gaining chrono-geometrical significance, the tensor thereby gains “rods-and-clocksical” significance. Likewise, using light rays and test particles would give the tensor “test-particle-and-light-rayical” significance.95

95 Brown would likely argue that the objects should be grouped according to how they go about their measuring. He stresses that the waywizer, essentially a wheel connected to an odometer, is a good
The language developed in Section 6.2.2 can be used to state the point more precisely and without requiring any apologies for the use of historically-loaded terms. Rods and clocks have degrees of freedom accessible to observation; these are not independent of the primary degrees of freedom in the underlying field, and can therefore be used to determine them.\(^\text{96}\) To ask about the nature of the primary degrees of freedom is to ask about their mathematical origins; if mathematicians refer to those kinds of degrees of freedom as geometrical because they were originally formulated by generalizing what has historically been called geometry, then that is as good a reason as any for physicists to do so as well. Brown’s overall point remains, and it is at the heart of the Weyl-Einstein reasoning; the relationship between the primary degrees of freedom and those accessible to observation is purely mathematical, determined solely by the theory’s fundamental equations. Because the two sets of degrees of freedom, the primary and those accessible to observation, are not independent, each can be characterized, at least in part, in terms of the other. The task of determining this two-way relationship—primary-to-observable and observable-to-primary—cannot be stipulated away. The question of how the degrees of freedom accessible to observation are identified in the first place is a separate question, which I will take up in Section 6.4; it is worth noting that Brown, like Weyl and Einstein, does not address it.

In a recent article, Jeremy Butterfield outlines the task in a way that makes its two-directional nature explicit. After noting, à la Weyl and Einstein, that “one has a right

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\(^\text{96}\) Note that it does not matter whether the underlying field is viewed as constituting the rods and clocks (as in the geometrodynamical perspective), or as interacting with their primary degrees of freedom (i.e., coupled to them).
to expect a theory of how the [measuring] instrument works,” he describes how such an expectation would be met:

An account that filled this lacuna would proceed: from (i) the postulation of fundamental metric and matter fields on spacetime; to (ii) a theoretical description of how (at least idealized) rods and clocks (or whatever else) behave, powerful and accurate enough to secure that: (iii) such rods and clocks (or other matter) would indeed do their stuff—their pointer-readings report the metric. (Butterfield 2001, p. 8-9)

In the language developed here, these three steps read as follows, generalized for arbitrary theories and measuring devices:

1) A theory’s first principles, along with the kinds of primary degrees of freedom they specify, are identified and postulated.

2) A representation of a measuring device, in terms of its observed degrees of freedom, is constructed from the theory’s primary degrees of freedom.

3) The primary degrees of freedom are shown to be characterizable in terms of the degrees of freedom in the representation of the measuring device.

The first step includes both the articulation of the theory as well as its being understood, not just as a mathematical theory but also as a source of potential representations of objects and phenomena; again, I take these sub-steps to fall under the purview of semantic content and refrain from analyzing them here. The second and third steps are the two directions of degree-of-freedom-determination indicated above. Step #2 describes an *ontic* determination; the primary degrees of freedom generate or constitute others, in this case those accessible to observation. Step #3 describes an *empirical* determination; observation provides information that is then transferred to other parts of the theory. Mathematically, however, there is no difference; each determination is simply a matter of characterizing one set of degrees of freedom in terms of another. The
philosophical difference stems from the fact that one set is understood as given and the other as accessible to observation. To say that rods and clocks read the metric, then, is to say that they are ontically determined by and empirically determinate of the metric tensor. *Both* directions are necessary to explain how measuring devices can function as *measurers.*

To say that one set of degrees of freedom determines another is to say that there is a well-defined mapping from the possible values of one set to those of the other; put another way, the variables representing one set can be written in terms of the variables representing the other. The mapping can be one-to-one, in which case an inverse mapping exists, or many-to-one, in which case there is no inverse. The most desirable scenario is that there is a one-to-one mapping between the possible values of the primary degrees of freedom of a given system and the degrees of freedom accessible to observation of that system or of another one interacting with it. If ontic determination exists without empirical determination, then the device cannot function as a measurer of the theory’s primary entities. Complete empirical determination is not always possible, however, as evidenced by gauge theories. In that case, the possible values of the primary degrees of freedom are grouped together into classes that can be mapped one-to-one to the possible values of the degrees of freedom accessible to observation. Note that empirical determination can only exist without ontic determination if the representation is not constructed solely on the basis of the first principles of the theory (i.e., some external

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97 Note that the degrees of freedom understood as being empirically determined (i.e., measured) need not be primary, although, as with all secondary degrees of freedom, they must be constructed from primary ones.
structure is added), or if some of the primary degrees of freedom used in the construction are ignored.

