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Rationalism in Science

DAVID STUMP

If rationalism is to be defined, in part, as the belief that at least some of our knowledge of the world is gained by pure reason alone, prior to experience, then science, as the main example of human knowledge, should be a focus of discussion in philosophical debates over rationalism. Although the traditional characterization of modern philosophy as a debate between the British empiricists and the continental rationalists that was superseded by Kant has been widely acknowledged to be problematic for various reasons, recovering the scientific influences on modern philosophers will be the key to discussion here. Descartes and Leibniz were scientists as much as they were philosophers, and Locke explicitly claimed to be representing scientists. Hobbes, Berkeley, and Kant, who, unlike Descartes and Leibniz, are classified purely as philosophers, also engaged in scientific studies, albeit with mixed success. Furthermore, in both the early modern and in later periods, scientists such as Newton, Boyle, Herschel, Helmholtz, Duhem, Mach, Poincaré, and Einstein all wrote on philosophical topics. To a great extent, the philosophical debate between rationalism and empiricism took place within science.

The fact that the philosophical debate over rationalism and empiricism followed in the wake of the establishment of modern science does not seem accidental. The new science was taken to be the best, if not the only way, to discover the true nature of the world, most significantly expressed in laws of nature in mathematical form. Perhaps some claims of the newness of modern science and of its overwhelming superiority to scholasticism are simply overstated rhetoric, but nevertheless, both sides of the philosophical debate over rationalism and empiricism embraced modern science. Since the distinctive trait of modern science is taken to be its combination of experiment and the application of reason, especially mathematics, to the study of nature, it may seem paradoxical that the philosophical debates over rationalism and empiricism should arise, given that modern science could be viewed as the ideal compromise between rationalism and empiricism. In fact, the roles of reason and the senses in knowledge and in the formation of ideas are at stake in the philosophical debates over rationalism and empiricism precisely because of the inconsistent claims made about their roles in the new science. These debates can be seen as having been born out of the methodological reflections on the relative roles of experiment and reason in creating the success of the new science.

The rationalist claims that some part of scientific knowledge about the physical world is *a priori* – known through reason or intellectual intuition – while the empiricist

claims that knowledge about things in the world can only be obtained through experience. Both sides accept the ability of the mind to formulate and to understand representations of nature and acknowledge the role of perceptual knowledge in science and in everyday experience, but empiricists claim that reason is limited to what Hume calls the "relations of ideas," that is, the defining of one term by means of another, or the discovery of the logical consequences of propositions (*Enquiry* IV). It is important to note that a rationalist need not be committed to *a priori* knowledge of the existence of anything, nor of the properties of any individual object, but rather only to general claims about the nature of things in the world. For example, a rationalist might claim that geometry expresses the real nature of space and the things in it, so any triangle (or even something that approximates to a triangle) in nature must have certain characteristics that we can discover *a priori*. Once we know that a triangle is a three-sided figure, we can use pure reason to show that the sum of the three angles of any triangle must be equal to two right angles (Descartes 1984: 45; AT 64). Rational intuition is claimed to tell us how the world must be, since the general principles and laws that the rationalist claims to discover are not contingent facts.

The burden of proof for the rationalist is explaining what rational intuition is and why we should think that it will reliably tell us something about the world. A consideration of thought experiments that purport to give an *a priori* justification of claims about the physical world will be part of the basis of this discussion. Even if thought experiments seem to lead to reliable knowledge about the world, the rationalist cause also carries the burden of explaining the overthrow by later science of various principles that were claimed to be *a priori*, necessary, and known with certainty. A consideration of some of the philosophical response to the development of non-Euclidean geometries will be the basis of this discussion. The burden of proof for the empiricist in this argument, especially after Kant, is to show how science can exist without any *a priori* knowledge. While Kant limited reason and acknowledged that experience is the main source of scientific knowledge, he also argued that there is a residual element of *a priori* synthetic knowledge, what we might now call the theoretical elements of a science, that cannot be eliminated. For example, Kant argued mathematics is both *a priori* and synthetic, that is, it tells us more than Hume's relations of ideas convey. Since mathematics is clearly central to science, it becomes a major stumbling block to the empiricist claim that there is no *a priori* element in science. This suggests that a good way to investigate rationalism in science is to ask whether there are some elements of science that are intractably *a priori*. Mathematics, a few fundamental principles or laws of nature, and other theoretical elements of science seem to be good candidates for *a priori* knowledge for which the empiricist will need to provide an account. Thus, the focus of this chapter will be on a few illustrative examples of potentially *a priori* sources of knowledge in science: thought experiments, mathematics, and theory in science.

