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### The genesis of a mediaeval historian: Pierre Duhem and the origins of statics

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## The Genesis of a Mediaeval Historian: Pierre Duhem and the Origins of Statics

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### Summary

Contrary to what might be expected given a religious or other motivation, Pierre Duhem's interest in mediaeval science was the result of his surprise encounter with Jordanus de Nemore while working on *Les origines de la statique* in the late autumn of 1903. Historical assumptions common among physicists at that time may explain this surprise, which occasioned a frantic search for more mediaeval precursors for Renaissance mechanics. It also raised serious historiographical problems that threatened even his methodological views, until they were resolved in his *To save the phenomena* of 1908.

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The commentaries of the Scholastics on the *Mechanical questions* of Aristotle added essentially nothing to the ideas of the Stagirite; to see these ideas grow new shoots and yield new fruit, we must await the beginning of the 16th century.

Pierre Duhem, October 1903.<sup>1</sup>

Before coming to the fundamental treatise on statics produced in the Middle Ages by the enigmatic Jordanus de Nemore, we must collect the debris scattered through the manuscripts of the writings composed in Alexandria on the science of equilibra.

Pierre Duhem, April 1904.<sup>2</sup>

### 1. Introduction

The systematic study of the exact sciences in the Middle Ages was begun by Pierre Duhem (1861-1916), who remains to this day the most voluminous contributor to the field.<sup>3</sup> Besides the two volumes of *Les origines de la statique*, the three of the *Etudes sur Léonard de Vinci*, and the incomplete ten of *Le*

<sup>1</sup> *Revue des questions scientifiques*, 54 (1903), 469; *Les origines de la statique*, vol. 1, 13. (More details of these and other references are given in footnote 3 below.) It is not clear what commentaries Duhem has in mind. According to P. L. Rose and S. Drake, 'The pseudo-Aristotelian questions of mechanics in Renaissance culture', 18 (1971), 65-104, the work was virtually unknown in the Middle Ages. Duhem may have assumed their existence without checking. I thank Dr. C. B. Schmitt for this reference.

<sup>2</sup> *Revue des questions scientifiques*, 55 (1904) 561; *Les origines de la statique*, vol. 1, 62.

<sup>3</sup> It is not possible in this article to give a full bibliography of even Duhem's historical writings. In lieu I cite the memorial volume of the *Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux*, (7) 1, pt. 2 (1927). Besides a biography by E. Jordan and articles on aspects of his work by O. Manville, J. Hadamard and E. Darbon, it contains a 'Notice sur les titres et travaux scientifiques de Pierre Duhem' prepared by Duhem himself for his 1913 candidacy for non-resident membership of the Académie des Sciences (pp. 71-169), and an extensive bibliography (pp. 41-70, incomplete), based on the list which Duhem prepared on the same occasion, and according to the editor updated to 31 December 1917. I refer to this work hereafter as 'Bordeaux'. See also the article by D. G. Miller in the *Dictionary of scientific biography*, vol. 4 (1971, New York), 225-233.

[Continued on next page.]

*système du monde*, Duhem wrote numerous articles and papers. His criticism of Galileo in his *To save the phenomena* continues to arouse controversy.<sup>4</sup>

Despite much criticism,<sup>5</sup> historians have continued to use his work, and it remains an essential secondary source for workers in the field. But essential secondary sources can be dangerous to use without critical interpretation, without some idea of their author's presuppositions and intentions; and so over the years historians have based their varied interpretations of Duhem's work on their understandings of his varied commitments. There are plenty of these for consideration: his chauvinistic French patriotism; his open and explicit Catholicism; his commitment to the late 19th-century anti-atomistic programme of reducing the whole of physics to the laws of energy transformation (energetics); and the instrumentalist methodology associated with energetics. But, I claim, theories concerning Duhem's work based in any simple way on one or other of these commitments all ignore the development of his career. Explanations based only on his Catholicism fit uncomfortably with the fact that none of his works before 1904 shows any knowledge of mediaeval science; and his interests in physics alone cannot explain the difference in their expression before and after that date.