The Brown-Butterfield picture, as I have laid it out here, serves as a clarification of the Weyl-Einstein reasoning. The demand that the behavior of measuring devices as measurers be derived from first principles is understood as a requirement of mutual determination between what is primary in a theory and what is accessible to observation, at least in regard to the theoretical representations of the measuring devices and what they purport to measure. By making more precise the notion of empirical determination, this picture will allow a refinement of the notion of empirical content in terms of degrees of freedom. A concrete example will help demonstrate this, as well as illuminate the task that remains—to explain why certain degrees of freedom in a representation should be considered accessible to observation in the first place.

6.3.2. Light-rays and test-particles

The preceding account makes clear why simpler measuring devices are better; the fewer degrees of freedom in the representation, the easier the mapping between them and the primary degrees of freedom will be to construct. It was partly for this reason that Weyl sought an account of measurement in relativity using only light-rays and freely-falling test-particles, objects/phenomena with far fewer observed degrees of freedom than rods and clocks. He identified particular mathematical structures in the theory, null geodesics and timelike geodesics, with the paths of light-rays and test-particles, respectively, and then showed how the metrical structure of spacetime is fixed by them. In the terms used here, the degrees of freedom of the metric tensor are written in terms of the degrees of freedom associated with collections of worldlines. Whether this is
possible or not is a purely mathematical question; the geodesics are well-defined by the
geodesic equation (or, equivalently in Riemannian geometry, by minimizing the path
integral), and the question of whether a certain, finite combination of them can fix the
components of the metric tensor at a point is well-posed and has been definitively
answered in the affirmative. What remains, in line with the analysis of Section 6.1.2,
are two distinct questions:

1) Why should we consider light-rays and test-particles to be observable?

2) Why should we think that light-rays and test-particles traverse geodesics?

The second is clearly a question that should be answered by physics; I will argue in
Section 6.4 that the first is, too.

The demand that question #2 should be answered by physical theory is another
instance of the Weyl-Einstein reasoning. The behavior of these objects/phenomena
should not be stipulated, but rather derived from first principles, just like rods and clocks.
A representation of light-rays and test-particles should be constructed from the primary
degrees of freedom of the theory and then shown to behave in the expected way. The
obvious problem for general relativity, at least concerning light, is that the theory does
not incorporate electromagnetism, and so cannot (or at least is unlikely to be able to) give
a subject-matter-complete representation of a light-ray. It cannot even explain why light
moves at the speed it does, the crucial property for being and staying on a null geodesic.
However, Maxwellian electrodynamics can be ported to the curved spacetimes of general

98 The classic demonstration of how worldlines can be used to fix the metric is given by Ehlers,
Pirani et al. (1972). Misner, Thorne, et al. (1973, chapter 16) give a more comprehensive account that
includes discussion of some less common measuring devices. A more detailed discussion of Weyl’s use of
light-rays and test-particles can be found in Ryckman (2005, p. 85-6).
relativity, and it is relatively straightforward to show that, in the appropriate limit (known as the geometric optics limit), light-rays do in fact traverse null geodesics.\textsuperscript{99} The cobbled together of the two theories is not aesthetically pleasing, and certainly fails to form a theoretically rigid\textsuperscript{100} theory, but it comes much closer to providing a subject-matter-complete representation of electromagnetic radiation than a simple null vector in general relativity. Furthermore, taking one theory (Maxwellian electrodynamics) as unproblematic background in order to allow empirical determination of the elements of another theory is standard practice in physics. It is only in the context of a theory of everything that such a tactic is unavailable.

The status of the claim that test-particles follow timelike geodesics, long known as the “geodesic hypothesis,” is rather more interesting, as it can be evaluated without any additional background theory. At first, Einstein assumed that the hypothesis had to be postulated independently of the field equations, but he later published collaboratively a derivation of an equation of motion for matter from the field equations (see Einstein and Grommer 1927).\textsuperscript{101} Since then, many others have found similar derivations, with a variety of different starting points and assumptions about the matter distribution.\textsuperscript{102} All of the derivations assume a finitely extended matter distribution (i.e., not point-like), and

\textsuperscript{99} See Brown (2005, p. 163-5) for a concise proof of this. He suggests that the derivation has been appreciated as far back as Eddington (1923).

\textsuperscript{100} See Chapter 5 for a discussion of theoretical rigidity.

\textsuperscript{101} Others, including Weyl, worked on the problem earlier, though Einstein was apparently unaware of their work. The somewhat messy early history of the geodesic hypothesis is chronicled in Havas (1989).

\textsuperscript{102} Misner, Thorne, et al. (1973, p. 471-480) provide an elegant technical presentation of one of these proofs. Brown (2005, p. 161-3) and Butterfield (2007, p. 22) offer characteristically concise reviews of several others.
all conclude that adherence to motion on timelike geodesics is not exact, but rather approximate. For example, the center of mass might deviate slightly from a geodesic if the local field varies strongly enough. Indeed, as the field gets even stronger, the distribution might not even retain its shape and could cease to be a good representation of a simple particle.