The New Experimental Science as a Challenge to Intuition

Although it is still a matter of controversy among historians of science, many now say that there is much more continuity between early modern science and medieval

scholasticism than the standard histories of the scientific revolution allow (Barker and Ariew 1991; Duhem 1980; Martin 1991; Lindberg and Westman 1990; Freeland and Corones 2000). The question of continuity is important here because the distinguishing mark of modern science is often taken to be its empirical nature, despite the fact that, as much as anyone, Descartes and Leibniz, the exemplary scientific rationalists, defined themselves in opposition to scholasticism and in favor of the new science (e.g., see Garber 1992, 1995). Consider Bertolt Brecht's *Life of Galileo*, a surprisingly influential history of the scientific revolution that is more fun and perhaps only slightly more fictitious than some of the standard histories of science. Brecht's Galileo disproves Aristotle's theory of why ice floats by performing a simple experiment, while the characters of the mathematician and the philosopher refuse to look at the moons of Jupiter through the telescope, insisting that *a priori* argument alone settles the question of their existence (Brecht 1967: 63, 32). Empirical evidence proves that *a priori* methods are bankrupt.

Of course, there are far more nuanced ways for advocates of modern science to make the case that the scientific revolution marked a radical break from scholasticism. Interpretation of the complex and comprehensive works of figures such as Galileo, Descartes, Newton, and Leibniz in simple terms is difficult. As noted above, the development of modern science is often viewed as the unification of mathematical methods and experimental methods, embodied especially well in figures such as Galileo and Newton. Advocates of this position seem to claim that rationalists do not take the role of experimental science seriously enough, or indeed think that actually conducting an experiment is superfluous. In the concluding section of the first study in the famous *Harvard Case Histories in Experimental Science*, James Bryant Conant remarks:

> The development of experimental science in the seventeenth century was the consequence of the combination of deductive reasoning with the cut-and-try type of experimentation. Two great figures of this period who contributed to the study of pneumatics symbolize the two traditions whose combination produced modern science. Blaise Pascal was primarily a mathematician, Robert Boyle primarily an experimentalist . . . In Pascal's treatise on hydrostatics and his work on pneumatics it is hard to tell whether or not most of the so-called experiments were ever performed. They may well have been intended rather as pedagogic devices – as demonstrations that the reader performs in his imagination in order better to understand the principles expounded . . .
>
> Boyle in one of his discussions of hydrostatics (1666) gently pokes fun at Pascal for having written of experiments that appeared impossible of execution . . . As an example of some of the things that Pascal described that strained one's credulity, Boyle refers to an experiment in which a man sits 20 feet under water and places against his thigh a tube that extends above the surface of the water. (Conant 1957: 59–60)

In contrast to Pascal, Boyle set the standard for reporting complete results of experiments, even when they seemed irrelevant and especially when they were negative. He was also meticulous about providing detail so that experiments could be reproduced, though the air pumps that he had built were beyond the technical resources of all but a few elite centers of scientific research. In the history of science, whole areas of inquiry and knowledge sometimes depend on the invention of a new piece of equipment to investigate nature. Boyle claimed that facts were exhibited in the air pump, but the

experiments were very difficult to carry out. It took two strong men "divers hours" to evacuate the chamber, it constantly leaked, and phenomena were open to multiple interpretations (Conant 1957: 9). Granting that proper empirical investigation leads reliably to scientific knowledge, we can consider here whether thought experiments can also lead reliably to such knowledge.

Advocates of thought experiment sometimes claim that a successful thought experiment provides us with genuine knowledge about the physical world by pure reason alone. This has been called the "paradox of thought experiment" by Horowitz and Massey (1991) and has also been recognized as a rationalist position (e.g., Sorensen 1992). As an antidote to Brecht, and to utilize a scientific thought experiment that most commentators take to be convincing, we can consider Galileo's refutation of Aristotle's law of free fall, not by dropping balls off of the Tower of Pisa, but rather by an argument that appears in *Discourse on Two New Sciences* (Galilei 1974: 66ff.). According to Aristotle, heavier objects fall faster than lighter objects. Galileo asks us to consider what will happen to the rate of fall of a heavier and a lighter object if they are combined in various ways, assuming that objects do indeed fall in accordance with Aristotle's law. Consider putting the smaller object directly on top of the larger one before dropping them. If the objects fall in accordance with Aristotle's law, they should separate and each fall at the speed at which they would have fallen if they were never in contact. If the heavier object is put on top, however, it will push down on the slower, smaller object, which in turn will act as a brake on the larger one. The resulting speed will be in between that of the two objects falling separately. If the objects are connected, we can also imagine non-uniform results. If the connection is a flexible chain, for example, the heavier object should fall faster at first, but then experience drag from the slower-moving object chained to it. On the other hand, if there is a rigid connection between the two objects, we might want to say that they together form a new heavier object that will fall even faster. However, this situation is hardly different from the case where the heavier object is simply pushing down on the lighter one, without being connected at all. We have arrived at the apparently contradictory result that the combination of two objects will sometimes fall faster than the heavier of the original two alone, and sometimes fall slower. Galileo concludes that the way to avoid this result is to give up Aristotle's law. Indeed, he says that the natural conclusion to draw from this thought experiment is that free fall always occurs at the same rate, no matter what the weight of the object. If Galileo's argument is correct, free fall must be independent of weight, so (if we replace the term "weight" with "mass") it would seem that the modern law of free fall is not empirical at all. Furthermore, the law of free fall would be necessary in a very strong sense because, if Galileo is right, it is the only possible law of free fall. Contrast this law of nature to another law of nature that governs the behavior of physical bodies: light travels at 186,000 miles per second and we discovered that speed empirically. It seems natural to say that we might have discovered that the speed of light was much faster or much slower.

