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Conventionalism: Poincaré, Duhem, Reichenbach

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0. Conventionalism

A recurrent theme in philosophy of science since the early 20th century is the idea that at least some basic tenets within scientific theories ought to be understood as conventions. Various versions of this idea have come to be grouped together under the label “conventionalism”. At least implicitly, conventionalism emphasizes the social character of the scientific enterprise. A convention, after all, is a solution to a problem of coordination between multiple actors (cf. Lewis 1969, ch. 1). Whenever achieving the best solution to a given problem depends not on every actor choosing one uniquely optimal course of action, but rather on everyone choosing the same option (or, as the case may be, corresponding ones), we say that the problem can be solved by agreeing on one approach “by convention”. The notion that some statements of scientific theories are conventional thus goes against a (self-) understanding of science as an essentially individualistic endeavor, according to which any scientific investigation could “in principle” (i.e., if only it weren’t too much work) be performed by any intelligent individual using the right methods, and reason and experience would uniquely determine the investigation’s conclusions. Deviating from this understanding, philosophers of science of the first half of the 20th century have devised some influential arguments in support of an essential role for convention in science. The story of conventionalism has its roots in the philosophical reflection on the nature of space and the status of geometry.

1. Poincaré

The curious nature of geometry has taxed many philosophers. On the one hand, it is a mathematical discipline within which theorems are established by proof and without reference to experiment or observation. On the other hand, it seems to make direct claims about the physical world that we inhabit and experience. In the late 18th century, the philosopher Immanuel Kant offered a solution to the problem of integrating these two aspects into one coherent conception of geometry (Kant 1781/87: B37-B45). For Kant, space is a "pure form of intuition" and belongs to the principles that the human mind provides for structuring and ordering sense experience. All our experience of external objects is structured by the spatial form of intuition, which is why our geometrical knowledge holds good for all the world of experience. However, in order to attain geometrical knowledge, we need not have any actual experience of external objects. The proofs of geometrical theorems can be constructed in pure intuition itself, thereby rendering geometrical knowledge "a priori", i.e. independent of sense experience. That they *have* to be thus constructed shows, according to Kant, that the truths of geometry are not just consequences of the meanings of the terms "point", "straight line" and so forth—in Kantian terms, geometrical knowledge is not analytic, but synthetic.

Kant's conception of geometry as a body of synthetic a priori truths soon became immensely influential. But the 19th century saw developments within the science of geometry itself that would ultimately shake its foundations. These developments began when non-Euclidean geometries were discovered. Geometers had long puzzled over the status of Euclid's fifth postulate, the so-called parallel postulate. It is equivalent to the claim that given a straight line l and a point P not on l, there exists exactly one straight line through P that is parallel to l. Euclid needed the postulate to prove such basic geometrical propositions as the theorem that the angle sum of a triangle is always equal to 180°. To many geometers the postulate itself seemed less simple and self-evident than the other axioms. However, all attempts to prove it on the basis of the remaining axioms failed. In the 1820s, two mathematicians working independently, the Russian Nikolai Lobachevsky and the Hungarian János Bolyai, managed to demonstrate that the parallel postulate cannot be proven from the other Euclidean axioms. In effect they showed that the other axioms are logically consistent with the assumption that more than one parallel to l can be drawn through P. Later, Bernhard Riemann showed that they are also consistent with the assumption that the number of parallels to l through P is zero. Riemann was one of the first and most successful among those mathematicians who took up the task of exploring and systematizing the "non-Euclidean geometries", in other words, the various consistent systems that can be generated by dropping the parallel postulate and replacing it with alternative assumptions.

At the same time, a second task that slowly attracted attention was that of determining the epistemological status of the various non-Euclidean geometries, especially in relation to Euclidean geometry. The Kantian position that the latter necessarily constituted the uniquely true conception of space came to be doubted. To illustrate the challenge, consider the following thought experiment, which was used in a similar fashion by the German physicist Hermann von Helmholtz in an influential paper (Helmholtz 1876). Imagine a two-dimensional world — let's call it "Flatland" — inhabited by two-dimensional creatures.[1] But do not imagine their universe as a flat plane, but instead as the surface of a large sphere. The creatures cannot perceive the spherical form of their world, as they are themselves two-dimensional and ought to be pictured as living "within" the surface rather than on it. The shortest path between two points on a spherical surface is an arc of the great circle through these two points. (A great circle is any circle on a sphere's surface that exactly cuts it in half.) So what is a straight line for the Flatlanders is, from our perspective, a great circle. In the geometry of Flatland, there is never a parallel to a given straight line l that goes through a point lying outside l, because two numerically distinct great circles on the surface of a sphere always intersect. Another curious fact about Flatland geometry is that the angles of triangles do not add up to 180°. To see this, consider one of the triangular pieces of orange peel that you get by cutting an orange into eight pieces of exactly the same size by perpendicular cuts. All the cuts meet at right angles, so the