Question #2 thus has a reasonably good set of answers; once we recognize that the radiative solutions of Maxwell’s equations on curved spacetime do indeed represent the optical phenomena we observe, and that localized matter distributions do indeed represent the freely-falling test-particles we observe, then the empirical determination of the metric tensor $g$ is entirely unproblematic. The degrees of freedom in $g$ are successfully characterized in terms of the degrees of freedom accessible to observation in the representations of those objects and phenomena, representations that were themselves constructed from the primary degrees of freedom of the theory. We have not yet answered question #1; as I will argue below, this requires much more to be said about the observer.

6.3.3. Empirical content in terms of degrees of freedom

As defined in Section 6.1, the empirical content of a theoretical representation is that which can be determined about it through observation. This stands in sharp contrast to what the representation asserts to be real, a component of its semantic content. The primary degrees of freedom serve as representations of the ontological primitives described by the theory; the manner of dependence of those degrees of freedom on one another characterizes the laws of nature governing those primitives. These are the defining postulates of the theory, and understanding them is a matter of understanding the
basic meaning of the theory—its semantic content. How the theory relates what it postulates as real to what can be observed, on the other hand, is the domain of the empirical.

The framework employed here, in particular the Brown-Butterfield picture of measuring devices, allows for a further refinement of the notion of empirical content in terms of degrees of freedom. Once a set of degrees of freedom accessible to observation is identified, there will be a large number of other degrees of freedom that can be determined by them; indeed, it might be that every degree of freedom in the theory (or class of solutions under consideration) can be so determined, though not necessarily at the same time. It is in this way that the notion of empirical content can be given more precise expression:

**empirical content, refined:** Given a set of degrees of freedom identified as accessible to observation, the empirical content of a representation, relative to that set, will be whatever sets of degrees of freedom can be determined by it, along with their manner of dependence on one another.

There will be enormous freedom in characterizing that content (i.e., many different sets of degrees of freedom determinable), thanks to the enormous flexibility provided by the degree of freedom concept. The content will be “about the world” to the extent that the degrees of freedom accessible to observation are part of representations of objects or phenomena. But there is no need to characterize the content in terms of the exact degrees of freedom in the representation; others constructed from them, or even the primary ones they are constructed from, will do. Again, this is principally a logical-mathematical point concerning the freedom involved in characterizing logical or mathematical structures. It is in this sense that it is as fair to say that general relativity is about light-rays and test-particles as about whatever (call it “spacetime,” if so desired) is represented by the metric
tensor, whose degrees of freedom are empirically determined by those objects/phenomena. The empirical content can be characterized equally well either way. If one is more comfortable saying that a theory is about whatever is represented by its primary degrees of freedom, then so be it.

However the empirical content of a theory is characterized, it must have some dependence on the kinds of objects and phenomena used for the empirical determination, even if the degrees of freedom in their representations can ultimately be eliminated from the characterization of that content. The dependence might be very weak for that reason, but it cannot be entirely eliminated; a given theory could potentially allow almost any object or phenomenon to be used in the empirical determination of its primary degrees of freedom, but it cannot do without mediation through objects or phenomena altogether. In other words, theories do not possess empirical content ex nihilo, but always because of the possibility of observations of physical processes.

The Brown-Butterfield picture clarifies how empirical determination can work, and how this allows the empirical content of a theory to be characterized in many different ways, but it leaves a crucial element of the story unanalyzed—how the degrees of freedom accessible to observation are identified. In the spirit of the Weyl-Einstein reasoning, I will require that this be done without stipulation. In Weyl’s procedure, however, it seems that one must simply declare that light-rays and test-particles are observable. Reichenbach recognized this and argued that the procedure was for this reason little better than using rods and clocks, also taking the opportunity to emphasize that such a stipulation is unavoidable (Reichenbach 1924, p. 82, fn. 27). It is important to note that deriving the behavior of light-rays and test-particles from first principles is not...
an adequate response to the challenge Reichenbach poses here; that would be to answer question #2 from the previous section, when Reichenbach is really asking question #1. What is needed is some justification, on the basis of physical theory, for the designation of what is observable. In the context of a theory of everything, the only possible resource is the theory itself; that is, a theory of everything should allow, on the basis of its own first principles, a justification of the identification of the degrees of freedom accessible to observation within it. In the next section, I will describe how such a justification can be given.

6.4. The role of the sense-organs

In Section 6.1.2, as well as in the previous section, I argued that, to give a complete account of how theoretical representations gain empirical content, it is not enough to show how measuring devices can be used to determine the properties postulated within the representation; one must also justify the choice of particular measuring devices on the basis of that theory or of some other physical theory taken provisionally as unproblematic background. In this section, I will argue that such a justification must include a detailed account of the physical sensory devices used by whatever is constructing and evaluating the theory. I will consider whether this implies that the structure of those devices plays a constitutive role in physical theory, concluding that it could but need not. I will then consider the possibility that a hidden stipulation remains, one that cannot be eliminated without a complete account of the mind. Finally, I

103 The mistake is easily made. For example, Ryckman does so when he claims that Weyl’s procedure is “epistemologically on quite a different footing than an appeal to the approximately rigid behavior of rods and periodic behavior of clocks” (Ryckman 2005, p. 101).
will discuss the importance of the new requirement, which I take to be almost entirely philosophical and not something that should guide or constrain the development of future physics.