One response that the empiricist might make to thought experiments is to claim, like Mach, that thought experiments do not by themselves lead to knowledge about the world – even successful thought experiments must be repeated physically. While thought experiments might be useful as guides to research, pedagogy, etc., they are in a strict sense superfluous (Sorensen 1992: 61–2). John Norton (1991) clarified this

line of response by defending the view that thought experiments are arguments (cf. Bishop 1999). In the philosophical debate between rationalists and empiricists, both sides accept that genuine knowledge can be logically deduced from known premises. Therefore, if the premises of a thought experiment are already known empirically, they are unproblematic; if not, the advocate of the thought experiment must be claiming that there can be a non-empirical source of knowledge about the physical world. Whether the empiricist can be convincing would seem to depend on whether or not all thought experiments can be seen as arguments, and that it is possible to obtain the information resulting from the thought experiment in some empirical way.

Another response that the empiricist might make is to simply deny that the thought experiment leads to the given conclusion. For example, Alexandre Koyré's analysis of Galileo's thought experiment focuses on the issue of whether the connected bodies form a new unitary object or not. According to Koyré, Galileo has not derived a contradiction from Aristotle's law of free fall, but rather has given one case where there is a new single object that falls faster than the original heavier object and another case where there are two separate objects that fall slower than the original heavier object (Koyré 1968: 51). The question of whether or not a connection between the original two objects makes it into a new unitary one is vexing. Perhaps we can agree that the connection must be rigid, but how rigid? What if the distance between the two objects was great? There is at least some ambiguity about what to call a single object here, but perhaps not a contradiction, as Galileo claimed.

Dijksterhuis (1986: 327) rejects Stevin's purported proof of the law of the inclined plane in a very similar manner. Stevin's argument depends on the assumption that perpetual motion is not possible, even in an idealized, frictionless system. Since Stevin has not justified his claim that perpetual motion is impossible in an idealized system, Dijksterhuis refused to accept the validity of the argument. If an empiricist can show that the thought experiment does not lead to the given conclusion, then there is no risk of thought experiments being used to show that rationalism is true. Faulty arguments do not show that thought experiments can lead to genuine knowledge of the physical world.

Geometry and Intuition

As noted above, both rationalists and empiricists accept argument as a source of genuine knowledge; that is, it can be logically deduced from known premises. However, rationalists claim that some fundamental principles or laws of nature can be known *a priori* by intellectual intuition, while empiricists claim that such principles must either be definitions in disguise, like Hume's relations of ideas, or else they must be justified empirically. Intellectual intuition is understood as a kind of "grasping" by which we recognize the truth of a proposition or understand the meaning of an idea. Descartes spoke of the "natural light" by which we could understand "clear and distinct ideas" and recognize their certainty. Propositions known by intellectual intuition are said to be "self-evident," that is, needing neither logical demonstration nor evidence gained from sense experience. As such, they are taken to express necessity and to be known with certainty, but not all claims of *a priori* knowledge seem certain and many have

been shown to be false by later developments in science (Hahn 1933). This hardly makes *a priori* claims look like candidates for reliable knowledge. Equally as problematic for the rationalist as changes in supposedly certain and obvious principles, however, is the fact that the rationalist cannot explain such a principle or convince someone who does not understand it.