[1] The name is borrowed, not from Helmholtz, but from Edwin Abbott's (1884) literary fiction about a two-dimensional world.

angle sum of such a triangle is 270°. If you cut out smaller triangles of orange peel (always along great circles on the orange's surface), you will get angle sums of less then 270°, but always more than 180°. The geometry of Flatland is thus a close illustration of the two-dimensional case of that variety of non-Euclidean geometry discovered by Riemann which is now called "elliptical" geometry.[2] Since the Flatlanders have never known anything other than Flatland, the geometry of their schoolbooks may very well be of the elliptical variety. To them, a geometry that claims an identical angle sum for all triangles independent of their size might seem as absurd and immediately refuted by geometrical intuition as the idea of a straight line that has no parallels seems to many humans. On the other hand, a perhaps even more thought-provoking idea is the hypothesis that the Flatlanders might in fact use Euclidean geometry and get along fine with it—provided that they inhabit (and confine all their measurements to) only a very small region of the Flatland universe. A small enough region of a spherical surface could approximate a Euclidean plane so closely that the deviations would be too small to be measured. This reveals that the situation of the Flatlanders could be our own. While it is impossible for us to visualize how our three-dimensional space could be "curved" (just as the two-dimensional Flatlanders would perhaps have no way of picturing *their* universe as a curved surface), the mathematical possibility that a three-dimensional space can have such structure is exactly what is proven by the existence of three-dimensional non-Euclidean geometries. Helmholtz (and others) concluded that the geometry of physical space is an empirical matter, to be decided by measurement. (Helmholtz also thought that astronomical measurements gave some evidence in favor of a Euclidean geometry of physical space.)

It was in this general context that conventionalism was first proposed as a philosophical viewpoint on the status of geometry, as an alternative to both Kantian apriorism and Helmholtzian empiricism. The man who made this proposal was none other than Henri Poincaré (1854-1912), a celebrated French mathematician with a staggering list of

[2] A spherical surface is an *exact* illustration of elliptical geometry only if we identify every point on the surface with its antipode. That is because an axiom of all (Euclidean and non-Euclidean) geometries demands that there be exactly one straight line connecting two distinct points. If a point and its antipode are considered distinct, this axiom is violated by spherical geometry, because there are an infinite number of great circles connecting the two.

achievements in a wide range of mathematical specialties, including complex analysis, algebra, number theory and topology. In addition to this, he made important contributions to mathematical physics and to celestial mechanics.[3] Starting with a series of papers in the 1890s (many of which were later published in Poincaré 1902), he also addressed philosophical questions about mathematical and scientific knowledge, and in particular about geometry.

With Helmholtz (and also alluding to the Flatland thought experiment), Poincaré rejects the Kantian claim that our choice of geometry is *a priori* restricted (Poincaré 1902: 49). But he also denies that experience decides the matter. Selecting a geometry involves a real choice, where experience merely plays the role of giving "the indications following which the choice is made" (Poncaré 1905: 72). Another thought experiment, presented by Poincaré himself in support of this conclusion (1902: 65-68), will help us to explain his reasoning.

This time, we are to imagine a three-dimensional world, filling up the inside of a sphere—let's call it "Sphereland". This world has an absolutely stable temperature distribution, where temperature varies only with distance r from the center of the sphere. The law according to which it varies is $R^2 – r^2$ (where R is the radius of the sphere itself), so that temperature is highest in the center and approaches zero toward the edge of the world. All bodies expand and contract thermally, such that their linear dimensions vary proportionally with temperature. In addition, the refractive index of the optical medium that fills this world also varies with $R^2 – r^2$. As a result, light travels not on straight lines, but on circular ones (that cut the sphere's edge at a right angle). So far, we have described Sphereland using Euclidean geometry. But how are the Spherelanders themselves likely to describe it? To begin with, they will not regard their world as confined within a sphere, but as infinite, because as they approach the sphere's surface, they (and their steps) get smaller and smaller, rendering them unable ever to reach an end of their world. They obtain the same result if they take a solid body, use it as a ruler and try to measure the diameter of the universe. (Figure 1 shows how the ruler contracts—from our

3 On Poincaré's mathematical and scientific accomplishments, see Dieudonné 1975.

point of view. The ruler is bent because of the stronger contraction effects on those parts of its material that are further away from the center of the world.)