6.4.1. To the sense-organs

Physics is distinguished from mathematics by representing objects whose effects can be seen, heard, etc.; we, as evaluators of physical theory, can interact with the entities it describes. Indeed, in order to evaluate the empirical claims made by a theory, we must interact with the entities it describes, and a complete description of those entities will have to include an account of those interactions. Put another way, we as theorizers must have some presence in the domain described by the physical theory, if it is to qualify as empirical science.

I should be clear that I intend to make no assumptions about the nature of theorizers; I leave for philosophers of mind the interesting questions of what exactly it is about humans that enables us to theorize, as well as that of whether electronic devices or organisms other than humans are capable of theorizing. I do assume that theorizing is at least possible and that humans can do it.\textsuperscript{104} The physical interface between the theorizer and the entities described by the theory, whatever form that interface takes, is what I intend by the terms “sensory device” or “sense-organ.” Since we humans are, to date, the best and clearest examples of entities that can construct and evaluate theories, I will concern myself with how it is that we use observation to test those theories—through information gathered by way of our sense-organs: the eyes, ears, etc. Even where

\textsuperscript{104} Behaviorists, of course, will not agree.
measuring devices are used, a physical interaction with the sense-organs of a theorizer must occur in some way before an empirical claim made by a theory can be evaluated. Any attempt to articulate a notion of “indirect observation” must acknowledge this fact. The connection of theory to world must pass through sense-organs in some way; they are the bridges between the theorizer and the elements described by theory.

The sense-organs are physical objects, and as such are capable of being represented within physical theory. Constructing such representations is the task of the various medical and biological sciences associated with the individual organs (e.g., ophthalmology), or of neurophysiology more generally. These sciences are today extremely advanced, so that most of the current activity in the science of perception deals not with modeling the sense-organs themselves, but with efforts to understand how the information they generate is subsequently processed by the central nervous system. Of course, much remains to be learned about the medical aspects of sensory systems (i.e., how to diagnose and treat disorders), but the proper functioning of the various organs is generally well-understood. That understanding has been won through stimulus-response testing, dissection, microscopic observation, and chemical analysis—direct examination of the organs themselves. The theoretical representations of the organs involve a mixture of chemistry, engineering, and simple drawing. These representations can be expressed in the language developed in Section 6.2; each sense-organ can be represented as a

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For example, Roberto Torretti distinguishes “personal” vs. “impersonal” observation, arguing that the two are equivalent from the perspective of physical theory. But he, too, recognizes that the involvement of the senses is inevitable, even in impersonal observation: “Human perception must indeed always intervene at some stage of the harvest of impersonal observation data for use in science” (Torretti 1990, p. 18).

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An excellent reference on the science of sensory systems, one that pays some heed to the interesting epistemological questions surrounding perception, is Smith (2000).
characteristic mix of chemical and spatial degrees of freedom (from which relatively simpler, sub-cellular structural degrees of freedom might be constructed). These correspond to the ways in which the organ might undergo change, including, most importantly, its ability to couple changes in its environment to changes in the nervous tissue demarcating its interface with the central nervous system.

It bears repeating: these representations of sense-organs are borne from direct examination of the organs themselves. They can be evaluated on their own merits, according to how well the variability in the representation—its degrees of freedom and their manner of dependence—matches or fails to match the variability evident in the examinations of the actual organs. That is, one can ask whether the theoretical representations of the sense-organs are subject-matter-complete or not. This is not in principle any different from asking whether a vector captures the degrees of freedom evident in our experiences of measuring rods. Again, I am not primarily concerned here with exactly how this evaluation is made. Rather, the point I wish to emphasize is that the behavior of the sense-organs can and should be derived on the basis of physical theory; to the extent that current neurophysiology is confirmed, and that modern chemistry either is physical theory or can be derived from modern physics, this has already been done.107

Once it is recognized that representations of sense-organs can be evaluated for subject-matter-completeness on their own, the last piece needed for a solution to the problem of empirical content presents itself readily. The degrees of freedom accessible

107 The question of whether chemistry is reducible to quantum mechanics is a live one in the philosophical literature. See fn. 74 (in Section 5.2.1) for more on this.
to observation in a theory can be identified by constructing a representation of a sense-
organ from the primary degrees of freedom of that theory. The construction is guided by
a posteriori experience of the sense-organs attached to the theorizers performing
observations, not any a priori constraint on the notion of observation. For example, the
fact that electromagnetic radiation of certain wavelengths is observable can be known
from the fact that we have sense-organs—eyes (retinas, to be specific)—with degrees of
freedom capable of coupling directly to those in the radiation; in other words, we have
organs that are photo-sensitive.