Kant tried to correct the use of reason in science by limiting its scope. Kantian critical philosophy does not go beyond what it can legitimately prove. Many traditional philosophical issues are ruled out of court as unanswerable and many others are left to empirical science to settle. Despite the limits that Kant set out and despite the fact that Kant was deeply engaged with the Newtonian exact sciences of his time and even produced some original scientific work, Kantian philosophy was also proven wrong by later advances in science. The fall of Euclidean geometry from its place in Kant's philosophy is perhaps the most famous case of a former bit of necessary and certain knowledge that became merely one of several alternatives that are perhaps not true at all. Kant argued that Euclidean geometry is a true description of space (space as a phenomenon, not as noumena) and that knowledge of space and, hence, knowledge of Euclidean geometry, was a necessary precondition for the possibility of any science at all. Kant had argued that our knowledge of Euclidean geometry is thus both *a priori* and synthetic, and it is also certain because alternatives to Euclidean geometry are impossible. To make a long story far too short, mathematicians first showed that alternatives to Euclidean geometry were possible and physicists (especially Einstein) next showed that the actual structure of space was not Euclidean.

Euclidean geometry has zero curvature, there is exactly one parallel to a given line through a point outside that line, and the sum of the interior angles of a triangle is equal to 180 degrees. Bolyai-Lobachevskii geometry has negative curvature, there is more than one parallel to a given line through a point outside that line, and the sum of the interior angles of a triangle is less than 180 degrees (how much less depends on curvature). Riemannian geometry has positive curvature, there are no parallel lines, and the sum of the interior angles of a triangle is greater than 180 degrees (how much greater depends on curvature). Riemann went even further and developed general coordinate systems that can describe spaces of variable curvature. After the acceptance of non-Euclidean geometries around 1870, Helmholtz directly challenged Kant's view of geometry (Helmholtz 1977a, 1977b; Nowak 1989).

Not all was lost for defenders of Kant, however. Even though Kant seemed to say that only Euclidean geometry was possible, he could not have meant that other geometries were logically inconsistent. One of the criteria that Kant put forward as a definition of an "analytic" statement is that its negation leads to a contradiction. If alternatives to Euclidean geometry were contradictory, they would have to be analytic in Kant's terms. But Kant thought that both geometry and arithmetic were synthetic, that is, they are not merely built up from explicit definitions, but also require intuition in a quite specific sense. Intuition is what allows us to construct figures and objects in our minds. Thus, the neo-Kantian of the early twentieth century could maintain part of Kant's claims about intuition that leads to *a priori* knowledge of the world, despite the mathematical challenge (Renouvier 1889, 1892).

Of course, the situation changed dramatically with Einstein's General Theory of Relativity, in which space (more precisely, space-time) has no fixed structure at all

independent of the matter and energy distributed in it. The geometry of space is determined empirically, not *a priori*, and far from being necessary, Euclidean geometry is not even true. It is, of course, approximately true of small regions of space-time and good enough for engineering, but so are all of the alternative metric geometries. Einstein's General Theory of Relativity had a tremendous impact on Schlick, Carnap, and Reichenbach, the great founders of philosophy of science as we know it today. All three wrote philosophical interpretations of Einstein's Theory of Relativity in the 1920s and 1930s, and developed their strictly empiricist philosophy in response to the new physics (Carnap 1922; Schlick 1920; Reichenbach 1965, 1969).

Prior to the development of the General Theory of Relativity, Henri Poincaré had extended Kant's strategy of giving up one kind of *a priori* knowledge while maintaining others, arguing that while arithmetic follows from a synthetic and *a priori* intuition of the concept of whole number and of the principle of mathematical induction, our intuition of the metric properties of bodies is completely empty. The issue to consider here is how Poincaré came to view geometry and arithmetic so differently and whether or not there can be a consistent criterion for accepting rational intuition in one area of knowledge while rejecting it in others. Poincaré's basic position on the role of intuition in geometry is developed in an 1889 article on logic and intuition, and is repeated in several places in his works (Poincaré 1889). Like Mach, he accepts the role of intuitive arguments in pedagogy and in scientific discovery, in addition to its special synthetic *a priori* role in the epistemology of arithmetic. Poincaré also recognizes the increasing demand for rigor in mathematics. Crucial to this rigor is the idea that we must ferret out all of our implicit assumptions, replacing them with explicit definition and strict proof. For geometry especially, this work culminates in Hilbert's *Foundations of Geometry*, and in a review of this work, Poincaré agrees that rigor demands avoiding all appeals to intuition:

> Is the list of axioms complete, or have we let escape some that we apply unconsciously? This is what we need to know. To find this out, we have one and only one criterion. We must investigate whether or not the geometry is a logical consequence of the explicitly stated axioms; that is to say, if these axioms, entrusted to a reasoning machine, could produce the entire series of geometric propositions. If they can, we will be certain that we have not forgotten anything, because our machine cannot function except according to the rules of logic by which it was constructed. It does not know of this vague instinct that we call intuition. (Poincaré 1902c: 269)[1]

