

# THE TROUBLE WITH AUTHORITY: THE GALILEO AFFAIR AND ONE OF ITS HISTORIANS\*

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## 1. Introduction

When Maffeo Barberini, the future Pope Urban VIII, interviewed Galileo<sup>1</sup> in 1628, he appealed to the authority of logic: was it not possible, despite the weight of Galileo's arguments to the contrary, that God so moved the spheres that the earth did not in fact move? In his famous letter of 1615 to Paolo Foscarini,<sup>2</sup> St. Robert Cardinal Bellarmine did no less: could Galileo really prove that only the motion of the earth was compatible with the observations? He also appealed to the authority of Scripture, to the authority of the Council of Trent itself appealing to the authority of the united tradition of the Fathers of the Church for the interpretation of Scripture. For those interested in tradition and authority, the Galileo affair should have enough to satisfy: exercises of authority, instruments of torture, and house arrest, but also carefully argued appeals to tradition, to Scripture, and to logic. Public executions and burnings at the stake are the only obvious omissions.

It has always been a sensitive test of the commitments of those who treated it, and its sensitivity is particularly visible in its continual use by those wishing to attack ecclesiastical authority. Though from one point of view the Galileo affair could be regarded as the story of how an arrogant man accustomed to humiliate his adversaries got his just deserts, it has more typically been regarded as an instance of the tyrannous exercise of illegitimate authority obstructing the progress of truth. In his *Areopagitica*,<sup>3</sup> John Milton, for example, took the latter view when he recalled visiting Galileo, 'a prisoner of the Inquisition for teaching otherwise than the Popes thought', and in this he was in due course followed by the writers of

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anti-religious tracts in the 19th century and after. For J. W. Draper<sup>4</sup> and A. D. White,<sup>5</sup> it was a prime case of religious obscurantism obstructing the onward march of truth, and their view of the matter continues to inform much public discussion of religion and science. In scholarly work, however, it has been largely displaced by less confrontational approaches.<sup>6</sup>

## 2. *Historical Sources of the New Historiography*

At least three developments lie behind the more nuanced view that now prevails among scholars, all of them with roots in the 19th century: the rise of detailed critical scholarly work on Galileo; critical discussions by positivists and others of the mutual roles of theory and observation in the growth of science; and historical research on ancient astronomy and on the methodological principles appealed to in the literature surrounding it. In what follows my treatment of all three of these developments will be cursory in the extreme: my prime interest will be in the writer who brought all of these developments together, in the work that has influenced all subsequent scholarly discussion of the affair: the turn of the century French Catholic physicist, philosopher and historian Pierre Duhem<sup>7</sup> who, as it happened, had a strong interest in questions of authority and tradition.

The common factor behind all three developments was 19th century positivism,<sup>8</sup> a movement which, in marked contrast to most of its 20th century heirs, laid considerable emphasis on the history as well as on the philosophy of science: concerned to make empirical scientific knowledge the standard by which all else was to be judged, positivists put much effort into investigating the logic of theory and experiment,<sup>9</sup> believing that the history of science was a guide to its philosophy, they also put much effort into historical scholarship,<sup>10</sup> of which the fruits were increased knowledge of both ancient and modern science; hostile to religious belief they were interested in episodes like the Galileo affair in which religion and science appeared to be in conflict. In all these areas those opposed to positivism could also participate; indeed they had an obvious interest in so doing, and Catholics in particular were encouraged in that direction by the rise of neo-Scholastic thought in the last quarter of the century.<sup>11</sup>

## 3. *The Rise of Galileo Scholarship*

In the late 19th century various political factors, principally the forcible incorporation of the Papal states into a reunited Italy, facilitated research into the Galileo affair, which, in view of recent assertions of papal authority, was in any case a sensitive matter. In his *Copernico* (1876), the Italian anti-clerical politician Domenico Berti<sup>12</sup> published an extensive collection of the documents involved, including the famous letter, referred to above, of Bellarmine to Galileo's ally the Carmelite friar Paolo Foscarini. In his *Galileistudien* (1862), the German Jesuit Hartmann Grisar<sup>13</sup> published an analysis of the actions of the Roman congregations involved.