I claim that mediaeval physics took Duhem completely by surprise in the late autumn of 1903, and that the disruptive consequences of the surprise threatened even his methodology of physics, then being given its final expression in *La théorie physique; son objet et sa structure*. The symptoms of the disruption can be seen in the large-scale hunt for the precursors of early modern mechanics, of which his *Etudes* is the best known expression. The discovery of precursors for Duhem's own methodological *problems* may have enabled him to find his feet in his newly discovered field.

I cite or mention several times below the following works by Duhem:

(1) *L'évolution de la mécanique*, first published in the *Revue générale des sciences pures et appliquées*, 14 (1903), and in book form (1903, Paris). There were also contemporary German and Polish translations. I cite the French book original as '*Mécanique*'.

(2) *La théorie physique; son objet et sa structure*, first published in the *Revue de philosophie*, 1904 and 1905, and in book form (1st ed. 1906, 2nd ed. 1914, Paris). Both are preferable to P. P. Wiener's translation *The aim and structure of physical theory* (1954, New York). I cite the French book version as '*Théorie*'.

(3) *Les origines de la statique*, first published in the *Revue des questions scientifiques*, 54-60 (1903-06) and in book form (2 vols., 1905-06, Paris). I cite the journal as '*RQS*', and the book as '*Origines*'.

(4) *Etudes sur Leonard de Vinci*, mostly first published in the *Bulletin italien*, 5-12 (1905-12), and in book form (3 vols., 1906-13, repr. 1955, Paris).

(5) *Sozein ta phainomena: essai sur la notion de théorie physique de Platon à Galilée*, first published in the *Annales de philosophie chrétienne*, 156 (1908), *passim*, and in book form (1908, Paris). It is preferable to the translation of E. Doland and C. Maschler, *To save the phenomena; an essay on the idea of physical theory from Plato to Galileo* (1969, Chicago and London). I cite the book as '*Phenomena*', and the journal as '*APC*'.

<sup>4</sup> See, for example, M. Clavelin, 'Galilée et le refus de l'équivalence des hypothèses', *Revue d'hist. sci.*, 17 (1964), 305-330 (p. 306ff.); A. C. Crombie, 'Galilée devant les critiques de la postérité', *Conférences du Palais de la Découverte*, ser. D, no. 45 (1964); S. Drake, in his translation of L. Geymonat, *Galileo Galilei* (1965, New York), 239, n. 4; G. de Santillana, *The crime of Galileo* (1952, London), 107; and E. Rosen, *Three Copernican treatises* (3rd ed. 1971, New York), 33.

<sup>5</sup> See, for example, O. Neugebauer, *The exact sciences in antiquity* (2nd ed. 1957, Providence, R.I.), 206; R. Palter, 'An approach to the history of early astronomy', *Stud. hist. phil. sci.*, 1 (1970), 93-133 (p. 130, n. 38); and E. Rosen in T. Hilton (ed.), *The Renaissance: a reconsideration of the theories and interpretations of the age* (1964, Madison, Wis.), 77-103.

My claim rests on the text of the *Origines de la statique* and on relevant circumstantial evidence.<sup>6</sup> To this I now turn.

## 2. The surprise

My mottos above are from chapters 2 and 5 of the *Origines*. After a chapter on Aristotle and Archimedes, Duhem passes to Leonardo da Vinci, the hero of the next three chapters, and justifies his complete omission of anything in between by the doctrine of mediaeval sterility expressed in my first motto. But in chapter 5, in flat contradiction of what he said in chapter 2, he announces a fundamental treatise on statics produced in the Middle Ages by one Jordanus de Nemore, and proposes to interpret it with the aid of a study of the 'Alexandrian sources of mediaeval statics'. His inconsistency implies a change of mind; but because the early chapters were already in print when it happened, he had no opportunity to conceal it by amending his work to restore consistency. The work was published as a serial in the Brussels *Revue des questions scientifiques* before being reprinted verbatim in the independent volumes.

The *Revue* was published in January, April, July and October by the (Catholic) Société Scientifique de Bruxelles. The issue of October 1903 carried chapters 1–4 of the *Origines*, that of April 1904 contained chapter 5, and thereafter publication continued quarterly through to the completion of the work in 1906. The chronology which this sequence establishes has a gap corresponding to the January 1904 issue. It is tempting to connect the gap with the inconsistency and to suppose that he ran into the material that forced his change of mind in the late autumn of 1903, and that the resulting research caused him to miss the copy date for the January 1904 issue.