Poincaré argues that geometry concerns only the properties that are common to all of the alternative metric geometries. Since projective geometry studies the properties of figures that are invariant under a group of projective transformations, groups alone are fundamental:

> The different ways in which a cube can be superposed upon itself, and the different ways in which the roots of a certain equation may be interchanged, constitutes two isomorphic groups. They differ in matter only. The mathematician should regard this difference as superficial, and he should no more distinguish between these two groups than he should between a cube of glass and a cube of metal . . . What we call geometry is nothing but the

study of formal properties of a certain continuous group; so we may say, space is a group. (Poincaré 1898: 40, 41)

Poincaré's central argument for the elimination of metric intuition from geometry is that geometric objects can be stipulatively defined from more fundamental objects. In his early work, groups play this role, since all of the metric properties of geometries can be expressed in the theory of continuous groups (Poincaré 1889: 129). Thus, part of what had seemed to be *a priori* knowledge to Kantians can be seen to be "empty," as empiricists suggested.

There is a strong tradition in nineteenth-century mathematics of interpreting unknown (or seemingly impossible) objects as combinations of known simples. This tradition goes back at least to Hamilton's geometric interpretation of complex numbers, and continued in Beltrami and Klein's early treatment of non-Euclidean geometries. Poincaré follows tradition; however, there are limits to this process, and a synthetic *a priori* element must be maintained in science, according to Poincaré. Thus, groups maintain their status as innate concepts that are known *a priori* (Poincaré 1889; Picard 1901), and our knowledge of arithmetic depends essentially on the principle of mathematical induction and on the concept of number taken to be a synthetic *a priori* element of science. Indeed, distancing himself from the "global conventionalism" of Elouard Le Roy, Poincaré distinguishes three elements of science – *a priori*, conventional, and empirical:

> Here are three truths: (1) The principle of mathematical induction; (2) Euclid's postulate; (3) the physical law according to which phosphorus melts at 44° (cited by M. Le Roy). These are said to be three disguised definitions: the first, that of the whole number; the second, that of the straight line; the third, that of phosphorus. I grant it for the second; I do not admit it for the other two. I must explain the reason for this apparent inconsistency. First, we have seen that a definition is acceptable only on condition that it implies no contradiction. We have shown likewise that for the first definition this demonstration is impossible; on the other hand, we have just recalled that for the second Hilbert has given a complete proof. As to the third, evidently it implies no contradiction. Does this mean that the definition guarantees, as it should, the existence of the object defined? We are here no longer in the mathematical sciences, but in the physical, and the word existence has no longer the same meaning. It no longer signifies absence of contradiction; it means objective existence. (Poincaré 1920: 185–6; 1913: 468)

Metric primitives can be explicitly defined in terms of more basic primitives and a complete proof of the consistency of the metric geometries can be given formally. However, in reply to logicism and formalism, Poincaré argues that it is impossible to prove the consistency of arithmetic without using mathematical induction or its equivalent (Poincaré 1902a, 1920). Therefore, while one can eliminate metric geometry by taking all of geometry to really concern numbers plus some group theoretic or topological primitives that are non-metric, one cannot eliminate arithmetic and our intuitive knowledge of mathematical induction. In a second argument presented on the following page, Poincaré repeats his claim that geometric (i.e., metric) terms can be "defined away," while arithmetical ones cannot, arguing that whole number and the principle of mathematical induction have equivalent definitions, but only in virtue

of a synthetic *a priori* judgment, not on the basis of an explicit stipulative definition (Poincaré 1920: 188–9; 1913: 469–70).

After reading Hilbert's *Foundations of Geometry*, Poincaré extended his strategy of eliminating primitive terms in geometry. In a review of Hilbert (Poincaré 1902b) and even more explicitly in the nomination that he wrote for the awarding of the third Lobachevskii prize to Hilbert (Poincaré 1904), Poincaré argues that Lie's work contains an artificial limit: the study of continuous groups. Hilbert shows how to move beyond this limit and thus overcome the idea that "group" is a primitive concept. However, this simply moves the intuitive element back to a more abstract level, from projective geometry to topology. Referring to Hilbert's second group of axioms – the axioms of order – Poincaré says:

> The axioms of order are presented as dependant on projective axioms, and they would not have any meaning if one did not allow the latter, since one would not know what three points in a straight line is. And yet, there is a peculiar geometry which is purely qualitative and which is absolutely independent of projective geometry, that does not presuppose as known either the notion of a straight nor that of a plane but only the notions of line and surface; it is what one calls topology. (Poincaré 1904: 8)

How are we to understand Poincaré's arguments that metric geometry is a formal, non-intuitive science, when at the same time he defends intuition in arithmetic? First, Poincaré's arguments for formalism in geometry and against formalism in arithmetic both seem remarkably question begging. His view that we have no geometric intuition seems to be equivalent to his acceptance of the consistency of non-Euclidean geometries and his rejection of formalization of arithmetic seems to depend on his belief that the principle of mathematical induction or its equivalent is fundamental to arithmetic in a way that metric was not fundamental to geometry.