I think it is fair to say that the information revealed by this research was incomprehensible to most of those who studied it: arguments about the permissible bounds to be put on the interpretation of Scripture were accompanied by elaborate arguments about the extent to which truth could be claimed by the 'mathematician' on the basis of his theories, arguments that depended on precise demarcations between logically independent sciences, demarcations foreign to the thinking of the time. There were two questions: 'What was the source of such distinctions?' and 'What sense can be made of them'. Though in theory these two questions are independent of each other, without an answer to the first of them it is difficult even to give serious consideration to the second. In a world that has long since forgotten what it was like for claims that the earth moved to be at all problematic, elaborate arguments about whether Copernicus was fully entitled to make them can seem futile and too pedantic to be worth considering: they can seem to do no more than betray the bad faith of those who advance them. It was well known that Copernicus' publisher, the Lutheran theologian Andreas Osiander, had suggested that the system he was presenting was no more than an improved set of mathematical hypotheses with no claim to truth,<sup>14</sup> but the learned world had long since followed Kepler<sup>15</sup> in dismissing this move as mere frivolity.

#### 4. Ancient Science and the Saving of the Phenomena

In the late nineteenth century, however, the perceptions of many were changed when it became known that claims of the sort made by Osiander, and like claims by Bellarmine and Pope Urban VIII, had a *tradition* behind them, and a long one at that.<sup>16</sup> The tradition in question, which seemed to go back to Plato himself, regarded astronomy as a mathematical science whose aim was, in the traditional technical phrase, to *save the phenomena* of the movements of the stars, to give a mathematical representation conforming to the observations of their motions. Within that tradition, there had been much debate about whether more was involved, whether such theories could claim truth of any kind, whether an observationally satisfactory theory could claim precedence over the metaphysical theories of Aristotle. Writers in this tradition had pointed out (correctly) that observational success was no guarantee of truth, and, surprising as it may seem to modern ears, what was at stake was the viability of the astronomy, not of Copernicus, but of Ptolemy: arguments in this tradition were used both to support this geocentric theory and to attack it. Its nineteenth century rediscovery, moreover, was the work of people with concerns far removed from that of vindicating the actions of the Roman congregations in the Galileo affair. Henri Martin<sup>17</sup> and Giovanni Schiaparelli<sup>18</sup> were concerned with understanding the fate of the sun-centred system of Aristarchus<sup>19</sup> of Samos in ancient astronomy. It was known that it was abandoned in antiquity, and even possible that it

served as the source of the essential mathematical apparatus of the earth-centred systems that succeeded it.<sup>20</sup> Why? The answer Martin and Schiaparelli gave was that they were mathematicians who accepted the saving of the phenomena as their sole concern, mathematicians who in the face of alternative earth-centred and sun-centred hypotheses unhesitatingly took the former as the most plausible.

##### 5. *A Positivist Historiography of Scientific Method*

But to discover a tradition of this kind is one thing, to assess it critically another. Critical assessment of traditions in science or scientific method inevitably takes place against the background of later traditions in scientific method, in this case those engendered by 19th century positivism. As remarked above, these were mostly oriented towards the assertion of experimental science as the paradigm of all knowledge. With that aim commonly went a distrust of theory, a distrust of anything that went beyond experiment, and a view of theory as no more than an instrument for the prediction of the results of future experiments.<sup>21</sup> The keen logic of the positivist critic of scientific theory had discovered the sheer impossibility of a really rigorous proof, based on nothing but experience, of any theory, no matter how trivial, a point that was elaborated in the mid 1890s in a famous article by the young Pierre Duhem,<sup>22</sup> and further explored by others such as Henri Poincaré<sup>23</sup> and Édouard Le Roy.<sup>24</sup>

It was only a matter of time before a scholar aware of that modern *tradition* exploited it historiographically. It could be used in support of a re-evaluation of ancient astronomy in which the latter emerged as a tradition with positive scientific merits. It could also be suggested that in the ancient saving the phenomena tradition lay its intellectual ancestry. Both moves seem to have been explicitly made about 1900 by Belgian Catholics associated with the Société Scientifique de Bruxelles, with which Duhem was strongly associated during his years at Lille: in a series of papers later issued in book form, the Jesuit Jules Thirion,<sup>25</sup> editor of the main organ of the Société, the *Revue des Questions Scientifiques*, concentrated on the Greek achievement of a mathematical astronomical system adequate to represent the astronomical data then available, while putting on one side the question of the truth of the geocentric hypotheses their systems were based on; in various works, the Ghent mathematician Paul Mansion<sup>26</sup> drew the conclusion that the ancient tradition of a phenomena-saving mathematical astronomy that made no claims to the truth of its hypotheses was vindicated by modern critical discussions of theory and experiment, of the kind due to his friend and contemporary Pierre Duhem. On the way he pointed out the presence of saving the phenomena type arguments in Aquinas, and observed their use by Galileo's critics. Under pressure from Catholic critics<sup>27</sup> because of the alleged sceptical tendencies of his views, Duhem grabbed Mansion's suggestion with both hands:<sup>28</sup> it offered both antiquity and the appearance