The inconsistency in the *Origines* has been noticed before. It was discussed in the course of a two-part article on Duhem's work in the history of science published in the *Revue* in 1921, five years after his death.<sup>7</sup> In it Fr. Henri Bosmans comments that each chapter was perfectly clear on its own, but that the whole seemed confused and incoherent. He can throw light on the reason for this inconsistency. In a conversation with the editor Fr. Jules Thirion while the work was in progress, he had asked if he could see the remainder of the manuscript and received the reply: 'I haven't got it yet. Duhem hasn't finished it yet. He says he still has some reading to do. He has promised me the remaining chapters as they are written'.<sup>8</sup>

Bosmans's question assumed that Thirion would already have the complete manuscript; but Duhem was living from hand to mouth and had already found material needing to be followed up before writing the succeeding chapters. He may well have thought that at least the early chapters could be finalized and published. But Bosmans thinks not; he doubts Duhem's view of the dominating importance of Leonardo, and adds: 'Moreover, I know two short treatises *On Weights*, both attributed to Jordan de Nemore. Duhem will eventually come across them, and I shall be surprised if he doesn't attach

<sup>6</sup> This includes three letters written by J. Thirion to Duhem, which add little to the story told below. I thank Professor D. Miller for supplying me with copies; see his (footnote 3).

<sup>7</sup> H. Bosmans, 'Pierre Duhem (1861–1916): notice sur ses travaux relatifs à l'histoire des sciences', *RQS*, 80 (1921), 30–62, 427–448.

<sup>8</sup> *Ibid.*, 40–41.

some importance to them'.<sup>9</sup> As the following chapter (appearing in April 1904) was to show, Bosmans's prediction had been fulfilled already. But Duhem did not leave his readers to divine his change of mind; as Bosmans says of the preface to the independent edition of the *Origines*: 'The reworkings in the initial plan of the work that I had merely suspected, Duhem admitted them, shouted them out loud'.<sup>10</sup>

I now turn to this preface. It opens with a claim to have brought about nothing less than a revolution in the history of statics:

Before undertaking the study of the origins of statics, we read the few writings that treat the history of that science. It was easy to recognize that most of them were very condensed and lacking in detail, but we had no reason to suppose them incorrect, at least in broad outline. Hence, when we turned to the study of the texts they referred to, we anticipated having to add or alter many details, but nothing led us to suspect that the history of statics in its entirety would be upset by our researches.<sup>11</sup>

He had read the secondary sources and found them plausible if inadequate, and had no reason to suspect them fundamentally wrong when he went to the original texts. An initial surprise was the discovery that Leonardo da Vinci had been nothing like as isolated as tradition made out, that his work had been used by such key Renaissance mathematicians as Cardano and Benedetti. But after that he was able to follow the development of statics from Leonardo and Cardano on without finding any essential differences from the commonly accepted account. He continues: 'We had begun to retrace this development in the hospitable pages of the *Revue des questions scientifiques*, when the reading of Tartaglia, whose name no history of statics even mentions, showed us unexpectedly that the work already begun had to be restarted on an entirely new plan'.<sup>12</sup> To attract Duhem's attention, Tartaglia had done what, in Duhem's terms, was good physics: he had obtained an important correct result from an important general principle. But it seemed that the discovery was not his:

Tartaglia, indeed, long before Stevin and Galileo, had determined the apparent weight of a body on an inclined plane; he had derived it entirely correctly from the principle whose complete generality Descartes was later to affirm. But that discovery, mentioned by no historian of mechanics, was not his, in his work it was an impudent piece of plagiarism; Ferrari attacked him fiercely for it and claimed priority for a 13th century mathematician, Jordanus Nemorarius.<sup>13</sup>

So we have a double surprise; first, the non-isolation of Leonardo, and second, Jordanus. To interpret it, we may pose two questions: What was Duhem doing when surprised? What did he take for granted beforehand? The first is essentially that of the significance for him of statics and its history;

<sup>9</sup> *Ibid.* The two treatises are: *Libri Jordani . . . De ponderibus, propositiones XIII . . . per Io. Petreium* (1533 Norimbergae); and *Jordani opusculum de ponderositate, Nicolae Tartaleae studio correctum, . . .* (1565, Apud Curtium Troianum, Benetiis). The second of these could find a place in a study of the mechanics of Tartaglia and naturally lead on to Jordanus.