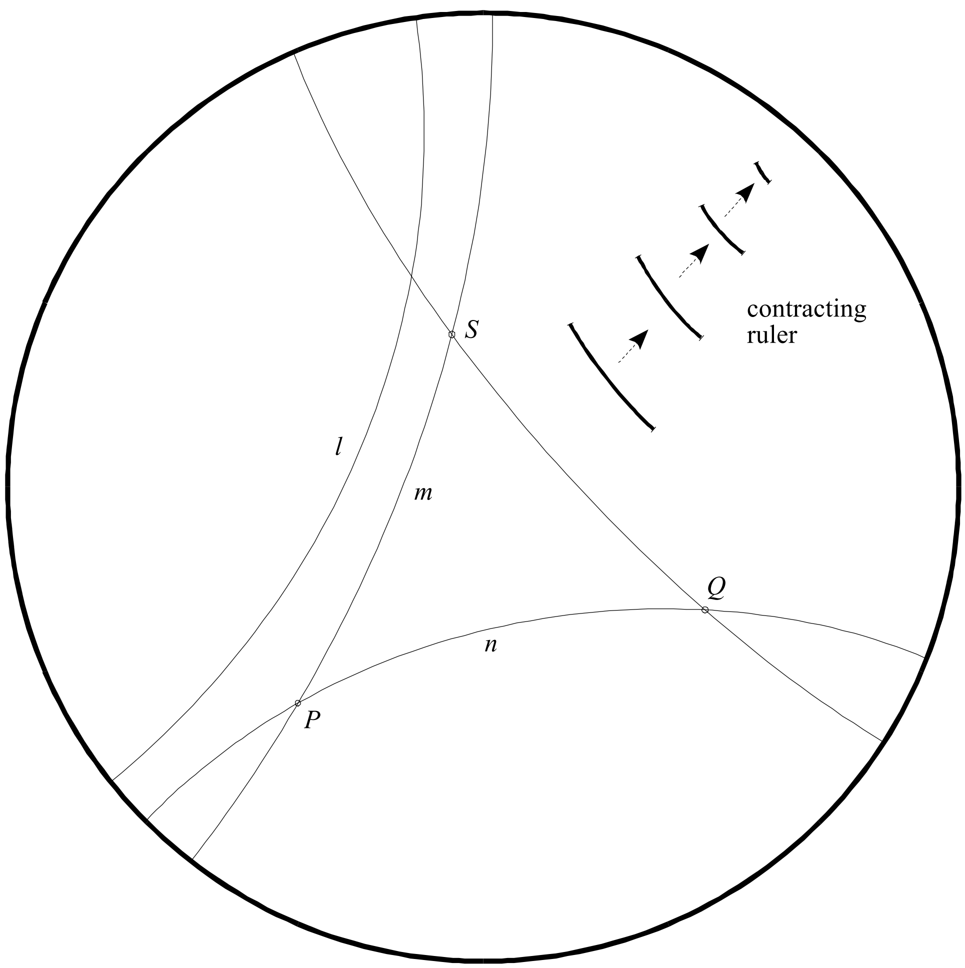

Figure 1: Sphereland (a planar cut through its center)

If the Spherelanders take light to travel along straight lines, they will find that given a line *l* and a point *P* not on it, more than one line exists in the same plane that passes through *P* and vanishes into "infinity" without ever intersecting *l* (as illustrated by lines *m* and *n* in figure 1). And if they measure the angles of a large triangle with an optical surveying instrument, they will find the angle sum to be less than 180° (compare the triangle *PQS* in figure 1). As a result, the geometry that the Spherelanders must adopt if they regard light rays as straight lines and solid bodies as reliable rulers is of a non-Euclidean kind — to be more precise, it is the "hyperbolic" type of non-Euclidean geometry that was discovered by Lobachevsky and Bolyai. Of course, they will then *not* agree that bodies contract as they are transported away from the center of the universe (measured with their rulers, things retain their size), nor that light rays in Sphereland are bent by a medium of varying optical density (light rays follow what Spherelanders take to be straight lines).

But what if a revolutionary Sphereland physicist were to challenge this hyperbolic world view? He might instead propose the Euclidean description that we used at the outset to introduce Sphereland—bodies contract, therefore all measurements made by the Spherelanders with rulers need to be corrected by a correction factor, likewise for angular measurements with optical instruments, because light rays are bent, and so forth. This Euclidean view of Sphereland posits both a different geometry and different physical laws about the behavior of bodies and of light, but the overall description of Sphereland that it gives is just as accurate as the hyperbolic one. No measurement or observation could decide between the two.

