

Mathematics, motion, and truth: the Earth goes round the Sun

Abstract

The reality of the Earth's motion, as proclaimed by Copernicus, quickly proved contentious. Accepted by Kepler, disputed by theologians (Lutheran and Catholic alike), veiled in suggestions of mere convenience, adopted and explained by Newton as a consequence of universal gravitation, parent of the notion of force – what is involved in accepting as true that the Earth goes round the Sun? This talk traces these debates from the early 1600s to the time of Poincaré.

Jeremy Gray

Open University and University of Warwick

1 Copernicus

Copernicus's *De revolutionibus orbis coelestium* (On the revolutions of the celestial spheres) was published in 1543, the year of his death. It is famous for shifting opinion from an Earth-centred or geocentric cosmos to a Sun-centred or heliocentric one. The heliocentric hypothesis was not original with him. It had been held by the Greek astronomer Aristarchus, could be found in the astronomical writings of Nasīr al-Din al-Ṭūsī, and was revived by Oresme and by Nicolaus Cusanus, a 15th-century bishop. Copernicus himself mentions Aristarchus, but without giving a source, and also refers to related comments by ibn Sina and ibn Battuta, a 14th-century Muslim geographer and traveller whose journeys took him from Morocco, where he was born, to China.

Copernicus had entertained heliocentrism for many years, and was aware of the demandingly high standards of his contemporary astronomers. Which is why, by the way, in Book III, which deals with the motion of the Sun and the precession of the equinoxes, the Sun itself is placed not at the very centre of the universe, but slightly off centre. The book is dedicated to Pope Paul III, doubtless for political reasons.

Its path was smoothed by the preface by Osiander, who asserted that *De revolutionibus* offered an account of the motions of the heavens, but did not provide a true cause or explanation. As he put it, it is the job of the astronomer:¹

– since he cannot by any line of reasoning reach the true causes of these movements
– to think up or construct whatever causes or hypotheses he pleases such that, by the assumption of these causes, those same movements can be calculated correctly from the principles of geometry for the past and for the future too. [...] it is not necessary that these hypotheses be true, or even probably [so].

¹Copernicus (1952, 505).

The major contribution of Rheticus (who greatly improved the trigonometry of the time) is not mentioned, perhaps for political reasons, because he was about to become Dean of Liberal Arts at the University of Wittenberg, a largely Protestant town – but Osiander was also a Protestant, as, of course, was their more famous contemporary Martin Luther. It is to the door of Church in Wittenberg that Luther is said to have nailed his 95 theses.

The University of Wittenberg became associated with promoting Copernicus’s ideas, and Erasmus Reinhold, an astronomer there who later became Dean and Rector of the University, set about producing a set of planetary tables based on them. Their accurate predictions helped to spread the Copernican system, even though Reinhold himself did not accept the heliocentric hypothesis.

It is often said that the Catholic Church was particularly hostile, but in fact as late as 1600 there was no official Church position on *De revolutionibus*, and the leading Jesuit astronomer Christoph Clavius used it in his teaching without endorsing the heliocentric hypothesis. In fact, it was (or may have been, the story is disputed) the Protestant leader Martin Luther who inveighed against Copernicus’s work, on the grounds that it contradicts the *Bible*, and specifically Joshua’s command in Joshua X, xii, that the Sun stand still:

On the day when the LORD gave the Amorites over to the Israelites, Joshua spoke to the LORD; and he said in the sight of Israel, “Sun, stand still at Gibeon, and Moon, in the valley of Aijalon.” And the Sun stood still, and the Moon stopped.

But in the end it was the Church’s condemnation of Galileo that caused the most controversy. The ostensible charge was that Galileo falsely affirmed the motion of the Earth, and the Church’s up-front argument was that his theory of the tides was wrong – as indeed it is and as Clavius could show. But the behind the scene argument was that the Church needed to affirm Biblical literalism as part of its attempts to rebut Protestant theology; the needs of the Counter-Reformation movement over-rode the arguments of astronomers of the wrong sort. The Inquisition sentenced Galileo to life imprisonment, later commuted to house arrest. Famously he is said to have uttered the phrase ‘Eppur si muove’ on leaving the court – ‘But yet it does move’ – the story too is likely apocryphal. The reality of the motion is the theme of this lecture.