<sup>10</sup> Bosmans (footnote 7), 41.

<sup>11</sup> *Origines*, vol. 1, i.

<sup>12</sup> *Ibid.*, ii.

<sup>13</sup> *Ibid.* Jordanus has been the subject of much scholarly interest since. The text of the *De ponderibus* will be found with an English translation in E. A. Moody and M. Clagett (eds.), *The mediaeval science of weights* (1952, Madison, Wis.).

the second concerns his sources and particularly presses, because the mechanical interests of both Jordanus and Tartaglia appear to have been known to such historians of mathematics as Montucla and Moritz Cantor.<sup>14</sup>

### 3. The origins of the *Origines*

There is no difficulty in understanding Duhem's concern with the history of physics. Historical works on physics by physicists were, as will be shown in the next section, not unusual at the time. A historical study of the subject was published alongside his doctoral thesis on magnetization by induction in 1888,<sup>15</sup> and further historical studies followed in the next ten years or so. Duhem's main theoretical interests in the theory of heat, in physical chemistry, and in the theory of electricity and magnetism were all reflected in historical works published in the 1890s and early 1900s. In 1913 he claimed that methodology was the point of it all: 'Every abstract thought needs testing by the facts; every scientific theory calls for comparison with experiment; our logical considerations relating to the proper method for physics cannot be sanely judged unless compared with the teachings of history'.<sup>16</sup> But at this earlier stage of his career, general methodology seems to have been subordinate to Duhem's specifically scientific aims: to show two rival traditions in physics, one of which he approves going from success to success, while the other, of which he does not approve, runs into the sands of increasing complication and diminishing returns. Hence, methodology plays an historiographical rôle: to select and interpret the facts, to structure the argument.

Nor is there any difficulty in understanding the importance of statics for Duhem. Of the two parts into which mechanics has traditionally been divided, it represents the theory of the equilibrium of forces, as opposed to dynamics which is the theory of the motions forces produce. Duhem's *L'évolution de la mécanique* tells *inter alia* the story of how J. L. Lagrange built his statics on the principle of virtual displacements and then used D'Alembert's principle to erect on that basis a dynamics.<sup>17</sup> The so-called analytical mechanics of Lagrange was the basis of the physics alike of Pierre Duhem and James Clerk Maxwell, and Duhem saw the principle of virtual displacements as fruitful over wide areas of physics.

Given its importance for him it was only natural that he should investigate its origins. He had raised the question of statics in *L'évolution de la mécanique* that very year (1903) without giving an answer, and we can see this concern in the structure of the *Origines* as it finally emerged. Its final chapter is dominated by the coordination of the laws of statics on the basis of the principle

<sup>14</sup> J. E. Montucla, *Histoire des mathématiques*, vol. 2 (1st ed. 1758, Paris), 417, 621, (2nd ed. 1799, Paris), 506, 691. The index to the first edition is more detailed, though that of the second also cites his mechanics and Tartaglia.

M. Cantor, *Vorlesungen über die Geschichte der Mathematik*, vol. 2 (1st ed. 1892, Leipzig), 55, (2nd ed. 1900, Leipzig), 60, has an unindexed paragraph mentioning the existence of the *De ponderibus*, and a footnote mentions a manuscript examined by Curtze.

<sup>15</sup> P. Duhem, 'Etude historique sur l'aimantation par influence', *Ann. Fac. Sci. Toulouse*, 2 (1888).

<sup>16</sup> Bordeaux, 158.

<sup>17</sup> See *Mécanique*, 43ff.

of virtual displacements, and he must have seen it behind the statement of the principle of moments that so excited him when he wrote:

Not only had the mediaeval West received, either directly or through Arab intermediaries, the tradition of certain Greek theories of the lever and the steelyard, but its own intellectual activity had generated an autonomous statics, unsuspected in Antiquity. By the beginning of the 13th century, perhaps even before that time, Jordanus de Nemore had demonstrated the law of the lever from this postulate: the same force is required to raise different weights when they are in inverse proportion to the heights they traverse.<sup>18</sup>