of scholastic precedents for his own views on scientific method. In an account inspired by Mansion's analysis, Duhem claimed that Galileo had been rightly caught *in flagrante delicto* of bad logic. In Duhem's opinion he should have contented himself with the legitimate claim that the Copernican theory was better at saving the phenomena observed in the heavens, and eschewed the additional illegitimate claim that it was true. But as Duhem was in due course to realize, the situation was not as simple as Mansion's theory allowed: the mainly scholastic thinkers Mansion pointed to used saving the phenomena arguments to protect their metaphysics by pointing to the insecurity of the experimental 'proofs' of the so-called empirical facts that appeared to conflict with it; 19th century positivists, by contrast, used them to eliminate it, for to them the insecurity of any generalisation beyond experiment meant that such things were simply meaningless and should be dispensed with in favour of positive knowledge, the pure description of the laws of the phenomena.

#### 6. Duhemian Anti-Positivism and the Saving of the Phenomena

Duhem, however, had doubts of another kind. It is a deep historical irony that the positivist distrust of any theory going beyond observation is now best known through the presentations of positivist arguments to be found in his works, for Duhem did not share that distrust, and did not believe in discarding everything that could not be proved by experiment.<sup>29</sup> The man who had dedicated his career to theoretical physics was hardly likely to acquiesce in the elimination of his own subject. Duhem started out from the logical truism that a deductive proof never does more than establish one proposition on the basis of others supposed in advance—a deductive proof that Socrates is mortal depends on the prior propositions that he is a man, and that all men are mortal. From this it followed that any deductive proof whatever required the prover to suppose propositions themselves possibly in need of a like deductive proof, so that Duhem could plausibly argue that to try to restrict physics to what could be proved logically would make even experimental physics impossible, for that would require the prover to prove all these extra propositions that had previously been supposed, and so on in turn to prove the propositions on which these new proofs depended, and there was no way of putting a stop to this proof requirement. In the particular case of experimental proof, Duhem argued that every claim about what had been proved by experiment had to depend on *unproved* observational propositions, propositions whose truth had to be established in quite different ways: deductive logic on its own could never justify them and so was insufficient to give a full account of experimental physics: intuitive elements like common sense and good sense were required to fill in the story. Duhem was not here denying logic—an illogical science was for him an abomination—but merely claiming that on its own it was not

enough — but he did make the further claim that our non-deductive common sense was the only reason we could ever have for requiring the use of logic. Duhem's final view called for a balanced harmonious union of intuitive and logical factors in all rational discourse.

This Duhemian analysis of the ultimate grounds of scientific method has important implications for his understanding of the history of science. The physicist in logical difficulties should not necessarily give up: he might reasonably stick to his guns and hope that by opening an alternative way out of his impasse posterity would vindicate him. If Galileo was in logical trouble with his ecclesiastical opponents, it did not follow that Duhem thought he was in the wrong. In Duhem's view, a more careful analysis of the method of physics showed that even on grounds of logic the Mansion analysis needed qualifying. But in any case Duhem had other reasons, of a quite different order, for rejecting that analysis, reasons to do with his attitude to authority and tradition. These reasons took time to emerge: he never publicly withdrew the Mansion analysis: indeed he republished it in his *Physical Theory*<sup>30</sup> of 1906, but his 1908 *To Save the Phenomena* put it in a new context, giving it thereby a quite different aspect. Closer analysis may indicate what was at issue.