Poincaré concludes that this is generally the case. If, for example, we ourselves were to measure the angles of a large triangle in the actual universe with an optical instrument and come up with a result other than 180°, we would always have a choice: We could either relinquish the assumption that space is Euclidean, or we could give up the assumption that light travels along straight lines. There is nothing in the science of geometry that dictates which objects in the physical world are to be identified with straight lines and rigid bodies. Geometry in and of itself can therefore never be tested by physical measurement — it is only the conjunction of geometry with a set of assumptions about the behavior of certain physical things (solid bodies, rays of light) — in short, the conjunction of geometry and physics — that can conform or fail to conform to empirical evidence. By changing the physics, we can uphold whichever geometry we want: "[N]o experiment will ever be in contradiction with Euclid's postulate; but, on the other hand, no experiment will ever be in contradiction with Lobatschewsky's postulate." (Poincaré 1902: 75)

Later philosophers of science would use the word "underdetermination" to describe situations like the one analyzed by Poincaré: The choice of geometry is underdetermined by experience, because two descriptions of the world can be given that differ with regard to geometry but have exactly the same observable consequences. However, while Poincaré insisted on a real choice in connection with geometry, he did not think that the choice was an arbitrary one. Geometries differ in convenience. Which one is most convenient depends on experience, which therefore "guides us" in our choice of geometry (Poincaré 1902: 70-1).[4] Given our actual experience with the properties of solid bodies, and given the superior simplicity of Euclidean geometry, Poincaré is convinced that "Euclidean geometry is, and will remain, the most convenient." (*Ibid.,* 50) Nonetheless, its axioms are "neither synthetic a priori intuitions nor experimental facts. They are conventions." (*Ibid*.)

Poincaré also took some careful and very restricted steps to extend his conventionalist thesis beyond geometry. Certain laws in the experimental sciences (in particular in mechanics) take on such a central and entrenched place within the system of thought that they come to be regarded as principles or definitions. One of his examples is Newton's law of acceleration, $F = ma$. In the established systematic treatment of classical mechanics it is best understood as a definition of force, says Poincaré. "It is *by definition* that force is equal to the product of the mass and the acceleration; this is a principle which is henceforth beyond the reach of any future experiment." (*Ibid.,* 104)

With regard to Newton's law of gravitation, Poincaré gives the following explanation of how a law, once elevated "into a principle by adopting conventions" (1905: 124), can be excluded from experimental testing:

> Suppose the astronomers discover that the stars do not exactly obey Newton's law. […] We can break up this proposition: (1) The stars obey Newton's law, into two others; (2) gravitation obeys Newton's law; (3) gravitation is the only force acting on the stars. In

[4] A further restriction that bears mentioning derives from the fact that Poincaré was a steadfast adherent of Sophus Lie's group theoretical classification of geometries, as exposed, e.g., in Poincaré 1898 (see also Poncaré 1902: 46-7). Lie and others had characterized geometries by the specific group of displacements of a rigid body allowed in each of them. Poincaré thought that Lie's groups describe an idea of geometry that "pre-existed" in our minds (1902: 87). This in effect limits the choice to geometries of constant curvature (see below fn 8).

> this case proposition (2) is no longer anything but a definition and is beyond the test of experiment; but then it will be on proposition (3) that this check can be exercised. (*Ibid*.)

Even in the face of deviating observations, the law of gravitation can thus be saved from experimental disconfirmation by dropping the assumption that the behavior of heavenly bodies is governed by gravitation alone.

The thought that some elements of scientific theory become so well-established that they rise to the rank of conventions was further generalized by later philosophers of science such as Rudolf Carnap (see chapter 4). The idea was later understood in such a way that the conventional elements of scientific theory, rather than expressing facts, span the conceptual framework within which facts can be expressed. For Poincaré, the border between conventional and empirical elements in science was not entirely strict. He explicitly emphasizes that even conventional principles can be destabilized by recalcitrant empirical evidence. In the face of continuing problems to square a principle with experience, it becomes empty and unfruitful and is thereby undermined (Poincaré 1905: 109-10).

Poincaré also emphasizes that conventionalism does not mean that scientists can devise laws as they please. "No, scientific laws are not artificial creations" (1905: 14) — even the conventional principles were first discovered as empirical regularities. What is conventional is merely the choice of which to elevate to the rank of principles. "Conventions, yes; arbitrary, no—they would be so if we lost sight of the experiments which led the founders of the science to adopt them, and which, imperfect as they were, were sufficient to justify their adoption." (Poincaré 1902: 110)

Poincaré's significance for the philosophy of science is not limited to the origins of the conventionalist tradition. He also made important contributions to the philosophy of arithmetic (see Folina 1992). Interestingly, there he defended a decidedly pro-Kantian view, arguing that a core principle of arithmetical proofs, the principle of complete induction, was grounded in intuition. He therefore thought that arithmetical truths were synthetic rather than analytic, as Bertrand Russell and other logicist philosophers of mathematics claimed. Poincaré thus became one of the early critics of logicism and made an influential proposal on how to steer clear of logical paradoxes (by avoiding "non-predicative" definitions, see Poincaré 1908: 177-196). However, our focus in this chapter will remain on ideas associated with conventionalism. While Poincaré is one of its prime originators, the ideas of others have shaped the philosophical reception of conventionalism in important ways.

2. Duhem

One of these others was the French theoretical physicist, philosopher and historian of science Pierre Duhem (1861-1916). As a physicist, Duhem made important contributions to thermodynamics, electromagnetic theory and hydrodynamics (cf. Miller 1971 and Jaki 1984: 259-317). His wide recognition, however, rests more on his lasting influence as a historian and philosopher of science.