#### 4. A historiographical programme and its refutation

Now that we have seen the relationship of Duhem's interest in the origins of statics to his work as a theoretical physicist, we are in a position to discuss his secondary sources. I begin with those actually cited in the work. Recalling the chronology of the chapters published in the *Revue*, we can say that Duhem does not use such standard sources as Heiberg, Montucla and Moritz Cantor before the 2nd and 3rd instalments, that is, he does not use them before the first of the two surprises noted in section 2 above. In the first instalment of October 1903, he cites none of them. Instead, there are five references to volume 3 of G. Libri's *Histoire des sciences mathématiques en Italie* (1838–40, Paris), and one each to G. B. Venturi's *Essai sur les ouvrages de Léonard de Vinci*,<sup>19</sup> E. Wohlwill's *Entdeckung des Beharrungsgesetzes*,<sup>20</sup> and Part I, section 1 of J. L. Lagrange's *Mechanique analytique*.<sup>21</sup> A footnote criticizes Libri for careless and anachronistic reading of the texts,<sup>22</sup> and another criticizes Lagrange for anti-Aristotelian prejudice.<sup>23</sup>

In view of the importances, noted above, of Lagrangian mechanics for Duhem's physics, there is no surprise in Duhem's use of Lagrange: but his use of Libri is more problematical. If he had used Libri before starting work, he would have found the mechanical interests of Tartaglia there and then,<sup>24</sup> whereas Lagrange knew only of Guido Ubaldo and Stevin between Archimedes and Galileo, and credited Stevin with the first correct resolution of the inclined plane problem.<sup>25</sup> It seems likely that Duhem only went to Libri for information on the problems raised by his other reading.

A search in the subject-index of the British Museum for works on the history of statics or mechanics that could have been available to Duhem produced only two: Ernst Mach's *Die Mechanik in ihrer Entwicklung historisch-kritisch dargestellt*,<sup>26</sup> and Eugen Dühning's *Kritische Geschichte der allgemeinen*

<sup>18</sup> *Origines*, vol. 1, ii–iii.

<sup>19</sup> *Essai sur les ouvrages physico-mathématiques de Léonard de Vinci avec des fragments tirés de ses manuscrits apportés de l'Italie* (1797, Paris).

<sup>20</sup> Cited by Duhem (*RQS*, 24 (1903), 491; *Origines*, vol. 1, 35) from *Ztsch. Völkerpsych. Sprachwiss.*, 14 (1883), 386.

<sup>21</sup> 1st ed. 1788, 2nd ed. 1811–15 (also in Lagrange's *Oeuvres*, vols. 11–12), 3rd ed. 1853–55, Paris. Relevant passages in the first edition are the briefest.

<sup>22</sup> *RQS*, 54 (1903), 507; *Origines*, vol. 1, 51.

<sup>23</sup> *RQS*, 54 (1903), 465; *Origines*, vol. 1, 8.

<sup>24</sup> Libri, vol. 3, 160ff.

<sup>25</sup> Lagrange (footnote 21), 2nd ed., vol. 1, 7–8.

<sup>26</sup> (1883, Leipzig), and later editions. French translation: *La mécanique: Exposé historique et critique de son développement* (1904, Paris).

*Principien der Mechanik*.<sup>27</sup> Both could have been readily available to him.<sup>28</sup> He read and published in German, and reviewed the French edition of the Mach work before its publication in 1904.<sup>29</sup> In the period between Archimedes and Galileo, Mach knew only of Leonardo, Guido Ubaldo and Stevin, while Dühning knew only of Leonardo and Guido Ubaldo, and referred to Libri and Venturi for information. On the interval between Archimedes and Galileo Dühning has this to say:

Hence the eighteen centuries separating Galileo from Archimedes prevent us not at all from joining the one event almost directly to the other. What has to be considered before or almost contemporaneously with Galileo may be broadly characterized as a preparative re-awakening of the ancient inheritance, and moreover, in view of its occurrence relative to the length of time only shortly before Galileo's achievements, we can on the whole regard this interval as a historical desert.<sup>30</sup>

Like Mach and Dühning, Duhem assumed in all his works before the *Origines* that the Middle Ages had been scientifically sterile. So they and authors like them, together with Lagrange, could have constituted his preparatory reading. Their citation of Libri and Venturi would have led him to the evidence that upset his expectations.