### 7. Motives of Credibility in an Authoritarian Apologetic

Observe what Mansion's suggestion does and does not achieve: it bypasses questions about the legitimacy of such exercises of authority as Galileo encountered, and bypasses likewise the question of the appropriateness of using displays of the instruments of torture to help to decide scientific questions; what it does do is show that, at least as far as concerns the substantive issues, those who used these methods of coercion may have had right on their side. It takes it for granted that if they were right they were entitled to use their authority to enforce what was right. It offers what are technically known as 'motives of credibility'. The strategy is a common one in Catholic apologetic: where 19th century secularists were wont to contrast the authority of the senses with religious authority, contrasting the rationality of the one with the irrationality of the other, Catholic apologists refused to admit the premise, and indeed could hardly have done otherwise. They did not, however, rebut the charge by separating the spheres of faith and reason in the style of much Protestant theology. Instead, they claimed that they were in harmony, that there was no possibility that they could ever be in conflict.<sup>31</sup> In his *Letter to the Grand-Duchess Christina* Galileo<sup>32</sup> confidently appealed to that principle, as did the first Vatican Council of 1870 when it anathematized anyone who denied that the knowledge of God was accessible to reason.<sup>33</sup> This, of course, was the Council that also defined the dogma of Papal infallibility: it was entirely characteristic that both definitions would be the work of the one Council. The harmony of faith and reason puts

the authority of the two on a level because it is rational to accept the authority of reason, and reason proclaims the existence of an Almighty God, so also it is rational to accept the authority of the church that tells us more about that God. It is all a matter of *rational obedience*: the authority of reason and the authority of tradition are harnessed together into a system of ecclesiastical authority demanding obedience.

#### 8. Instant Rationality and Instant Obedience

Duhem's whole orientation was deeply subversive of this position. He did not doubt that he ought to respect reason, and the respect for tradition was a centrepiece of his intellectual orientation. He did not even question the need to respect ecclesiastical authority or doubt his duty of obedience. But he had considerable reservations about the manner in which the Roman authorities of his time used their authority to enforce that obedience, rejecting outright the claim made by them and their secular allies that obedience should be implicit, that is instant and automatic. Imre Lakatos<sup>34</sup> once rejected what he called 'instant rationality', criteria that could tell you at once what was the rational thing to do: a like rejection was fundamental to Duhem's<sup>35</sup> position and that rejection was coupled with a similar rejection of 'instant authority'. By design and intention, his writings on the philosophy of science were a protest against the idea that there were explicit criteria for deciding scientific questions: designing experiments, applying logic, regarding hypotheses as refuted, devising new hypotheses, all these things were matters of judgement for which there were no explicit criteria and never could be. Under the influence of his lifelong friend Maurice Blondel, he carried the same attitude over into questions of authority and its exercise.<sup>36</sup> All that remained of the dominant Roman position was the coupling of rationality and authority in a single system. Obedience to authority, like obedience to reason, was a matter of judgement alone. In the rest of this paper I attempt to expand on this point.

Consider his treatment of the Galileo affair in 1908. Though it comes with much more scholarly detail, his discussion of the logic of the situation is essentially that of Mansion's paper of 1899, but in addition to numberless shifts of presentation, there is now a conclusion<sup>37</sup> that attempts to explain what Duhem sees as a paradox: Bellarmine, Osiander and Urban VIII got their logic right but contributed nothing to the rise of modern science, whereas Copernicus, Kepler and Galileo, who got their logic wrong, are the ones now remembered in the history of science. Duhem's answer is that there were more important things than logic: the heroes of science had got a vision of a new science. For all their logical appearance, the old arguments about saving the phenomena had in fact been tied to a cosmology in which earth and heaven had been kept firmly separated, with a different physics for each, whereas Copernicus, Galileo and Kepler, each in their own way, had glimpsed a vision of a new physics in which earth and heaven would be on

the same footing. They had glimpsed it, not seen it, for its achievement was in the future, with Newton's *Principia*, and their vision was imperfect—it is ever thus with new ideas—but it was in pursuit of that ideal that they stretched their logic beyond its limits. Duhem, it is clear, applauds: scientific decisions for him are matters of judgement not of pure logic.