2 Newton and Johann Bernoulli

This part of the talk is wholly indebted to the work of George Smith and Niccolò Guicciardini, to whom, and to whose work, you are referred for all detailed questions. Here I am interested in why Newton was persuaded of the inverse square law of gravitation, and how he endeavoured to persuade the readers of his *Principia*.

Christiaan Huygens had shown in 1673 in his *Horologium oscillatorum* that what he called the centrifugal force by which a body constrained to move in a circular orbit of radius r at constant speed v attempts to escape from the centre is given by v^2/r times the weight of the body.²) Now,

²Huygens, *De vi centrifuga*, in *Oeuvres complètes*, Vol. XVI, pp. 255-301, tr. M. Mahoney, available at <http://www.princeton.edu/~hos/mike/texts/huygens/centriforce/huyforce.htm>.

once you – with Newton – are persuaded that uniform (constant speed) motion in a straight line is equivalent to rest, the focus of enquiry into motion must shift to whatever it is that changes velocity. What ever it is is manifested in acceleration, and it was clear to Newton by 1671 that this was handled by a study of circular motion. For bodies moving on any curve whatever their acceleration at any instant is measured by Huygens' quantity at that instant. That is to say, the moving body is considered to be moving, at that moment, on a circle. The acceleration will be said in the *Principia*, to be caused by a centripetal force, and it will be measured by the quantity v^2/r times the mass of the body, where v is the velocity at that instant, and r is the radius of the circle of curvature at the instant.

Newton had been interested in the radius of curvature of a curve since at least 1671, when he gave a formula for it, and of course it is fundamental to Descartes' method of normals, which is equivalent to a method of tangents.

Consider now the problem of celestial mechanics as Newton saw it in the second half of the 17th century. There was an increasing amount of increasingly accurate data about the motion of the planets, the motion of the Moon, and the motion of the four known satellites of Jupiter. There was no agreed theory of how or why they moved as they did, although Newton knew that Continental authors had a good deal of sympathy for Descartes' vortex theory, according to which a swirl of invisible particles goes round the Sun more or less in a plane and pushes all the planets round. This theory explains: why the planets all have their orbits more-or-less in the same plane, and why they all go round the same way. In addition, it makes a natural sense: the planets move because they are pushed.

The way to proceed is, it seemed to Newton, to go from the observations to the explanation of the motion. In the *Principia* Newton showed that if the motion is in an ellipse and the force is directed to a focus of the ellipse, then it is inverse square in the distance of the planet from the focus, and if the motion is in an ellipse and the force is directed to the centre of the ellipse, then it is inverse first power.

Newton identified two problems with this analysis: the observations are necessarily approximate and support a variety of conclusions about the orbit; and the Sun wobbles and so displaces the focus, which means that the orbit cannot actually be an ellipse. He deduced that his conclusions could only be approximate, and set himself the task of wondering how robust they might or could be. He investigated motion in an ellipse to an arbitrary point, motion in eccentric circles, motion in rotating ellipses, motion in near circles. He found that planetary precession was so small that any departure from inverse square could be ruled out, and that even the motion of the Moon conformed to this hypothesis. The inverse square law even held up for orbits that were markedly eccentric and for orbits that were not even perfect ellipses. The conclusion was remarkably robust.

I haven't time on this occasion to discuss the most surprising of these, which concerns the motion of the Moon, but Smith has shown that even here Newton carefully analysed the perturbed, non-elliptic orbit of the motion and found that the rotation of its apses supported the inverse square law. Instead, I want to look at a well-known controversy, which concerns Newton's skimpy treatment of the inverse problem: to deduce from the inverse square law that the motion must be in a conic section. Our guide here is Niccolò Guicciardini.

In October 1710 Johann Bernoulli published a letter he had written to Jacob Hermann, then Professor of Mathematics in Padua, in which he asked for a solution to the problem finding a trajectory given a central force law and an initial position and velocity. The solution was required to be an unparametrised curve. Hermann had already given a solution, but Bernoulli correctly faulted it for being insufficiently general (specific to the inverse square law) and gave a more general theory of his own. All this was by way of a warm-up for the real confrontation, which was to be between Bernoulli and Newton over exactly this question.

Newton, in the first edition of his *Principia*, had contented himself with the mere assertion that when the force is inverse square the trajectory will be a conic with the centre of force at a focus. He had already been criticised for this, and in October 1709 he instructed Roger Cotes to add a comment to the second edition, which they were then preparing, that claimed firstly that there was always a conic meeting these conditions and which had the correct curvature at the initial point, and that there is only one such curve – so the solution is indeed the conic section.