When we use instruments to mediate observation, to convert what cannot be
observed with sense-organs into something that can, what we are doing is using an
intermediate physical process to couple the degrees of freedom in an unobservable
system to those in our sense-organs. The display of an instrument (i.e., its pointer,
numerical indicator, graphical chart, etc.) and the sense-organ used to read it must both
allow enough variability to transmit the desired information (the set of values of the
relevant degrees of freedom); otherwise that information is lost in transit—i.e., it is not
part of the observation. The description is easily reversed; certain devices can be
considered extensions of sense-organs precisely because the degrees of freedom in their
outputs can couple to the degrees of freedom in our sense-organs. On the other hand, if
some physical entity cannot somehow be coupled to our sense-organs, we will be unable
to make empirical statements about it; we could not in that case formulate a physical
theory whose empirical content concerns it.

As indicated in the previous section, it is only in the context of a theory of
everything that the sense-organ representation has to be constructed from the primary
degrees of freedom of the theory we wish to endow with empirical content. In less ambitious contexts, we can take modern sensory neuroscience as unproblematic background and use it to identify the kinds of degrees of freedom that are accessible to observation (e.g., those of electromagnetic radiation), which could then be located as primary or secondary in the theory we wish to connect to experiment. The identification of what is observable would then be based on physical theory, just not the particular theory under consideration. It should be stressed that the assumption of some theory as unproblematic background is not an arbitrary stipulation of the kind Reichenbach thought was unavoidable. Rather, it is a provisional step taken with the understanding that a future, systematic theory will eventually reproduce and justify the assumption. In that sense, the move is on a par with the assumptions that measuring rods are rigid and that light-rays follow null geodesics. In a theory of everything, however, no background theory can be taken as unproblematic; all must be derived from first principles. To the extent that we expect our current physical theories to arise as effective theories in appropriate limits in any plausible theory of everything, these assumptions ultimately might not be so difficult to justify. On the other hand, one should not place too much stock in predictions about the form of future theory.

What the Weyl-Einstein reasoning makes clear is the need to show how measuring devices do their job as measurers. One half of this task involves the interaction between the device and the system being measured, which the Brown-Butterfield picture captures very ably. But there is another half that cannot be ignored: the interaction between the device and whatever is theorizing about the system to be measured. Both interactions are physical, and hence both should be described by
physical theory. And it is by way of this description that physical theory gains empirical content and becomes capable of test through observation.  

6.4.2. A possible constitutive role

It follows from the preceding analysis that the empirical content of the physical theories that we construct and evaluate must depend in some way on the kinds of sense-organs we have. In one way, this statement is so obvious as to be mundane. If we could not see light, for example, we would not report the results of our experiments using electromagnetic radiation. Recall, however, that the refined notion of empirical content outlined in Section 6.3.3 does not require that it be characterized in terms of the degrees of freedom accessible to observation. We are free to characterize the empirical content in terms of any degrees of freedom constructed from (i.e., determinable by) those accessible to observation. Thus, if some set of degrees of freedom (primary or secondary) is determinable from representations of two different sense-organs, then the empirical content of the theory will be insensitive to the choice between those two sense-organs. The enormous flexibility inherent in the degree of freedom concept practically ensures that many different kinds of sense-organs will be able to couple to any kind of physical entity, if not directly then mediated by some measuring device. If it is the case that any degree of freedom in the world can be coupled, however indirectly, to every other degree of freedom, then it does not really matter what kind of sense-organs we have, only that...

108 Note that the role of the theorizer in this picture is not in any way dynamical. The theorizer is not required to be represented as a conscious being, only as something capable of interacting with the objects being represented. Consciousness only matters here because it is the source of theoretical representations, not because the objects being represented lack a mind-independent existence. This is very different from the Copenhagen interpretation of quantum mechanics, where interaction with a conscious observer causes a special dynamical change in the state of a physical system.
we have some at all. Note, however, that this universal coupling requirement is highly non-trivial; one can easily imagine theories that do not satisfy it.

This is all to say that physiology is unlikely to play a deep constitutive role in a final physics, though that could really only be determined by the final theory itself through its connection to modern neuroscience. In any case, I intend a significantly more limited conclusion here, that the physiology of the sense-organs of the theorizer must be taken into account in any non-arbitrary determination of what is observable and what is not to that theorizer, rather than that the concepts used in physics (i.e., its semantic content) depend on human physiology, as Helmholtz famously argued. This follows directly from the credo enunciated in Section 6.1.2, that there is no such thing as observable simpliciter; all observability claims implicitly refer to a physically-constituted observer. The limitations that this implies are determined by the dependence relationship between the degrees of freedom in the representations of our sense-organs with other degrees of freedom in the theory. Any degrees of freedom that can be determined by those in the representations of our sense-organs will be accessible to observation by us—they will represent something observable.