Poincaré in turn was an important influence for Duhem. (He also sat on the panel that accepted Duhem's doctoral dissertation in physics at the Ecole Normale Supérieure in Paris in 1888.) In his main philosophical work, *The Aim and Structure of Physical Theory* (1906), Duhem develops views on the development of scientific theories and their relation to observation and experiment that have greatly generalized and advanced the case for the methodological claim that scientists can choose to uphold certain elements of theory even in the face of recalcitrant empirical evidence. A central and well-known part of Duhem's argument for this is his critique of "crucial experiments" — experiments devised to bring about definite decisions between competing hypotheses. If different hypotheses give rise to different predictions about the behavior of a certain experimental set-up, one need only perform that "crucial experiment" in order to determine which of the hypotheses is in error, or so traditional methodology maintained. Duhem begged to differ and claimed that a "'crucial experiment' is impossible in physics" (1906, 188). This is so because in physics, a prediction about the outcome of an experiment is never derived from one theoretical hypothesis alone. In addition to the hypothesis tested, one relies on a whole set of theoretical assumptions about the objects under study in the experiment and about the functioning of the experimental apparatus in order to predict a particular result. If the experiment is then performed and the result is *not* obtained, there is no way of knowing for sure that the fault is with the contested hypothesis rather than with one of the other assumptions. "The only thing the experiment teaches us is that among the propositions used to predict the phenomenon and to establish whether it would be produced, there is at least one error; but where this error lies is just what it does not tell us." (Duhem 1906, 185)

To illustrate his point, Duhem discusses the example of Léon Foucault's experimental comparison in 1850 between the velocities of light in air and in water, which was widely regarded as a crucial experiment in favor of the wave theory of light and against Newton's corpuscular theory of light. While the wave theory explains the refraction of light rays at a water surface by the slower propagation of light waves in water, the corpuscular theory's explanation of the same fact involves the claim that the tiny light projectiles are accelerated by the attraction of the water particles. When Foucault determined the speed of light to be lower in water than in air, he and many of his scientific contemporaries took this to be the final blow for the hypothesis that light consists of tiny particles. Duhem objects that the mistake might just as well be hidden somewhere in the details of the Newtonian explanation of refraction, such that the corpuscular theory is erroneously attached to the idea of a higher velocity of light in water:

> [T]he experiment does not tell us where the error lies. Is it in the fundamental hypothesis that light consists in projectiles thrown out with great speed by luminous bodies? Is it in some other assumption concerning the actions experienced by light corpuscules due to the media through which they move? We know nothing about that. (Duhem 1906, 187)

Duhem's general conclusion is that a hypothesis in physics is never tested against experimental evidence in isolation. Instead, it is always a larger set of physical assumptions within which the hypothesis is embedded that is subjected to empirical testing as a whole. This conclusion is nowadays sometimes called the "Duhem thesis".[5] Note the similarities of Duhem's line of

[5] It was later radicalized by Willard Van Orman Quine, who expanded the body of knowledge that is put to test in every empirical check (not just in physics) to include even our mathematical and logical beliefs. (See chapter 5.) This amplified version is also called the "Duhem-Quine thesis".

reasoning with Poincaré's above-quoted explanation of how scientists may, if they so choose, uphold Newton's law of gravitation even given astronomical discoveries that appear to contradict it. Duhem himself remarks that his observations explain how specific theoretical elements "which the physicists of a certain epoch agree in accepting without test and which they regard as beyond dispute" can be saved from refutation by always choosing to modify one of the other assumptions whenever the data fail to match the predicted result (Duhem 1906, 211).[6] Duhem's thesis also entails that in physics, no experiment can ever be regarded as a definite falsification of any particular isolated theoretical conjecture. (It thereby poses a problem for Karl Popper's falsificationist philosophy of science, see chapter 6 below, and is often invoked in its criticism.)

In Duhem's own view, his arguments are about theory and experiment in *physics* specifically. To him, the special feature of physics that gives rise to all his methodological observations is the high degree to which the elements of physical theory are interconnected. This is mainly so because checking the observational consequences of a physical theory always requires the theoretical *interpretation* of an observation or a phenomenon, and this interpretation always involves other parts of physical theory (Duhem 1906: 144-6). According to Duhem, the same cannot be said about all sciences. Later generalizations of the Duhem thesis thus carry it beyond its originally intended scope (Ariew 1984).