Given what Poincaré says about the meaning of primitive terms in geometry, it is unclear how we can know in advance what an arithmetical system is. Negating the parallel postulate obviously changed the metric properties of geometry – indeed, the very idea of what a straight line is. Nevertheless, non-Euclidean geometries were accepted. It seems compelling to say that we need mathematical induction to show that arithmetic is consistent and that everyone accepts consistency as a requirement. However, the consistency of Bolyai-Lobachevskii was only gained by removing metric properties from what counts as geometry. Why could not the same argument be made in arithmetic?

A second problem for Poincaré and for rationalists generally is how to maintain a distinction between acceptable and unacceptable forms of intuition. For example, what do we say about the axioms of topology? Or the continuum? We could say (a) that there is no geometric intuition, and that "analytic geometry" is fundamentally arithmetical, rather than geometric. This interpretation fits the early passages quoted above. Or, we could say (b) that there is a limited form of geometric intuition, the analytical or qualitative part. This second interpretation fits the later passages that I have cited and also Poincaré's remarks on the intuition of the continuum and his rejection of the arithmetization of the continuum (see Folina 1992: ch. 6). Under both interpretations, however, Poincaré still has to claim that some intuition is completely bankrupt, while

other intuition is *a priori* knowledge in a classically rationalist sense and thus true and certain. Under interpretation (a), Poincaré can at least argue that while geometric intuitions are problematic, arithmetic intuitions are not. Under (b), the problem is amplified, since Poincaré must say that some geometric intuitions are reliable, while other kinds of geometric intuition are not. A single neat classification of intuitions would be preferable.

One of the most promising ways to understand Poincaré's rejection of some forms of intuition while maintaining other forms as necessary is to see his view as stemming from a distinction between intellectual and sensual intuition.[2] The neo-Kantians of the end of the nineteenth century had given up Kant's intuition as a form of sensibility, but left a conceptual intuition of the categories of the understanding in place. Poincaré seems to be firmly in this tradition, since he maintains that we can create geometry without any representations at all:

> The words, point, straight, and plane themselves should not cause any visual representation. They could arbitrarily designate objects of any nature, provided that one can establish a correspondence between these objects such that for all systems of two objects called points there corresponds one, and only one, of the objects called straights . . . The reasoning ought to be able, according to [Hilbert], to lead to purely mechanical rules, and to do geometry, it is sufficient to apply strictly the rules to the axioms, without knowing what they mean. One will in this way be able to construct all of geometry, I would not exactly say without understanding it at all, since one grasps the logical connection of the propositions, but at least without seeing anything. One could give the axioms to a reasoning machine, for example the logical piano of Stanley Jevons, and one would see all of geometry come out. (Poincaré 1904: 6–7)

Thus, Poincaré firmly rejects intuition as a visual representation of geometric objects. However, he claims that we need some conceptual intuition in order to understand mathematics, if only to understand what is explicitly in the axioms. Presumably, the reason why Poincaré would reject the idea that Jevons' logical piano could understand geometry is that it does not have the necessary *a priori* intuition of topology and arithmetic needed to do so.

A third and final problem with all Kantian and post-Kantian arguments for rationalism is that they are mostly negative: rationalists are only able to tell us that intuition leading to synthetic *a priori* knowledge is necessary to understand mathematics and science. Any attempt by Poincaré to argue that the mind has special intuitive capacities must be suspect, because he definitely changed his view on what is taken as primitive and intuitive. Originally, geometry is nothing but a group, but later it is even less than that. Poincaré's change of mind does not inspire confidence in intuition or in claims about what can and cannot be known intuitively. Will he now say that it is a topological concept that preexists in the mind? On the contrary, Poincaré even says that some topological properties of space are conventional, as well as intuitive and necessary; in particular, that the number of dimensions of space is conventional. However, he may be excused for such inconsistent views in this case, since the topological invariance of dimension was not completely understood at the time. The first proof was given by Brouwer in Dutch in 1911, one year before Poincaré's death (see Johnson 1979, 1981).