This defence became the source of much debate between the Newtonians and Bernoulli and his supporters, to the point where it was alleged that since Newton did not use the calculus in this discussion he could not even have possessed it in 1687. There is no need to deal with such an absurd claim, but it is evidence of the high stakes being played for here. Of greater interest is the fact that Newton had already discussed this problem for central forces of any kind elsewhere in the first edition of the *Principia*, and Bernoulli drew on this passage in writing his public letter of 1710. Newton's solution can only be understood if certain quadratures (integrals) are known in advance. What is at stake is the extent to which Newton was able to turn such problems into his calculus in 1687 or 1710 and solve them there.

Guicciardini argues that one way of reading the debate between Newton and Bernoulli is to see it as between a physicist concerned to keep explanatory terms in the discussion – to stay as close as possible to the physics – and a mathematician concerned to solve problems in a systematic and general way. Newton, of course, advocated that topics in science be treated by mathematical reasoning – he said so explicitly of colour in his *Optical papers* and did so at length in his *Principia*. But he did say to Gregory that he did not think algebra should be visible in the final account of a physical problem. As a matter of fact, Bernoulli probably thought Newton's inverse square law required further elucidation if it was to make sense as a piece of physics – how did the mysterious force actually work – but it made for good questions in mathematics, questions that he and the Leibnizian calculus could answer.

3 Euler

The first major work by Euler is his *Mechanica* (published 1736), a substantial work which indicates that Euler had already formed a project of studying mechanics at many levels: point masses, solid bodies, elastic and flexible bodies, bodies of non-constant volume, and fluids. At this stage Euler could only deal with point masses and their motion under forces, their motion in resisting media, (vol. 1) and their motion on surfaces (vol. 2). In more overtly programmatic later works, such as his Reflections on space and time (E149), Euler remarked that while the principles of mechanics have become incontestable, metaphysicians have criticised the subject for relying on notions of

space and time that are imaginary and have no reality. Euler therefore looked for real ideas (his term) from which these imaginary ones could be drawn by a process of abstraction.

The space occupied by a body is the real idea from which the idea of space is abstracted. From the real idea of a body being at rest, always in the same position, Euler was led to consider a relational theory of space. This he rejected by showing that it was incompatible with the (Newtonian) principles of mechanics, specifically that a body remains at rest or in a state of uniform motion unless forced to change. A relational theory, he argued, was forced to say that a body acquired motion from the impact of other bodies from some internal mechanism, and not because it is forced to move. Euler therefore adopted an absolutist position about space and time. He then attempted to derive mechanics from three fundamental properties of bodies: position, impenetrability, and inertia, this last being an ad hoc assumption needed to yield Newton's laws of motion. Euler remained throughout his life hostile to the idea of force as a primitive notion, as his *Research on the origin of forces* (E181) shows, and always sought ways to reduce it to contact forces, mediated quantitatively by the inertia of the bodies (or, perhaps, of their smallest parts).

Euler, then, maintained an absolutist rather than a relativist position about the concept of space: that is, he thought it made sense to talk about space as having properties, and mean by that more than what can be said about the relative positions of objects in space. But he did not like the concept of force.

4 Poincaré's geometric conventionalism

At some stage in the 19th century, the idea of force became accepted, until Hertz and Mach in different ways sought to banish it once again as being hopelessly obscure. Another, whose positions were so close to Mach's on many issues was Henri Poincaré. Indeed, so close were their positions that when Mach's essays were published in a French translation several were omitted as being more-or-less the same. What is involved is what is called Poincaré's geometric conventionalism.

The relation of mathematics to physics was a theme that Poincaré was to return to many times in his writings. In 1897, Poincaré gave a general address to the first ICM in Zurich [Minkowski was to find it bland and unexceptional]. Mathematics, he claimed, has three uses: it aids in the understanding of nature, it helps the philosopher make precise notions of number, space and time; and it has an aesthetic purpose. Indeed, he said, this aesthetic element is the very means by which mathematics and physics advance inseparably together.