It should be emphasized that Duhem did not single out any particular elements of physical theory and identify them as conventions. He did however highlight the freedom that physicists enjoy when they re-shape theory to fit experimental results. In deciding which elements of theory to retain and which to modify in reaction to new evidence, they have to exercise their "good sense" (Duhem 1906: 216-8). This good sense thus plays an indispensible

[6] Another sign of Poincaré's influence is Duhem's other example for an alleged crucial experiment, Otto Wiener's 1891 experiment on the plane of oscillation in polarized light (Duhem 1906: 184-6). Poincaré had criticized its purported role as a crucial experiment in a paper of 1891, and Duhem had taken up Poincaré's critique and generalized it as early as 1892 (Duhem 1892, cf. Martin 1991: 104-6 and Ben Menahem 2006: 72-3).

and fundamental role for "the entire edifice of scientific truth", according to Duhem (1903: 95).

The theory that the physicists thus establish is "a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely, and as exactly as possible a set of experimental laws." (Duhem 1906, 19) For Duhem, physics occupies itself with regularities that are accessible by experiment. It is *not* concerned with revealing natures and entities that are ultimately responsible for these regularities (*ibid*., 20-1, 7-18). Instead, Duhem embraces an idea of the Austrian physicist and philosopher Ernst Mach, that physical theory is first and foremost an "economy of thought" — its goal is to organize and structure empirical facts in order to facilitate their parsimonious representation (Mach 1882, Duhem 1906, 21-3). To claim for physics an aim more ambitious than mere representation and classification, such as giving explanations based on "the nature of the elements which constitute material reality", would mean to want something that "transcends the methods used by physics". Physics uses "the experimental method, which is acquainted only with sensible appearances and can discover nothing beyond them" (*ibid.*, 10).[7] Note that this view of physical theory and Duhem's methodological observations with regard to the "Duhem thesis" are not unrelated. If the confrontation of theory with problematic evidence always admits more than one rational response, and the development of theory is thus "underdetermined" by the empirical data (a term not introduced by Duhem, but by later methodologists), it seems fitting to regard the dependable content of physical theories as limited to the empirical generalizations they contain.

However, it would be a mistake to categorize Duhem's philosophy as an unambiguously anti-realist one. While he denied that the methods of physics were adequate for identifying the

[7] Apart from the view that physics aims at revealing the ultimate constituents of nature and giving explanations in terms of them, there is a second view of physical theories that Duhem was eager to criticize. It is the view that physics aims at constructing mechanical models that render physical phenomena understandable to the human mind, which Duhem considered to be manifest in the work of many of his English contemporaries (Duhem 1906, 69-75). While the English physicists seemed unperturbed by the fact that their many mechanical models (for example for the electromagnetic ether) often contradicted each other, Duhem regarded this as frustrating the objective of "the unity of science", which every physicist "naturally aspires to" (*ibid.,* 103).

real constituents of nature, he affirmed their suitability for arriving at ever truer classifications of the regularities discovered by experiment. He accepted that there *are* “hidden realities” underneath the observable data (albeit unknowable through the methods of science), and that over time, the classifications of experimental laws contained in physical theory will converge to a “natural classification” which reflects the “real affinities among the things themselves” (Duhem 1906: 24-7, 297-8). In the light of this aspect of his philosophy, the same has been described as the attempt to find a middle way between conventionalism and scientific realism (McMullin 1990).

Nonetheless, Duhem’s most influential contributions to philosophy of science remain his arguments regarding the impossibility of crucial experiments and conclusive tests of individual hypotheses. They have been taken by many philosophers of science to strengthen the case for the indispensability of non-empirical considerations in scientists’ efforts to advance scientific theories.

Over and above his philosophical impact, Duhem was an outstanding historian of science. He unearthed whole schools of medieval science from the obscurity of forgotten manuscripts and thereby demonstrated the existence of a continuous tradition that led to the discoveries of Leonardo, Galileo, and others. This work was instrumental in debunking the myth of the “dark middle ages”. Throughout his life, Duhem was a devout Catholic. This fact has been used to elucidate both his philosophical efforts on the one hand, as an attempt to separate physics from metaphysics, thus to secure a legitimate domain each for science and religion (cf. Deltete 2008), and his historical work on the other, as the endeavor to uncover the constructive contributions of Christian faith to the development of modern science (cf. Martin 1991).

3. Reichenbach

When thinkers like Helmholtz and Poincaré were reflecting on the possibility of a non-Euclidean geometry for physical space, their considerations still had the character of mere thought experiments and did not seriously challenge the use of Euclidean geometry in physics. This situation changed dramatically in 1915, with the advent of Einstein's general theory of relativity (GTR). According to GTR, the geometry of four-dimensional space-time is non-Euclidean. Physical space is described as possessing variable curvature,[8] the curvature depending on the distribution of masses and fields. The theory transforms the action of gravity into a feature of space-time geometry. Freely falling bodies are simply describing geodesics (shortest paths) in a four-dimensional space-time that is warped by the masses distributed within it, such that GTR has no more need for a gravitational force to account for those phenomena that were explained by it in classical physics. Gravitation has been "geometrized". For the conventionalist tradition, this development is both a triumph and a backlash. On the one hand, the conventionalist opposition to geometric apriorism is forcefully vindicated. What is more, important steps in Einstein's development of the relativity theories can be described as the discovery of conventional elements in physical theory, as we shall see. But on the other hand, the fact that geometry is now so intricately interwoven with physics seems to suggest that GTR reveals how the geometrical structure of space-time is really an empirical matter. Einstein, for one, supported geometric empiricism rather than conventionalism (Einstein 1921).