The Mathematical Tradition and Theoretical Science

It would certainly be uncontroversial to say that advances in mathematics were repeatedly responsible for tremendous progress in science. Archimedes solved practical experimental problems with Euclidean geometry, Descartes' invention of analytic geometry allowed him to invent a new physics as well, and Newton and Leibniz dramatically extended the mathematical methods available for science with their co-discovery of the calculus. In the nineteenth century, Gauss, Riemann, and many others made major contributions to extend the mathematical tools of physics. Fisher did the same for biology in the twentieth century and, of course, there are more such examples. The success of mathematics once again puts the burden of proof on the empiricist, who must explain why mathematics works. As before, the empiricist is left with the strategy of either arguing that mathematics is "formal" and does not tell us anything about the world, or instead that it is empirical, but the burden of proof seems even higher than it was in the case of thought experiments, since the use of mathematics cannot be eliminated from science (Field 1980; Sober 1999).

It seems ironic that the development of Newtonian mechanics is frequently described as a competition between English experimentalism and French analysis, thus echoing in a scientific context the traditional British empiricist vs. continental rationalist account of the philosophical debate. The development of Leibniz's version of the calculus by French mathematicians such as Pierre-Simon de Laplace and Joseph Lagrange put French mathematics far ahead of English mathematics. The subsequent applications of analytical methods not only to classical Newtonian mechanics but also to thermodynamics, by Fourier, electromagnetism by Ampère, and light by Fresnel, were extraordinarily successful. Poincaré, who is often said to be the last in this line of French mathematical physicists, expresses the traditional divide between the British and continental scientific styles, but finds that the debate between those who see science as *a priori* and those who see it is as experimental can be resolved by understanding some elements of science as hypothetical:

> The English teach mechanics as an experimental science; on the Continent it is taught always more or less as a deductive and *a priori* science. The English are right, no doubt . . . the difficulty is largely due to the fact that treatises on mechanics do not clearly distinguish between what is experiment, what is mathematical reasoning, what is convention, and what is hypothesis. (Poincaré 1982: 89)

We would likely now say that a highly mathematical science is "theoretical" instead of *a priori*, which is indicative of the extent to which rationalism has been out of favor. Of course, even with this change in terminology it would be an overstatement to say that the French success at analysis led to their total neglect of empirical science. For example, Ivor Grattan-Guinness (1984) has noted how much emphasis the French put on engineering. On the other side of the Channel, it seems impossible to justify calling Newton an empiricist, even if he did say, famously, that he will "frame no hypotheses." Indeed, many have argued that the most philosophically appealing aspect of Newton's methodology was his resistance to the extremes of empiricism and rationalism (Stein 1990), expressed in the recognition of a need

for both analysis and synthesis and for empirical experimentation (Hankins 1985: 20).

From Aristotle onwards, an analytic and synthetic method was distinguished in natural philosophy. One can either start from first principles and show how phenomena can be explained by them, or one can start with phenomena and infer fundamental laws of nature. Modern science adopted a hypothetical method, justifying the first principles or the existence of theoretical entities solely by their empirical adequacy. Thus, the only test for the truth of a hypothetical law of nature or the existence of a theoretical entity is whether or not a theory is consistent with observation. According to this contemporary view, there is never a need for *a priori* rational justification in science. Hypotheses are first adopted provisionally without evidence, and subsequently either accepted or rejected on the basis of empirical evidence. The hypothetical method that Poincaré mentions became the mainstream in late nineteenth- and early twentieth-century philosophy of science and in science itself, generally at the expense of rationalism, since the theoretical elements of a scientific theory might be understood without any appeal to rational intuition.

As noted at the beginning of this chapter, conflicting philosophical interpretations of the methodology of modern science can be seen as precipitating the rationalist–empiricist debate. Even with substantial agreement that rationalism in science was dead, a philosophical debate ensued over what follows from the evidence for hypotheses that we obtain empirically. So called "scientific realists" believe that we can legitimately claim to know that theoretical entities exist and that scientific theories are true, while "empiricists" or "instrumentalists" believe that theoretical entities are convenient fictions and that scientific theories are empirically adequate, but not true (or known to be true). Although "empiricist" has become the preferred name for the philosophical position that I am describing here, and "instrumentalism" is associated with a view that has been widely repudiated, I will use the term "instrumentalist," since both scientific realists and instrumentalists are very likely to be empiricists in the sense of the debate between rationalists and empiricists at issue in this volume. Indeed, scientific realism and instrumentalism can each be seen as a rejection of rationalism in science by means of showing that the success of science can be explained entirely by reference to what is learned empirically. While scientific realists claim that when we have the appropriate evidence, empirical data give us a legitimate claim to knowledge, even if that knowledge is indirect, instrumentalists claim that theoretical claims in science are never validated, empirically or by any other means. Both realists and instrumentalists consider the former *a priori* elements of scientific knowledge to be theoretical. Realists claim to show that all scientific knowledge is empirical, despite the existence of theoretical elements in science for which there can be no direct empirical evidence. Instrumentalists claim that empirical methods should strictly limit claims of scientific knowledge to phenomena and that theoretical elements are dispensable. The overwhelming majority of both scientific realists and instrumentalists reject rationalism, seeing no need for *a priori* synthetic claims in the sciences.