He dismissed out of hand the rationalist position that pure reflection will yield a law of nature; all attempts in that direction have failed. Rather, laws of nature are drawn from experiment and expressed in the language of mathematics. But, there are conceptual incompatibilities here. The results of this or that experiment are particular, whereas a law is general. Experiments are approximate, a law is exact, or at least pretends to be. So a law is a generalisation, but – and here comes the under-determination thesis – every truth can be generalised in infinitely many ways. The only way forward is by means of analogy. For example, Kepler's laws and Newton's agree that a single planet travels in an ellipse. But in Newton's theory it becomes possible to study the orbits of the planets as they perturb each other, even though no-one has written down an equation for such

a path, whereas Kepler's laws can only treat the perturbed orbits as some sort of generalisation of an ellipse. Similarly, Maxwell's equations were written down, said Poincaré, because their author wished to present a symmetrical theory, 20 years before there was an experimental justification.

All this should suggest that at the very least of it Poincaré was not a realist. He did not see the task of the theorist to produce richer and fuller accounts of reality. In fact, he openly advocated that the theorist's task was to espouse the plurality of possible theories. The interesting question to explore here is the relation of theoretical pluralism and Poincaré's conventionalism. Conventionalism was first set forth by Poincaré in 1891, and it applied to the way geometry was to be understood in a physical setting. By the late 1890s, there was much public discussion about the nature of space: was it Euclidean, or could it be non-Euclidean? Poincaré's surprising answer was that while non-Euclidean geometry made sense, there was no way of telling if Space was Euclidean or non-Euclidean.

Any experiment would involve an interpretation. One could always say that light rays (or whatever played the role of straight lines in the experiment) were indeed straight, and so space was non-Euclidean (if that is what the measurements seemed to indicate). Or, one could say that Space was Euclidean, and that rays of light were curved.

There was no possibility, however, of deciding between these alternatives on logical grounds. So the only way forward was an arbitrary choice based on human convenience. There would be a collective agreement or convention, but nothing was forced. [Naturally, Poincaré expected the conventional choice to be that Space was Euclidean - but not because it was a vector space but because, in his opinion, Euclidean geometry was more-or-less hard wired into our brains, as we shall see in a minute!]

I've called Poincaré's conventionalism geometric conventionalism, to distinguish it from the versions of conventionalism that became popular later in Vienna among the Vienna circle. Elsewhere in *Science et Hypothèse*, Poincaré distinguished several types of hypothesis. Some hypotheses are natural and necessary (that the influence of distant bodies can be ignored on such-and-such an occasion). Others he called indifferent (the same conclusion is reached on either assumption, for example, that matter is continuous and that matter is discrete). Others are real generalisations, to be confirmed or refuted by experiment. In optics and electrodynamics there are such non-conventional hypotheses.

Poincaré insisted on two aspects of mathematical physics, and excluded others. One pole was experimental results. Once these are securely established, they have to be explained. The other was the mathematics. It had to be rigorous. In a major paper [1890] in which he observed that approximate methods and hints from Nature are sometimes all that is available to the researcher, Poincaré nonetheless rejected the idea that one should settle for this. It was not pedantic to seek the rigorous solution of equations, even when they had only been established by approximate methods and rested on imprecise experimental foundations. For one could not be sure that a less than rigorous proof was not actually flawed; no one the right to say that something inadequate for mathematics was yet good enough for physics. In between these two poles lay a wide area of ambiguity. There was likely to be an infinitude of mathematics, very likely tied to a wide range of physical 'models' (Poincaré did not use the word) and all of these Poincaré held at arm's length. It was a rare event when Poincaré would grant that a hitherto hypothetical object actually existed.

For example, in a striking passage (*Science et Hypothèse*, p. 161-2) in which Poincaré compared the theories of Fresnel and Maxwell, Poincaré wrote:

The differential equations are always true, they may be always integrated by the same methods, and the results of this integration still preserve their value. . . . these equations express relations, and if the equations remain true, it is because the relations preserve their reality. They teach us now, as they did then, that there is such and such a relation between this thing and that; only, the something which we then called motion, we now call electric current.

But these are merely names of the images we substituted for the real objects which Nature will hide for ever from our eyes. [cf Kant] The true relations between these real objects are the only reality we can attain, and the sole condition is that the same relations shall exist between these objects as between the images we are forced to put in their place [cf Hertz's Bilder]. If the relations are known to us, what does it matter if we think it convenient to replace one image by another?