One of the first and most influential philosophers to reflect on questions concerning the conventionalist tradition in the context of relativity theory was Hans Reichenbach (1891-1953). Reichenbach had studied physics, mathematics and philosophy and in 1918/19 attended Einstein's first lecture course on GTR in Berlin. Originally strongly influenced by the neo-Kantianism then prevalent in German philosophy, he felt forced to give up apriorism in the face of GTR.

[8] Because two-dimensional non-Euclidean geometries can be modeled and illustrated with the aid of curved surfaces (see the "Flatland" thought experiment above), "curvature" has come to be used as a technical term for a central mathematical characteristic of geometries. The curvature of elliptical geometries (where the angle sum of a triangle is larger than 180°) is positive, the curvature of hyperbolic geometries (with angle sums smaller than 180°) negative. Euclidean geometry has zero curvature. The possibility of geometries with a curvature that varies across space was already described by Riemann in 1854.

In his early book *The Theory of Relativity and A Priori Knowledge* (1920), Reichenbach still thought that Kant's idea of synthetic a priori principles shaping all our cognitive activities could be partially salvaged. He distinguished between two meanings of the Kantian a priori: "First, it means 'necessarily true' or 'true for all times,' and secondly, 'constituting the concept of object.'" (Reichenbach 1920: 48) Under the impression of the revolutionary changes in geometry that were brought about by GTR, he considered the a priori in the first sense irretrievably lost, but in the second sense worth retaining. The constitutive principles that Reichenbach described, however, were not constitutive for human experience as such, as Kant's had been, but constitutive for the kind of scientific knowledge attainable within the framework of a particular theory. In particular, he identified these constitutive elements in the "coordinative principles" that establish the connection between the mathematical structures used by a theory (such as a certain geometry) with their empirical content – for example, by establishing how the length of an object is to be determined through a certain interpretation of measuring procedures (*ibid.*, 40). These ideas have since been interpreted as an attempt to recover the conception of a "relativized a priori" (Friedman 2001: 30-1, 72). Reichenbach himself soon became convinced that the direction of his thoughts had little to do with Kantianism any more.[9] His highly influential book of 1928, *The Philosophy of Space and Time*, is stripped of the Kantian allusions but still emphasizes that all scientific knowledge has to be informed by both empirical evidence and constitutive principles, now called "coordinative definitions".

One example that Reichenbach employs to explain the need for coordinative definitions is exactly the one that Poincaré uses in his arguments for geometric conventionalism. To relate the metric of the mathematical structures used in our physical theory of space to empirical reality, we have to define what kinds of bodies will count as measuring rods under which circumstances (in particular, under which circumstances we will assume them to retain their length). This definition is *based* on empirical observations giving us confidence that we can

[9] It seems to have been Moritz Schlick who convinced Reichenbach of this by correspondence, see Gerner 1997: 53-55.

consistently use certain bodies in this way. For example, we observe that two rods can be brought to match at different places, independently of the paths that are taken to move them around. However, "[w]hen we add to this empirical fact the definition that the rods shall be called equal in length when they are at *different places*, we do not make an inference from the observed fact; the addition constitutes an independent convention." (Reichenbach 1928, 16-7).

According to Reichenbach, Einstein's breakthrough consisted in part in "the discovery of the definitional character of the metric in all its details", i.e. in the realization that coordinative definitions are required also for the comparison of lengths in systems of different states of motion and for the comparison of time intervals occurring at different locations in space (*ibid.*, 177, cf. 15).[10] He calls this the "epistemological foundation" of the theory of relativity. The "physical" part of the theory that completes this is "the hypothesis that natural measuring instruments follow coordinative definitions different from those assumed in the classical theory" (*ibid.*, 177).