But anti-mediaeval prejudice is not all that they share with Duhem. They too use history as a basis of philosophical argument aiming to reform contemporary physics. That aim is reflected in the phrasing of their titles—'historisch-kritisch', 'kritische Geschichte'—which is reminiscent of Georg Helm's *Die Lehre von der Energie, historisch-kritisch entwickelt* (1887, Leipzig), and Duhem's own *Les théories électriques de J. Clerk Maxwell; étude historique et critique*,<sup>31</sup> works which are of just this type.

It is hard to say how far this represents a genuine historiographical tradition, and to identify its sources. But in addition to the doctrine of mediaeval sterility, and the use, noted above, of history as a medium for contemporary polemic, we may perhaps recall Mach's belief in history as a means to understanding ('anhand der Geschichte zu verstehen')—and note that he too expressed a concern with origins in his early *Die Geschichte und die Wurzel des Satzes von der Erhaltung der Arbeit* (1872, Prague).<sup>32</sup> It is not difficult to conjecture that traditions like these could have been handed down in the teaching of physics.

In this complex of attitudes, the doctrine of mediaeval sterility had been put at risk by Duhem's discovery in Jordanus de Nemore of a precursor for the mechanically significant parts of Leonardo. Was that doctrine thereby refuted or merely qualified? A refutation requires more than one Jordanus:

<sup>27</sup> (1873, Berlin); (1877, Leipzig). The Bibliothèque Nationale has a 3rd edition.

<sup>28</sup> Sarton's *Horus* (1952, Waltham, Mass.) does not add further references. Dr. C. B. Schmitt has referred me to M. Rühlmann, *Vorträge über Geschichte der theoretischen Maschinenlehre* (1885, Braunschweig). This seems to want to correct the sterility doctrine somewhat, but makes no essential difference to the picture here presented.

<sup>29</sup> The review was published in 1903 and the book in 1904, and the review reads *Etude* for *Exposé* in the title.

<sup>30</sup> Dühning (footnote 27), 1st ed., 10–11; 2nd ed., 9–10.

<sup>31</sup> First published in the *Annales de la Société Scientifique de Bruxelles*, 2<sup>e</sup> partie (Mémoires), 24 (1900) and 25 (1901), and in book form (1902, Paris).

<sup>32</sup> It appears also in the frequently reprinted *Populär-wissenschaftliche Vorlesungen* (1896, Leipzig).



it requires that he be part of a connected tradition. If Duhem was to establish such a tradition, he could hardly avoid a large-scale search for other mediaeval precursors of early modern mechanics and hence historical research on a massive scale. This is just what did happen.

### 5. Precursors and continuity

Of this massive programme of research, Duhem's *Études sur Léonard de Vinci* was to be the natural and best known expression, but the search for Leonardo's precursors was not the only motivating factor. In the preface cited above, he mentions another. The two treatises attributed to Jordanus referred to by Bosmans and cited in section 2 were radically incompatible with each other, so that 'If we wanted to know just what mechanics owed to Jordanus and his disciples, we had to go to the contemporary sources, to the manuscripts'.<sup>33</sup> But this is a limited problem, whose solution required only a limited search for Jordanus's manuscripts. Duhem's next sentence gives us something altogether more vast: 'There was no way out of analysing every manuscript relating to statics in the Bibliothèque Nationale and the Bibliothèque Mazarine'<sup>34</sup>—and all in six months, even with the help of the librarians he names. This is no limited investigation of the Jordanus problem, but a wide-ranging hunt for the roots of modern mechanics. Chapter 5, published in April 1904, confirms this impression:

But so far we have only obtained a crude sketch of the development of statics from Antiquity to the Renaissance; to the essential outlines we have given, a mass of detail must be added.