That a given periodic phenomenon (an electric oscillation, for instance) is really due to the vibration of a given atom, which, behaving like a pendulum, is really displaced in this manner or that, all this is neither certain nor essential. But that there is between the electric oscillation, the movement of the pendulum, and all periodic phenomena an intimate relationship which corresponds to a profound reality; that this relationship, this similarity, or rather this parallelism, is continued in the details; that it is a consequence of more general principles such as that of the conservation of energy, and that of least action; this we may affirm; this is the truth which will ever remain the same in whatever garb we may see fit to clothe it.

Why then was geometry different? It acquired this status because of its place in Poincaré's theory of knowledge. Like many of his contemporaries, in a line extending back to Gauss, Poincaré distinguished between arithmetic and geometry on the grounds that arithmetic was certain, but geometry was not. On arithmetic, Poincaré was at his most (and most explicitly) Kantian: there was a synthetic a priori quality to our knowledge of the principle of induction. Poincaré was quite scathing about the attempts to place Couturat-style mathematical logic underneath the natural numbers, or Zermelo's set theory. Our knowledge of geometry was different. It was acquired by our experience as infants making sense of visual, tactile, and motor spaces. The crucial element here was the motion of rigid bodies, starting with our own, and with an ability to distinguish our motion from the motion of things around us. This experience has also become part of the experience of the species.

For Poincaré, geometry was not just a matter of the truth of this or that observation, or this or that theorem, it was connected with how we can have knowledge of the external world at all.

4.0.1 Le Roy and Duhem

Poincaré's conventionalism was not the only kind in town, and he found he had to sharpen it around 1900 when he was outflanked by a curious alliance of French neo-Kantians and Catholics, including a bastion of the Catholic Modernist movement and a formidable conservative.

The challenge he faced revived an old issue Poincaré might well have thought was thoroughly resolved: the rotation of the Earth. It was raised by Edouard Le Roy in an essay of 1899 called ‘Science et philosophie’ and again in the paper he presented to the International Congress of Philosophers in Paris in 1900. Le Roy was then teaching mathematics in a Parisian Lycée, which he did for many years until he succeeded his friend Henri Bergson as Professor of modern philosophy at the Collège de France in 1921.³ Later, in his book *Dogme et critique* of 1907, he adapted Bergsonian vitalism to a modernist philosophy of Catholicism, arguing that dogma could be a source of moral values without being either inscrutable or in contradiction to rational knowledge, and for this he was attacked by Pope Pius X in his encyclical of 1907, when the Pope moved to shut down the Catholic Modernist movement.

Le Roy’s vitalism led him to claim that the only true knowledge lies in an authentic and immediate relationship with one’s surroundings, and all theoretical knowledge is a matter of invention. This is not far from Boutroux’s neo-Kantianism, as he admitted, but the article went further in advocating a radical form of conventionalism, according to which, there are no facts in science, only inventions, which are entirely arbitrary even though they may be necessary on pragmatic grounds. And as examples of scientific ‘facts’ that were in reality only inventions Le Roy cited the atom, the phenomenon of eclipses, and the rotation of the Earth. Furthermore, Le Roy argued that because this rotation was only an invention with no *truth* in it all, not only had the Catholic Church had done nothing wrong in condemning Galileo but Protestant and anti-clerical criticisms of the Church seeking to accuse it of bigotry and hostility to science were profoundly misplaced.

This made it a highly charged matter. Did Poincaré’s conventionalism commit him to such a position? It could well seem so, for he had written in *Science et Hypothèse* (p. 117) that the ‘affirmation: “the Earth turns round,” has no meaning, since it cannot be verified by experiment, [...] or, in other words, these two propositions “the earth turns round,” and “it is more convenient to suppose that the Earth turns round,” have one and the same meaning.’

Poincaré naturally thought he was not thus committed. Where Le Roy had seen a sharp distinction between brute facts and all the scientific facts that are really inventions, in his essays ‘La science est-elle artificielle?’ and ‘La Science et la Réalité’, Poincaré saw a succession of gradations, each with their own claim to compelling assent.⁴ From a state of ignorance one could progress to astronomical predictions, to accepting Newton’s laws, and finally to the deduction of the rotation of the Earth (and a defence of Galileo). The role of convention was restricted to the choice of units of length and time in physics, of definitions and postulates in mathematics. Once those were established, much else followed inevitably, and in particular scientific facts were merely the translation of brute facts into the language of science. Newton’s laws follow from some simple assumptions, or may indeed be taken as assumptions and their consequences tested – that, for Poincaré, is all a matter of facts. The rotation of the Earth, however, he allowed (p. 231) should not be spoken of as a simple fact, because it not comparable to the statement that at such-and-such a time the sky will darken and an eclipse occur.