Does this mean that Einstein's choice of a non-Euclidean geometry for GTR is based on completely arbitrary conventions? Reichenbach does not think so. He develops a principled account for the choice of a geometry that lets Einstein's choice emerge as a special case. The account is based on the distinction between universal and differential forces (*ibid.,* 13-4, 22-8). Universal forces affect all bodies in the same way and they cannot be shielded against. All other forces are called differential. The effect of temperature on solid bodies in our actual world can be regarded as a differential force in this sense, because it acts differently on a copper rod and an iron rod. In contrast, the effects of temperature in the "Sphereland" thought experiment introduced in section 1 provide a good example for a universal force. Sphereland also illustrates the fact that in the presence of a truly universal force, physical theory can be reformulated in such a way that the universal force and its effects are eliminated by choosing a suitable geometry. (We had assumed that the Spherelanders' own science would take that

[10] The question whether the metric, and in particular the definition of simultaneity in the special theory of relativity, should be regarded as conventional or not has led to a controversy that lasts to this day; see Janis 2008 for overview.

course by adopting hyperbolic geometry.) Reichenbach stipulates that this is exactly the course that *should* be taken when a geometry is chosen. He demands that the crucial coordinative definition that lays down what counts as a rigid rod in empirical reality may include correction factors for differential forces, but not for universal ones (*ibid.*, 22). The geometry that must be selected as a result is the one that in effect sets all universal forces to zero. The Spherelanders are thus committed to the hyperbolic option, and we to the variable curvature geometry of GTR. As a result of Einstein's equivalence principle, gravity in GTR must affect all substances in the same way. Its effect on the length of measuring rods should therefore be set to zero. By thus adopting the suitable geometry, we spare ourselves the talk of measuring rods contracting in gravitational fields. "This universal effect of gravitation on all kinds of measuring instruments defines therefore a single geometry." (*Ibid.,* 256)

Reichenbach, while emphasizing the inevitable contribution of conventional principles to every physical theory, attached great importance to the fact that the conventional choices should not be regarded as arbitrary. He therefore rejected the term "conventionalism" for his view (Reichenbach 1922: 38) and discreetly distanced himself from Poincaré, who he thought had overemphasized arbitrariness (*ibid.* and Reichenbach 1928: 36-7).

Reichenbach's importance for modern philosophy of science far exceeds the contributions in the focus of this chapter. Though he was never himself a member of the Vienna Circle, he was one of the key thinkers of Logical Empiricism (see chapters 5 and 6). A distinctive feature of several of his philosophical works is an emphasis on the concept of probability. For example, he advocated a probabilistic version of the early Logical Positivists' verifiability criterion of meaning. According to Reichenbach's version, a proposition has meaning if it is possible to determine a degree of probability for it, and two sentences have the same meaning if they obtain the same degree of probability by every possible observation (Reichenbach 1938: 54). This theory was intended to avoid the problem that demanding verifiability of meaningful propositions would render the general statements of scientific theories meaningless, because they can never be verified in a strict sense. Reichenbach made pioneering contributions to several specialties that continue to engage philosophers of science, including the philosophical foundations of probability, causation, the direction of time and the philosophy of quantum mechanics. He emigrated from Germany after the rise to power of the Nazi party in 1933. After five years at Istanbul University, he became full professor at the University of California at Los Angeles, where he remained for the rest of his life, and thus also played a role in establishing philosophy of science in North America.

4. The conventionalist legacy

As regards geometric conventionalism, the endeavor to establish an interpretation of GTR that stresses the role of conventions was taken up by Adolf Grünbaum (1973) and others (see Juhl / Loomis 2006 for an overview). While one might say that the mainstream of philosophy of physics today sides with geometric empiricism, the debate should not be regarded as settled. Recently, Yemima Ben Menahem (2006, 80-136) has argued that geometric conventionalism remains a live option. Among other things, she appeals to the fact that physicists like Richard Feynman (1971) have offered expositions which first develop the equations of GTR from considerations about gravitational fields; the identification of the Einstein tensor as determining the *geometrical* structure of space-time is added only at a later stage, as an *interpretation* of the equations.

Ben Menahem also proposes to distinguish two general elements that characterize the philosophical legacy of conventionalism in a wider sense: On the one hand, the conventionalist thesis has been extrapolated from its earliest articulations to a general explanation of how mathematical and logical truths are grounded in convention. The predominant author here is Carnap (see chapter 4). On the other hand, the conventionalists' core idea of underdetermination of theory by evidence has been widely received and developed further, in particular by Quine (see chapter 5). Ironically, the second of these two conventionalism-based traditions ultimately turned into a trenchant critique of the first: If the Duhem thesis is radicalized into the claim that we can choose *any* part of the theory and hold it true come what may, then the project of sharply distinguishing the logico-mathematical framework of a theory which is due to convention from the theory's content which is due to experience seems to become a futile undertaking.

5. Further reading

Ben Menahem (2006) gives a wide-ranging and detailed study of conventionalist philosophies. The development of non-Euclidean geometries and their interpretations that led up to Poincaré's insights are presented in Torretti (1978). Poincaré's philosophy is explored in Zahar (2001) and Folina (1992). On Duhem, see Martin (1991), Jaki (1984) and the papers collected in Ariew / Barker (1990). Ryckman (2007) surveys Reichenbach's (and other Logical Empiricists') contributions to philosophy of physics. On Reichenbach, see also the essays in Salmon / Wolters (1994), Spohn (1991) and Salmon (1979).

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