Among Duhem's least known writings are those he published by way of moral and intellectual support for the French war effort in 1915–16: the book *La Science Allemande* (1915), and the article 'Science allemande et vertus allemandes'<sup>38</sup> (1916). Both of them accuse the Germans of a kind of irrationality brought on by excessive confidence in deductive logic. It is in the latter work that the link is made to questions of authority and obedience to authority. Duhem endorses the traditional prejudice that the principal German virtue is implicit obedience to orders: to the orders given by their logical deductions, even when the result is obvious nonsense; to the orders of their officers even when the orders are cruel and inhumane. In the right circumstances of course, implicit obedience is a virtue. In which circumstances? In a monk obeying the instructions of his rule drawn up by a saint. Duhem does not apply this criterion to the delicate issue of obedience to ecclesiastical authority, but it is possible to speculate. Bellarmine has been canonized. Should Galileo have obeyed him? Bellarmine also appealed to tradition. Was that a reason for Galileo to accept his authority? It would seem not. Pope Pius X has also been canonized. Was that a reason why Duhem, Blondel and their friends should adopt scholastic philosophy on the orders of his encyclical *Pascendi Dominici Gregis*<sup>39</sup> of 1907? It is a matter of record that they did nothing of the kind. On the contrary, Duhem in particular devoted his latter years to undermining that philosophy, exposing the controversies surrounding its birth and expounding the history and scientific merits of the alternative philosophies current in the late Middle Ages.<sup>40</sup> Accepting ecclesiastical authority was for him, like accepting scientific authority, a matter of judgement.

#### 9. Epilogue: the Weakness of Authoritarian Apologetic

Duhem's position here is surely right. Whether they are made on behalf of the Bible, or on behalf of an ecclesiastical hierarchy, claims to authority have probably got to rest ultimately on tradition, whether implicitly or explicitly. But if they are to be any use in support of particular claims to obedience, they have to be cashed out, it has to be possible to point to specific institutions or beliefs that embody the tradition, and it is just at this point that the whole enterprise fails, when the authority is in question. For, because it is in question, the questions cannot be stopped: Has the tradition been correctly interpreted? Is the belief of the tradition really the same as the one now being enforced? Is the institution of the earlier tradition really the same in intention as the one now claiming the allegiance of the faithful? Is the belief or institution essential to the tradition or marginal to it, so that it can be



discarded or modified in the new situation? Is likewise the scientific approach supposedly supported by tradition really essential, or marginal, ready to be abandoned in the search for more complete truth? Observe that tradition here is in no worse case than deductive proof, for, as has been remarked above, if that is used in support of a claim to authority, all that does is to transfer the problem to the premises and principles on which it rests, besides raising awkward questions, resolvable only by judgement, as to whether the deductive formalization correctly reflects our informal intuitions.

In any case, necessary as they may be for the organization of churches in practice, there is something paradoxical about Christians claiming authority in this world. As we remind ourselves every time we use the Magnificat,<sup>41</sup> and as Duhem was most certainly aware, the God we worship is One who pulls down the mighty from their thrones and elevates those who are weak. From the point of view of much of the Old and New Testaments, the empires and kingdoms of this world are there for a season until the Lord God chooses to do away with them. To claim any kind of authority in this world is to claim a worldly empire,<sup>42</sup> such as God will in due course surely sweep away, and Peter's confession at Caesarea Philippi<sup>43</sup> is surely a weak basis for supposing the Christian ecclesiastical authority is any exception to the general rule. In this area as in all others, deciding when ecclesiastical authority is due for replacement must be a matter of judgement on the part of the faithful who pray to their Lord continually day and night. Authority and tradition have to win their own consent if they are to subsist at all.

#### NOTES

- \* This paper was presented at the March 1988 Liverpool annual conference (on the theme 'Tradition and Authority') of the Science and Religious Forum. I thank Mrs. Mary Midgley and others present at the conference for their critical comments.
- 1 A. Oreggi, *De Deo Uno* (Rome, 1629, 1630), p. 294 of 1630 edition.
- 2 G. Galilei, *Opere*, ed. naz., XII, pp. 171–2. Translated in G. Galilei, *Discoveries and Opinions of Galileo*, Translated with an Introduction and Notes by Stillman Drake (New York, 1957), pp. 162–4.
- 3 J. Milton, *Areopagitica, a speech of Mr. John Milton for the liberty of unlicenc'd printing, to the Parliament of England* (London, 1644).
- 4 J. W. Draper, *History of the Conflict between Religion and Science* (London, 1875), pp. 142–3.
- 5 A. D. White, *A History of the Warfare of Science with Theology in Christendom* (London, 1896), pp. 130ff. Compare J. R. Moore's remarks in Part I of his *The Post Darwinian Controversies* (Cambridge, 1979).
- 6 See e.g. H. Butterfield, *The Origins of Modern Science 1300–1800* (London, 1949, 1957); A. R. Hall, *The Scientific Revolution 1500–1800* (London, 1954, 1962). W. A. Wallace, *Galileo and his Sources, the heritage of the Collegio Romano in Galileo's science* (Princeton, 1984), and Beth McClumpha, *Some Aspects of the Intellectual Relations between Galileo and the Jesuits* (Unpublished PhD thesis, Leeds, 1984) examine in detail the possible Jesuit sources for much of Galileo's scientific thinking. See also Moore *op. cit.* (Note 5).