After a lengthy discussion of the nature of scientific laws, Poincaré then addressed the question

³See <http://www.rep.routledge.com/article/Q057?ssid=187996079&n=7#> Howard, Don (1998). Le Roy, Édouard Louis Emmanuel Julien. In E. Craig (Ed.), *Routledge Encyclopedia of Philosophy*. London: Routledge. <http://www.rep.routledge.com/article/Q057>.

⁴In (Poincaré 1905b, *La Valeur de la Science* 213–247 and 248–276.

of the objectivity of science, which he grounded in communication. Pure sensations (*my* experience of this patch of red, etc.) cannot be communicated, but relations can and what is objective is what is the same for all of us, once the translation between different conventions is allowed for. Science is a classification of apparently different appearances, so it is a system of relations (p. 266). Therefore to say that science cannot be objective because it can speak only of relations and never of things ‘in themselves’ or ‘as they really are’ is to have the problem entirely backwards. Science does not reveal the true nature of things, but that is because nothing can. Indeed, said Poincaré (p. 267), if some god did know the true state of things not only could he not find the words to express them but we could not understand them if he did. Indeed, it can seem that a theory is born one day, fashionable the next, classical the day after, then antiquated and on the fifth day forgotten; ruins accumulate on ruins. But a closer look shows that it is the theories that presume to tell us what reality is that perish, and more relational theories persist (p. 268).

Now Poincaré could return to the claim that the Earth rotates. It was a misinterpretation of his earlier work, he said, to say that it put the rotation of the Earth on the same footing as the parallel postulate (p. 272). Rather, it belonged with claims about the existence of the external world. And indeed, the two claims, that the Earth rotates and that it does not, cannot be told apart kinematically because there is no absolute space. But the claim of rotation is attached to a much richer dynamical theory, which covers the apparent motion of the stars, Foucault’s pendulum, and much else that would be disparate phenomena on a Ptolemaic theory. So the distinction, as Poincaré saw it, between brute facts and scientific facts was merely that scientific facts were brute facts translated into the language of science by being incorporated in a theory. The choice of theory was arbitrary, in much the way that one may speak French or German, the facts were inter-translatable.

This did not impress Le Roy’s ally, the staunch Catholic and French nationalist Pierre Duhem. He had graduated in mathematics from the Sorbonne in 1888 with a thesis on the theory of magnetization by induction, and became a prolific author of articles on various topics not only in mathematical physics, but also on the history and philosophy of physics.⁵ However, his conservative Catholic views as well as technical disputes about thermodynamics had not endeared him to the influential anti-clerical Berthelot, and Duhem was exiled to the provinces, where he made a successful career for himself at the University of Bordeaux. Between 1894 and 1906 he wrote a number of articles on the philosophy of physics, many in the neo-Thomist journals the *Revue de philosophie* and the *Revue des questions scientifiques*, the organ of the Société scientifique de Bruxelles, which had obeyed Pope Leo XIII’s instructions to espouse Thomism in 1890.⁶ This work culminated in his book *La théorie physique. Son objet et sa structure*, 1908, which had a considerable and lasting impact on the field.

Duhem was scornful of Poincaré’s idea about scientific languages (p. 149):

It is therefore clear that the language in which a physicist expresses the results of his experiments is not a technical language similar to that employed in the diverse arts and trades. It resembles a technical language in that the initiated can translate it into facts,

⁵See Howard, Don (1998). Duhem, Pierre Maurice Marie. In E. Craig (Ed.), Routledge Encyclopedia of Philosophy. London: Routledge, at <http://www.rep.routledge.com/article/Q027SECT1>. (Maz’ya and Shaposhnikova 1998) speak of 400 articles and 25 books.

⁶(Paul 1979, 171).

but differs in that a given sentence of a technical language expresses a specific operation performed on very specific objects whereas a sentence in the physicist's language may be translated into facts in an infinity of different ways.