The theoretical aspects of science have not been so easy to dismiss, however. Indeed, major figures in twentieth-century science, starting with Einstein, are known for their theoretical work in science. While documenting the development of theoretical science, especially in Germany, Jungnickel and McCormmach (1986) emphasize how recent

our current conception of theoretical science really is and what a major part of science it has become. Stephen Toulmin's *The Return to Cosmology* (1982) does not claim that theoretical science is new, but it does make a strong argument that there is far more theoretical work in science now than before and that science must include elements that are theoretical or even speculative. One striking feature of twentieth-century philosophy of science is the extent to which the topics that it took up are precisely those that had been considered *a priori* knowledge by Kant, such as geometry, space and time, causality, and the principles violated by quantum mechanics. While philosophers are not required to conclude that theoretical science is evidence for rationalism, its centrality does show that much of science is not empirical in any straightforward sense. Indeed, although synthetic *a priori* knowledge was officially rejected by the Vienna Circle in the 1929 manifesto (see Neurath and Cohen 1974), statements about the former Kantian *a priori* have often been given a special role, either as conventions, or as the hard core of scientific theories. Quine's critique of the analytic–synthetic distinction was supposed to replace the notion of any special status for what was formerly considered to be *a priori* with a thoroughgoing empirical holism, but several authors have questioned Quine's result (Creath 1991; Friedman 2001; Stein 1992; Richardson 1997). Going beyond mere critique of Quine's empiricism, some have even advocated a return to a form of rationalism, giving some element of our knowledge a special *a priori* status (De Pierris 1992; Friedman 2001).

The development of non-Euclidean geometries and especially of the General Theory of Relativity made a profound impact on philosophy of science in the early twentieth century by showing philosophers that the most fundamental aspects of physical theory could change. Geometry, space and time, causality, and the fundamental principles of physical theories were still seen to have a special role, even if Kant's rationalism was rejected. Mach and Poincaré took such elements of science to be conventions, a matter of free choice, regardless of what experiment says. Slightly different versions of conventionalism were developed by C. I. Lewis, Victor Lentzen, Arthur Pap, and Russell Norwood Hanson. Ian Hacking (1992) has advocated something similar to conventionalism with his notion of "styles of reasoning." Many of these treatments of *a priori* knowledge share the idea that what Kant took to be necessary can and indeed has changed through the development of scientific theories.

Conceptual change of this sort is what many postpositivist philosophers of science, such as Kuhn, Toulmin, Laudan, and Shapere, saw as a fundamentally important aspect of science. Indeed, Kuhn (1990) eventually (and tentatively) picked up the idea of a conventional treatment of synthetic *a priori* knowledge as an explication of his views. The idea that there are revisable conventions at the heart of science was already well developed prior to logical positivism, continued in logical positivism itself, and was maintained even in postpositivist philosophy of science. While Quine admits that some elements of empirical theory are much less likely to be revised than others, he underestimates the asymmetric relations between the "hard core" and the "periphery." It is not just that the "periphery" is more likely to be revised than the "hard core," but rather that the statements of the "periphery" cannot even be stated, let alone tested, without the "hard core" functioning in the Kantian sense as a necessary precondition. It is possible that fundamental elements of science will be *a priori* in a functional sense, given that they must be chosen prior to proceeding with any

theoretical or empirical work, and that these elements can be justified neither by rational intuition, nor by empirical test (Stump 2003). If they remain untestable empirically, and cannot be justified by rational intuition, there will be a component of scientific theory that falls outside of the range of the options provided by traditional philosophical rationalism and empiricism.

Notes

1   The "reasoning machine" to which Poincaré refers is Stanley Jevons' "logical piano," the logic of which is equivalent to Venn diagrams. Jevons demonstrated his machine in 1866 (Jevons, 1869: 59–60; 1958: 170ff.). The machine was conceived as a "logical abacus," a set of blocks representing subject, predicate, and middle terms. With the addition of levers to move the blocks, Jevons developed a sort of logical adding machine (see Gardner 1982: ch. 5).
2   Michael Detlefsen suggested such a distinction in his talk at the 1994 Poincaré Congress in Nancy, France. If the distinction can be maintained, we can credit Poincaré with having at least one clear way of distinguishing acceptable and unacceptable uses of intuition.

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