- 7 For a detailed treatment of Duhem's life and career, see S. L. Jaki, *Uneasy Genius, the Life and Work of Pierre Duhem* (The Hague, 1984). A very good critical account of Duhem's development in the first part of his career is Roberto Maiocchi's *Chimica e Filosofia, scienza e epistemologia, storia e religione nell'opera di Pierre Duhem* (Firenze, 1985).
- 8 On positivism see besides Chapter 1 of Maiocchi *op. cit.* (Note 7), Henri Gouhier, *La Jeunesse d'Auguste Comte et la formation du positivisme*, 3 vol, (Paris 1933–41) and vols 2 and 3 (Paris 1964–70); F. A. Hayek *The Counter-Revolution of Science, studies in the abuse of reason* (Glencoe, Illinois, 1952); W. M. Simon, *European Positivism in the Nineteenth Century, an essay in intellectual history* (Ithaca, NY, 1963).
- 9 For an extensive bibliography of writings on scientific method see L. Laudan, 'Theories of Scientific Method from Plato to Mach', *History of Science* 7 (1969) 1–63.
- 10 For a brief general discussion of French positivism and the history of science, see Maiocchi *op. cit.* (Note 7) pp. 239–43 and literature cited. Well known are the writings of P. E. M. Berthelot on the history of chemistry, of Ernst Mach on the history of mechanics and heat theory, and later those of Henri Berr and George Sarton and the journals these two founded. The Catholic Paul Tannery always claimed that his original inspiration to engage in history of science was positivist — see H. W. Paul 'Scholarship versus Ideology, the chair of the general history of science at the Collège de France, 1892–1913', *Isis* 67 (1976) 376–97. See also Duhem's remarks in Congrès Scientifique International des Catholiques, *Compte Rendu du troisième Congrès* (Brussels, 1895), vol 1 pp. 313–5 and 322–5.
- 11 On neo-scholasticism see G. A. McCool, *Catholic Theology in the nineteenth Century, the search for a unitary method* (New York, 1972) and Pierre Thibault, *Savoir et Pouvoir, philosophie Thomiste et politique cléricale au XIX<sup>e</sup> siècle* (Québec, 1972); See also H. W. Paul, *The Second Ralliement. the rapprochement between church and state in the twentieth century* (Washington DC, 1967) and *The Edge of Contingency, French Catholic reaction to scientific change from Darwin to Duhem* (Gainesville, Florida, 1979) pp. 179–94, as well as my essay review of the latter: R. N. D. Martin, 'Darwin and Duhem', *History of Science*, 20 (1982) 64–74.
- 12 Domenico Berti, *Copernico e le Vicende del Sistema Copernicano in Italia nella seconda metà del secolo XVI e nella prima del XVII . . .* (Roma 1876).
- 13 H. Grisar, *Galileistudien, historisch-theologische Untersuchungen über die Urtheile der Römischen Congregationen im Galileiprozess* (Regensburg 1882).
- 14 N. Copernicus, *De Revolutionibus Orbium Coelestium* (Nürnberg, 1543) and any other edition. Remarks on Osiander's preface can be found in any work on the 16th–17th century astronomical revolution. See e.g. A. Koestler, *The Sleepwalkers, a history of Man's changing vision of the universe* (London, 1959, 1964) pp. 169–175. Translation in E. Rosen, *Three Copernican Treatises*. (New York 1939), pp. 24ff.
- 15 In his *Astronomia Nova* of 1609. On Kepler see N. Jardine, *The Birth of History and Philosophy of Science. Kepler's 'A Defence of Tycho against Ursus', with essays on its provenance and significance* (Cambridge, 1984).
- 16 The classic brief account of this tradition is Pierre Duhem's *ΕΠΙΣΤΗΜΗ ΤΑ ΦΑΙΝΟΜΕΝΑ, Essai sur la notion de théorie physique de Platon à Galilée* (Paris, 1908, 1982) reprinted from *Annales de Philosophie Chrétienne* (April–September 1908), available in the English translation of E. Dolan and C. Maschler as *To Save the Phenomena, Essay on the idea of physical theory from Plato to Galileo* (Chicago, 1969). For critical discussion of this work see Jardine *op. cit.* (Note 15) and R. N. D. Martin, 'Saving Duhem and Galileo, Duhemian methodology and the saving of the phenomena' *History of Science*, 25 (1987) 301–19. The very condensed presentation of this work should be compared with that of Duhem's later *Le Système du Monde, histoire des doctrines cosmologiques de Platon à Copernic*, 10 vols, (Paris, 1913–59), especially parts I and II, vols 1–4).
- 17 T. H. Martin, *Memoires sur l'histoire des hypothèses astronomiques chez les Grecs et chez les Romains, Première Partie: 'Hypothèses astronomiques des Grecs avant l'époque Alexandrine'*, Ch. 5, §4 (*Mémoires de l'Académie des Inscriptions et Belles Lettres* 30, pt. 2.)
- 18 G. V. Schiaparelli, *Origini del sistema planetaria eliocentrico presso i Greci*, Ch. VI e appendice *Memorie del Instituto Lombardo di Scienze e Lettere, Classe di Scienze matematiche i naturali*, 18 [série 3, vol 9], (17 March 1896).
- 19 See T. L. Heath's classic *Aristarchus of Samos, the ancient Copernicus . . .* (Oxford, 1913), and *Dictionary of Scientific Biography* (*qv.*).
- 20 J. Thirion, *L'Évolution de l'astronomie chez les Grecs* (Paris, 1901), pp. 109–37, reprinted from 'Pour l'astronomie grecque', *Revue des Questions Scientifiques*, 45 (1899) 5–47, 435–75, and 46 (1899) 110–58.