His own philosophy of science emphasised that a network of physical theories is present at every stage in scientific work, and which generally permits different interpretations of any given result. This network rules out any idea of there being a 'crucial experiment' in physics, the outcome of which is decisive for a theory. Rather, the outcome of an experiment is a challenge to the whole network; this is Duhem's contribution to what has become known as Duhem-Quine holism. It implies that refuting a belief such as the non-rotation of the Earth would be a very complicated matter indeed, going well beyond the observation that assuming it fits into a broad general theory. It goes much further than Poincaré's mixture of geometric conventionalism and principles in regarding much more of the web of theory and experiment as in principle arbitrary. However, Duhem located the ambiguities and rival interpretations entirely within the physics. Duhem never acknowledged the modernist shift except to say, as he does of his friend Hadamard's work and that of Poincaré on celestial mechanics that it is useless to the physicist.⁷

His remarks about mathematics were entirely conservative. As befitted both its author's education and his nationalist sympathies, the book has a vigorous defence of French mathematical physics against British mechanical models. The trouble, apparently, is that the English (a term that generously includes Maxwell and Lord Kelvin) wish to reduce physical theory to nothing but models, something 'French or German physicists' would never have done 'of their own free will'. That said, no less a figure than Hertz had already reduced mathematical physics to a collection of algebraic models. This weakness was pandered to by Poincaré, and has spread on a fashion for all things English, especially among the engineering schools, producing piles of faulty reasoning and false calculation, a confusion of science and industry, and the rejection of abstract and deductive theories in favour of the concrete and inductive.⁸

The path to redemption would be, and had been all along, geometric, formal, and even axiomatic. As he put it (p. 107): 'A physical theory will then be a system of logically linked propositions and not an incoherent series of mechanical or algebraic models.' To produce an error-free account of a substantial theory is so difficult that only mathematics (specifically, arithmetic with its extension to algebra) is adequate to the task. 'It owes this perfection to an extremely abbreviated symbolic language in which each idea is represented by an unambiguously defined sign' a language which he said was created in the 16th and 17th centuries. Moreover, it emerges, after a discussion of measurement and of the distinction between quantity and quality, that the traditional and valid aim of a physical theory is to work with measured quantities and measured intensities of qualities, where measurements are simply numbers.

However, it should be noted that Duhem's intention was to show that science was an exercise in classification, and that it was independent of any metaphysics. Scientific laws were not capable of being true because they were only representations, and because they could not be true science was not capable of conflicting with religion. This was not the Thomist position, and Duhem was to be criticised from that quarter, even though Duhem had offered a spirited defence of scholastic philosophers as being much more akin to modern scientists than secularists liked to claim they

⁷(Duhem 1908, 138-143).

⁸(Duhem 1908, 89-93).

were. It was to be the Thomist claim that reason (properly guided) led to truth that was to fare much less well.

5 Conclusion

Does the Earth go round the Sun, or the Sun the Earth? We could try to go and look. The view from a sensibly positioned satellite would be that either the Earth goes round the Sun, or else the Sun and a huge cosmos goes round a very small Earth. Never mind theory, the evidence looks entirely one way. That's one of the changes from 1600: we no longer believe the lamps of Heaven are placed on a shell a few million miles away (Ptolemy's estimate of 50 million was the usual one). But kinematically there is no way to choose one over the other: the distinctive difference is in the dynamics. The theory of dynamics needed to support the heliocentric hypothesis embraces many diverse phenomena, it is predictive; the alternative is so awful no-one seriously contemplates it. When Poincaré placed the reality of the rotation of the Earth on a par with the existence of the external world, he surely had in mind this aspect of the comparison: you can believe the world is some kind of an illusion, but either you still somehow subscribe to all the beliefs about that most people do, or you are so strange as to be beyond if not his intellectual reach then at least his willingness to reach.

So, let us agree that the Earth moves. It really does, it is true to say it does. What have we just done? We have accepted a theory: a system of dynamics that explains the relationship between bodies in motion. We may have accepted some or all of the ingredients of the theory and the way they fit together, such as forces. We may have accepted a philosophy of science – and even a philosophy – that speaks of truth. In different ways Poincaré's geometric conventionalism and Duhemian holism offer a mixture of experience and inference as one's reason for preferring the claim that the Earth goes round the Sun to the earlier claim that the Sun goes round the Earth. There are various possible positions within the spectrum that opens up. Neither Duhem nor Poincaré, for different reasons, wanted to say that it was true that the Earth goes round the Sun, both had their reasons for agreeing that it was the right thing to say.