To establish this detail we have had to impose laborious drudgery on ourselves: we have had to examine and analyse the many manuscripts relating to statics held in the Bibliothèque Nationale and the Bibliothèque Mazarine. This analysis has allowed us, we believe, to discover more than one spring, unknown or misunderstood till now, whose waters have copiously contributed to the formation of modern science . . .<sup>35</sup>

These fruitful springs lay between ancient times and the Renaissance; they were the mediaeval precursors of modern mechanics. As Duhem says, his search for these springs was incomplete: 'but despite our investigations, many questions still remain obscure; we have no doubt that like investigations pursued in the main libraries of Europe would yield new finds to curious minds, allow them to fill in the holes we have had to leave gaping, and perhaps lead them to alter some of our conclusions'.<sup>36</sup>

Launched while the *Origines* was in progress, the hunt for precursors could hardly avoid being chaotic, and Duhem soon felt the need to give it conscious justification. In less than a year, in early September 1904, his 'De l'accélération exercée par une force constante' recorded his discovery of the theory of impetus. At its close he attends to the problem of identifying his precursors, and remarks of the law that acceleration is proportional to force:

Its birth was the result of a very slow and complex evolution; the few correct ideas making it up were only separated with great difficulty from the false ones that they had been confused with. After appearing at one moment,

<sup>33</sup> *Origines*, vol. 1, ii.

<sup>34</sup> *Ibid.*

<sup>35</sup> *RQS*, 55 (1904), 560–561; *Origines*, vol. 1, 61–62.

<sup>36</sup> *Ibid.*

they were often veiled again for a long time. It is almost always impossible to fix precisely the moment at which each appeared for the first time, vain to try to name the true inventor. There is scarcely any important doctrine in mechanics that does not lend itself to the same remarks.<sup>37</sup>

Thus for Duhem it is impossible to date the first appearance of important ideas at all precisely, and vain to assign priorities, and thus just as vain to divide the earlier writers into the true precursors who got it right and the false ones who did not. The hunt is for the partial truths contributing to the growth of the mature theory. As Duhem put it in October 1904: 'Science knows no spontaneous generation. The most unexpected discoveries are never created entire within the intellect that gave them birth. They are invariably the issue of an initial seed deposited in this genius whose rôle is limited to nurturing and developing this little seed sown in him, until the tree with its abundant foliage yields its flowers and its fruits'.<sup>38</sup> From this eloquent expression of Duhem's continuity thesis, I draw one consequence: on it he cannot ascribe to Tartaglia or Jordanus the principle of virtual displacements or any other principle of Lagrangian statics; instead, he will give them credit for a germ or seed out of which the principle will eventually emerge. Such seeds are the object of his search.

How do the seeds grow into the trees? To tell us, Duhem must do more than find precursors: he must identify the leading themes in the story of which they form part and reconcile the result, if he can, with his general methodological views. For the methodology itself was now at risk.

## 6. Precursors for Duhem

If Duhem's precursors were indeed the precursors of modern physics, they needed to be subject to the methodology he had evolved for that physics: in particular, they ought to be the products of an autonomous discipline showing no significant dependence on other disciplines such as metaphysics or theology. For Duhem, the autonomy of physics was an essential regulative principle, explicitly re-emphasized in the *Théorie physique* in progress in 1904–05. In chapter 2 of Part I of that work, he argued that treating physical theory as a metaphysical explanation of material reality made it dependent on metaphysics and thereby restricted its acceptance to those recognizing the philosophy to which it appealed. On the other hand, 'Could we not assign to physical theory an object that renders it autonomous? Founded on principles depending on no metaphysical doctrine, it could be judged for itself without the opinions on it of the various physicists being at all influenced by the philosophical schools they may belong to'.<sup>39</sup> If that was not how physical theory was seen in the 17th century, Duhem replies that the physical theories of Descartes, Huygens, Newton and Leibniz can be read out of the story. Despite their own opinions to the contrary, their metaphysics did not help them to their achievements.

But Duhem assessed the importance of Aristotle and Archimedes in the 16th century according to different principles. In October 1903 he broke

<sup>37</sup> P. Duhem, 'De l'accélération exercée par une force constante; notes pour servir à l'histoire de la dynamique', *Comptes rendus du Deuxième Congrès de Philosophie* (ed. E. Claparède: 1905, Geneva), 859–915 (p. 915).

<sup>38</sup> *RQS*, 56 (1904), 394; *Origines*, vol. 1, 156.