- 21 On this aspect of positivism see Maiocchi *op. cit.* (Note 7), Chapter 1.
- 22 P. Duhem, 'Quelques Réflexions sur la Physique Expérimentale', *Revue des Questions Scientifiques*, 36 (1894) 179–229. The final form of the views set out in this article is Duhem's well known *La Théorie Physique, son Objet et sa structure* (Paris, 1906, 1914, 1981), cited below as Part/ Chapter/Section/Page of 1914 edition (The pagination of the 1981 edition follows that of 1914.), translated by P. P. Wiener as *The Aim and Structure of Physical Theory* (New York, 1954).
- 23 J. H. Poincaré, *La Science et l'Hypothèse* (Paris 1902), and later works. (NB. The relevant chapters are all later than Duhem's article: See Duhem's note *Théorie Physique op. cit.* (Note 22) II/IV/II/217–8.)
- 24 É. Le Roy, 'Science et Philosophie', *Revue de Métaphysique et de Morale*, 7 (1899) 375–425, 503–62; 8 (1900) 37–72.
- 25 J. Thirion *op. cit.* (Note 20).
- 26 P. Mansion, 'Note sur le caractère géométrique de l'ancienne astronomie', *Abhandlungen zur Geschichte der Mathematik*, 9 (1899) 277–92. A book of his, *Sur les Principes fondamentaux de la Géométrie, de la Mécanique et de l'astronomie*, is cited by Duhem in 1893 with that date, and in 1905 in his *Théorie Physique* as 1903, published by Gauthier-Villars, Paris. However, I have not been able to locate a copy and the Bibliothèque Nationale does not possess one. R. Maiocchi reports (personal communication) a brief sight of a copy lacking a title page but dated 1893. I conjecture that the work reached proof copy stage but was never actually published. The fact that Duhem did not cite it again in his 1908 *ΕΠΙΣΤΗΜΗ ΚΑΙ ΦΑΙΝΟΜΕΝΑ op. cit.* (Note 16) is consistent with this conjecture.
- 27 See e.g. E. Vicaire, 'De la Valeur Objective des Hypothèses Physiques', *Revue des Questions Scientifiques*, 33 (1893) 451–510. This and other critiques are discussed in the final chapter of H. W. Paul, *The Edge of Contingency loc. cit.* (Note 11).
- 28 P. Duhem, 'Physique et Métaphysique', *Revue des Questions Scientifiques*, 34 (1893) 35ff.
- 29 See *Théorie Physique, op. cit.* (Note 22) Part I Chapters II and IV, *passim* and Maiocchi *op. cit.* (Note 7) *passim* and my 'Saving Duhem' *op. cit.* (Note 16).
- 30 *Théorie Physique op. cit.* (Note 22) I/III/II/54–60.
- 31 Both the *Annales* and the *Revue des Questions Scientifiques* of the Société Scientifique de Bruxelles carried on their title pages the Latin motto from the Constitution on the Catholic Faith of the First Vatican Council: 'nulla umquam inter fidem et rationem vera dissensio potest' (there can never be a real disagreement between faith and reason). It is interesting to note that in 1877, the year of its founding, the first issue (January) of the