On the other hand, it seems rather obvious that physics would have developed differently had our sense-organs been radically different. If we could detect electric fields directly, as sharks do, or if we had the ability to see neutrinos, then surely we would, at the very least, have come upon our current theories of electromagnetism and

\footnote{See, for example, DiSalle (1993).}

\footnote{The shark’s “electric sense” has only recently been properly identified; it is critical for predation underwater and is extraordinarily sensitive, capable of detecting $10^{-6}$ volt/cm (Fields 2007).}
the weak force sooner. Whether we would have come up with different theories altogether (in those or other fields), and whether a final theory would ultimately be different because of it, are open questions, answers to which, again, do not follow from the conclusions drawn here.

6.4.3. Still a stipulation?

There are, I believe, two ways in which one might worry that a kind of stipulation is being made surreptitiously in the account offered here. First, one could ask whether the identification of sense-organs as the origin of observation involves a stipulative or conventional moment. Second, one could ask whether the recognition that a particular set of degrees of freedom represents a sense-organ requires some prior notion of observability. I think both concerns can be allayed, but not, perhaps, without some cost.

In following the Weyl-Einstein reasoning, we have essentially been chasing stipulations or declarative definitions away. We do not need to declare that measuring rods read the metric tensor, for we can derive that they do from the laws of nature. We do not need to declare that measuring rods are observable, because we can derive that they are by representing the physical interactions with our sense-organs by means of the theory. Do we need to declare that sense-organs are the source of observability? One can imagine Reichenbach countering, “No epistemological advantage is gained by using eyes instead of measuring rods to give a theory empirical content.”

There are two ways to respond to this. On one hand, it could be argued that to be a sense-organ just is to be the generator of observable experience; the claim equating the two would in that case be analytic. On the other hand, one might not be satisfied without some account of what “observable experience” itself is (e.g., raw sense-data, qualia, outer
intuition, etc.) and exactly how it comes about, in which case an adequate response would require a full-fledged theory of the mind. I find the latter position tenable but overly strict; it requires that one solve everything before solving anything. I am satisfied here to chase the stipulative moment to the statement “Sense-organs are the generators of observable experience.” This claim is either analytic or a promissory note on some future theory of the mind. In either case, it is neither arbitrary nor conventional.

The second concern follows from the recognition that our representations of sense-organs will almost certainly employ degrees of freedom that are themselves not accessible to observation (e.g., chemical or electronic). When we develop those representations, or when we judge whether they are subject-matter-complete or not, we will very likely use physical theory that gains empirical content by assuming some other theory as unproblematic background. This has the appearance of circularity; it seems as if one must have the representation before one can find or develop the representation. But the circularity is specious. When a representation of a sense-organ is used to give empirical content to a theory, it does not need to get the empirical content from anywhere else. The representation is the generator of empirical content. It is only when the sense-organ is the object being observed that one need worry about how its representation gains empirical content. But the answer there is as obvious as it is in practice; a representation of another sense-organ, whether of the same kind or not, plays the needed role—a neuroscientist does not examine his or her own sense-organs. More generally, we make provisional assumptions in order to bootstrap our way to more sophisticated representations; the representation is ultimately judged by its empirical adequacy, not by the assumptions made in generating it.
One might still worry that a declaration of sorts is involved in the recognition that a particular set of degrees of freedom represents a sense-organ. For example, if there were two different ways of constructing the same subject-matter-complete representation of a particular sense-organ within a given theory, there would be nothing to compel the choice of one over the other. While this would certainly be true, it would not mean that the choice is conventional. If the two representations interact differently with other entities described by the theory, then the choice will have empirical consequences and will be one of the potential loci for modification if disconfirmation occurs. That is, the choice would be a disconfirmable hypothesis, not a convention. If there were no empirical differences, then there would be no choice to make.

6.4.4. Lessons—Philosophy vs. Physics

It is important to recognize both the limitations and the consequences of the conclusions drawn here concerning sense-organs. In particular, I do not think they can be used to derive constraints on future physics. Instead, they complete the story of how physical theory is connected to observation in a self-consistent manner and without arbitrariness. The main conclusion is that the sense-organs of the theorizer play a critical role in this story; if it is to be told completely, then a detailed account of those organs, as physical objects, must be included. Whether or not that implies that the empirical content of our physical theories has some dependence on our particular physical constitution is something that can only be determined by those theories in conjunction with neurophysiology. However, this picture does determine the proper necessary role of the theorizer in accounting for the empirical content of theoretical representations; the
theorizer is a necessary part of that story because only theorizers can construct and evaluate theoretical representations.

This story is, quintessentially, one for philosophers to worry about. It is the job of the physicist to concoct explanations of the world we experience. The mathematician and mathematical physicist are tasked with making those explanations technically rigorous and internally coherent. The philosopher of physics is the one to worry about what the explanations mean in a broader sense, about their “external coherence”; this is done by finding hidden assumptions relating to other webs of belief and examining their justification. What the analysis here shows is that physicists routinely employ implicit assumptions about sense-organs—they must if they are to connect their theories to observation. This should not be viewed as a problem; physicists are not concerned with explaining experiments down to every last detail and are therefore happy to make simplifying assumptions. It is only when we desire a truly complete story about the connection between theory and world that the sense-organs need enter; this completeness is a philosophical concern.