<sup>39</sup> *Théorie*, 25.

with tradition in proclaiming the most novel of Leonardo's thoughts on mechanics to be essentially dependent on Aristotle's *Mechanical questions*.<sup>40</sup> The implications of this judgement are made clear, again in 1903, at the close of chapter 1, as he compares the achievements of Aristotle and Archimedes: 'Hence, in the study of the equilibrium of weights, Archimedes had got to the same point as Aristotle, but by an entirely different route: instead of deriving his principles from the general laws of motion, he rested his theoretical edifice on a few simple and certain laws of equilibrium, and thus made of the science of equilibria an autonomous science owing nothing to the other branches of physics: he founded statics'.<sup>41</sup>

Duhem ought to have applauded that achievement, but he saw disadvantages: the method of Archimedes achieved perfect clarity and extreme rigour, but did so at the expense of generality and fertility. New problems required completely new independent principles to treat them. But instead of regarding this as a temporary loss, to be won back by Archimedes's mechanical successors, he continues: '... the certitude and clarity of his principles largely derive from their being drawn, so to speak, from the surface of the phenomena and not dug out from the foundation of things; in a phrase applied less appropriately by Descartes to Galileo, Archimedes 'explains very well *that it is* but not *why it is*'. So we shall see the most substantial advances in statics come from the teaching of Aristotle rather than the theories of Archimedes'.<sup>42</sup> Aristotelian mechanics did search for explanations and was not autonomous, but it was genuinely fruitful. By admitting so much, Duhem put his methodology at risk. It could perhaps be saved by a claim that in this early, primitive stage in the development of physics, different rules apply, but if that claim is to stick, Duhem needs to explain just how ancient and mediaeval physics were primitive, and how they led to the later physics to which his rules *do* apply.

In 1908 Duhem provided the beginnings of a solution to this problem in his *To save the phenomena*. Here he claimed that mediaeval physics was indeed primitive, and that its primitiveness consisted in the incomplete separation of physics and metaphysics in all fields except celestial physics (Duhem's terminology). Just as in modern physics as understood by Duhem, this had always been separated into a mathematical descriptive part ('astronomy') and a metaphysical cosmological part ('physics'); but in terrestrial physics things were different: 'In Antiquity, in the Middle Ages, and in the Renaissance it would have been hard to make this division: sublunary physics hardly knew mathematical theory... physicists were very embarrassed when they tried to assign perspective and the science of weights their proper place in the hierarchy of the sciences'.<sup>43</sup> But despite their primitive terrestrial physics, they debated, mainly in the context of celestial physics, a problem which Duhem recognized as his own:

What is the significance of physical theory? How is it related to metaphysics? Questions much discussed in our own time, but, like many others, by no means new: they belong to all time and have been put for as long as there has been a science of nature. If the clothes they wear change a little

<sup>40</sup> *RQS*, 54 (1903), 472–473; *Origines*, vol. 1, 16.

<sup>41</sup> *RQS*, 54 (1903), 468–469; *Origines*, vol. 1, 11–12.

<sup>42</sup> *Ibid.*

<sup>43</sup> *APC*, 156 (1908), 590; *Phainomena*, 138.

from one century to the next, because they borrow their variable clothing from the science of the moment, it is enough to strip the clothing to recognize that they remain essentially the same.<sup>44</sup>

In the debate about the relation of 'physics' to 'astronomy' in ancient and mediaeval celestial physics, Duhem had recognized his own problem of the relation of metaphysics to physics. He had found precursors for himself, and that discovery was to form the basis for his unfinished ten-volume synthesis, the *Système du monde* (1913–58, Paris).

### 7. Conclusion

A committed Catholic may not have been displeased to discover that the Middle Ages were not the barren desert which tradition asserted. Nor would a patriotic Frenchman be sorry to discover the significance, celebrated in vol. 3 of the *Etudes*, of the 14th-century Faculty of Arts of the University of Paris. But the essential element in my story is the surprise. To understand the surprise, we need to know what he expected and what he was doing. Pierre Duhem's expectations were shaped by his training as a physicist, and the researches that led to his surprise by his concerns as a physicist. Long ago at school Jules Moutier had got him 'to love the theories of physics'.<sup>45</sup> And it was the physicist that Moutier helped to train who was surprised by Jordanus de Nemore in the autumn of 1903.

<sup>44</sup> *APC*, 156 (1908), 113; *Phainomena*, 3.

<sup>45</sup> Bordeaux, 72. E. Jordan enlarges: '... et qui lui fit "aimer"', dit-il, en donnant au mot son sens le plus fort, "les théories de physique" (p. 11).

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