latter carried an article by one de Smedt 'L'Église et la Science', and the second (April) carried an article by one Philippe Gilbert on 'La Condamnation de Galilée et les publications récentes'. On this journal see M. J. Nye, 'The Moral Freedom of Man and the Determinism of Nature, the Catholic synthesis of science and history in the *Revue des Questions Scientifiques*', *British Journal for the History of Science*, 9 (1976) 274–92.
- 32 *Opere ed. naz. op. cit.* (Note 2) V, pp. 310–48. *Discoveries and Opinions op. cit.* (Note 2) pp. 175–216.
- 33 McCool *Catholic Theology, op. cit.* (Note 11) has a detailed discussion of this aspect of the Council's work.
- 34 See Section 3d of I. Lakatos 'Falsification and the Methodology of Scientific Research Programmes' in Imre Lakatos and Alan Musgrave eds., *Criticism and the Growth of Knowledge* (Cambridge 1970), pp. 154ff., and Imre Lakatos, *The Methodology of Scientific Research Programmes*, Philosophical Papers Vol. 1 (Cambridge 1978) pp. 68ff.
- 35 See Duhem, *Théorie Physique, op. cit.* (Note 22) II/VI/VI/304ff, II/VI/X/329ff, II/VII/I–VI/333–411; and the letter to a school friend quoted by E. Picard in his *La Vie et l'Œuvre de Pierre Duhem* (Paris 1922), pp. 50, 52.
- 36 A protest against demands for mechanical obedience in both political and ecclesiastical spheres was the central issue in Blondel's campaign against Action Française that dominated the final years of the venerable *Annales de Philosophie Chrétienne* before it was put on the *Index* and suppressed in 1913. See Testis (M. Blondel), 'La Semaine Sociale de Bordeaux, controverses sur les méthodes et les doctrines', *Annales*, 159 (1909–10) and 160 (1910) and *Index Librorum Prohibitorum* (1948 edition).
- 37 *ΕΠΙΣΤΗΜΗ ΚΑΙ ΦΑΙΝΟΜΕΝΑ op. cit.* (Note 16) pp. 136–40.
- 38 P. Duhem, *La Science Allemande* (Paris 1915); P. Duhem, 'Science allemande et vertus allemandes' in *Les Allemands et la Science* edited by Gabriel Petit and Maurice Leudet (Paris, 1916).

- 39 On this Encyclical see A. R. Vidler, *A Variety of Catholic Modernists* (Cambridge 1970), pp. 15–18; Gabriel Daly, *Transcendence and Immanence* (Oxford, 1970), pp. 199–217 and 232–4. For general background on the so-called modernist crisis of Roman Catholicism, see besides these the works of Émile Poulat, such as his *Histoire, Dogme et Critique dans la Crise Modernists* (Paris and Tournai 1962, 2nd ed. 1979). For a fuller discussion of it in relation to Duhem see my 'Darwin and Duhem' *op. cit.* (Note 11).
- 40 Principally in his *Système du Monde*, *op. cit.* (Note 16).
- 41 Luke 1. 46–55.
- 42 Matthew 4. 5–7 and Luke 4. 5–8.
- 43 See e.g. Matthew 16. 13–19, Mark 8. 27–30, Luke 9. 18–21. Consider also John 18. 36 'My Kingdom is not of this world'.