To make an assumption about the behavior of a sense-organ is to employ a representation or model of the object, typically a representation that is very subject-matter-incomplete. When the physicist designates some degrees of freedom within a theory as observable, he or she tacitly does just this. For example, in a theory including electromagnetism, where the radiative solutions are deemed observable, those solutions themselves function as representations of eyes or retinas. When we compare the representation to observations of actual eyes or retinas, we find that many degrees of freedom are missing, both in number and in kind. Yet it works quite well as a first
approximation to say that light is the generator of our sense-experiences; indeed, this is just the natural standpoint, naïve representationalism.

The comparison to the rigidity assumption for measuring rods is exact. An assumption is employed provisionally, and a more detailed story later justifies the assumption. Arguments from quantum theory can be used to justify the rigidity assumption, from electrodynamics to justify the null geodesic hypothesis, and within general relativity to justify the test-particle geodesic hypothesis. Here, arguments from sensory neurophysiology are used to justify the observability hypothesis. In each case, hypotheses or assumptions employed early on have to be justified later on the basis of physical theory; naïve representationalism is replaced by a more sophisticated, more detailed account. The observability assumption might also include a model of measuring devices that couple to the sensory organs (as, for example, in high-energy physics with particle detectors), but this does not affect the main point. *Every physical theory must include, tacitly or explicitly, a model of the sense-organs used by the theorizer conceiving its test, or else it would be no more than mathematics.* This is as true for theories that do not aspire to be theories of everything as for those that do. In a theory of everything, however, those representations must be made explicit and they must be subject-matter-complete.

Finally, as indicated earlier, this problem might be, for the most part, already solved. To the extent that we understand the behavior of the sense-organs on the basis of current theory, and to the extent that such theory is insensitive to the physics at the fundamental level, we already know what kinds of degrees of freedom our sense-organs can couple to. To give a particular theory empirical content, we need only locate those
kinds of degrees of freedom within it. From there we can consider the separate question of whether measuring devices can successfully couple those degrees of freedom to the primary ones in the theory.

6.5. Conclusion

I have attempted here to isolate one aspect of the relation between theory and world for close analysis, that which concerns the way that theoretical representations can be capable of test through observation. Though this inquiry was motivated by the debate between Weyl and Einstein concerning Weyl’s unified field theory, and especially their eventual agreement on the proper treatment of measuring devices in a fully-developed theory, I have tried to extend the analysis by filling in a gap left open in their account of how theories gain empirical content.

In order for theoretical representations of unobservable entities to have empirical content, two determinations are required: 1) degrees of freedom within the representation must be understood as accessible to observation, and 2) those degrees of freedom must be capable of determining, in a logical-mathematical sense, the degrees of freedom of the unobservable entities. This is the case whether the degrees of freedom accessible to observation are those of measuring devices or not. Weyl and Einstein required that the second determination be made on the basis of physical theory and not through stipulation, and the Brown-Butterfield picture shows how this can be carried out; I have done the same for the first, concluding that this cannot be accomplished without some kind of representation of the sense-organs of the theorizer constructing the representation. Together, these determinations justify the use of particular measuring devices in
measuring the theoretical quantities postulated by the theory; we use those measuring
device devices because we can and because they work.

In practice, for theories with explicitly restricted domains, the required sense-
organ representations can be implicit and simplistic. Indeed, I have argued here that at
least rudimentary models of sense-organs are involved in all physical theory. However,
in the context of a theory that purports to describe everything in existence without the aid
of any other theory, all assumptions about sense-organs must be made explicit and their
attendant representations must be subject-matter-complete. It is thus that physical theory
can be given empirical content in a complete and self-consistent manner, free of
arbitrariness and of reliance on background theory. The needed representations might in
fact already be largely completed, ready to serve as epistemological ballast for future
physicists reaching out toward a theory of everything.
APPENDIX A:

ABSTRACT DERIVATION OF THE LINEAR DIFFERENTIAL FORM IN WEYL’S GENERALIZATION OF RIEMANNIAN GEOMETRY

In this appendix, I will provide a derivation the new linear differential form in Weyl’s field theory that is more abstract and more transparent than either of the derivations given by Weyl and presented above in Section 2.3. Weyl’s reliance on component notation and his use of infinitesimal quantities obscure some key points in his derivation of the new geometry. The exact moment of generalization is difficult to see, and, perhaps more importantly, it is not clear that the generalization itself is not compelled by the requirement that the metric be re-scalable. My derivation here will clarify these points.

Recall that Riemannian geometry is characterized by the compatibility condition, (2.6), which says that there is some \( (0,2) \)-rank tensor field, \( g \), such that \( \nabla g = 0 \). While this condition is known as “compatibility,” since it allows the definition of an inner product that observes the chain rule under covariant differentiation, it could also be called a “constancy” condition. It asserts that \( g \) is invariant under parallel transport—that it is a constant of the manifold. This does not mean that its components are the same at all points, but rather that the tensor field itself, understood as a geometric object, is the same everywhere; indeed, its components must change in order to compensate for the twisting and turning of the basis fields. Now, because this condition fixes the affine connection
uniquely, there can be only one such constant metric, up to a scalar constant—any other
\( \left( \begin{array}{c} 0 \\ 2 \end{array} \right) \)-rank tensor field that is constant under parallel transport must be a scalar multiple of
\( g \). This provides a very elegant way to characterize Riemannian manifolds; they are
those that allow a constant \( \left( \begin{array}{c} 0 \\ 2 \end{array} \right) \)-rank tensor field.

This is not to say that other \( \left( \begin{array}{c} 0 \\ 2 \end{array} \right) \)-rank tensor fields cannot be defined on
Riemannian manifolds, only that they cannot be constant. In particular, we can define a
second tensor field to be a locally-varying multiple of \( g \). If we require that the
proportionality-factor be positive definite, so that it separates timelike and spacelike
vectors into the same classes as \( g \), then we can represent the new field by

\[ r = e^\rho g \]

where \( \rho = \rho(\varphi) \) is any smooth function on the manifold. Since \( r \) is an \( \left( \begin{array}{c} 0 \\ 2 \end{array} \right) \)-rank tensor
at each point, it defines an inner product on vectors and can function as a metric, just as
\( g \) does. It will, of course, give different answers to many calculations, and if the
observables in a physical theory depend on \( r \) instead of \( g \), then the geometry might
appear to be different in some respects.

To see how the rescaling sets the stage for Weyl’s geometry, consider how \( r \)
behaves under parallel transport:

\[
\nabla r = g \otimes (\nabla e^\rho) + e^\rho \nabla g = g \otimes (e^\rho d\rho) + 0 = r \otimes d\rho
\]
Allowing some flexibility in the notation, this can be rewritten in a way that shows how we can still think of $\mathbf{r}$ as a constant tensor field, if we also think of $\rho$ as forcing an adjustment to the covariant derivative:

$$(\nabla - \mathbf{d}\rho)\mathbf{r} = 0 \quad (A.1)$$

We can thus, if we wish, think of $\mathbf{r}$ as the “real” metric (i.e., the one with physical relevance), just one that is constant relative to a different connection than the Riemannian one. This is essentially what Weyl does at (2.10) above. However, we have not created a new geometry yet; the manifold still allows a constant $\left(\begin{smallmatrix} 0 \\ 2 \end{smallmatrix}\right)$-rank tensor field, one we just happen to be ignoring.

In order to generate a new geometry, (A.1) has to be generalized; the place to do so is clear. The linear differential form $\mathbf{d}\rho$ is not an arbitrary form, but rather one generated by applying the $\mathbf{d}$ operator to a scalar function. There are many linear differential forms that cannot be generated in this way. So we let

$$\mathbf{d}\rho \rightarrow \varphi = \varphi^\alpha \omega_\alpha \quad (A.2)$$

and (A.1) becomes

$$(\nabla - \varphi)\mathbf{r} = 0 \quad (A.3)$$

(A.3) takes the place of the compatibility condition, and it determines the affine connection uniquely. Unless there is some function $f$ such that $\varphi = \mathbf{d}f$, there can be no constant $\left(\begin{smallmatrix} 0 \\ 2 \end{smallmatrix}\right)$-rank tensor field, and there is not enough information in $\mathbf{r}$ or any other metric to fix the affine connection; this generalization truly does define a new geometry. However, it is generated by the generalization in (A.2) and not merely by a local rescaling of the metric.

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Finally, gauge transformations can be recreated in a straightforward way from (A.3). If we allow another local rescaling of the metric, say \( r^* = e^{\sigma} r \), then the form of (A.3) can be preserved with a simple adjustment to \( \varphi \):

\[
\begin{align*}
(\nabla - \varphi) r &= 0 \\
(\nabla - \varphi) e^{-\sigma} e^\sigma r &= 0 \\
(\nabla - \varphi - d\sigma) r^* &= 0 \\
(\nabla - \varphi^*) r^* &= 0
\end{align*}
\]  

where \( \varphi^* = \varphi + d\sigma \). This forces precisely the change on \( \varphi \) prescribed in (2.13) and (2.15) (just let \( \lambda = e^{\sigma} \)). From this perspective, it is easy to see why gauge transformations have no effect on the geometry; they are essentially ways of multiplying by 1, much like basis transformations.\(^{111}\)

\(^{111}\) Changes of basis, whether induced by coordinate transformations or not, can always be represented as multiplication by the identity matrix.


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