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# Fraud and Illusion in the Anti-Newtonian Rear Guard

## The Coultaud-Mercier Affair and Bertier's Experiments, 1767–1777

By James Evans\*

The clouds of prejudice through which we see objects distort them to the point that, for us, they no longer resemble themselves.

—Mercier, *Journal des Beaux-Arts et des Sciences* (December 1771)

IN 1769 THE SIXTH ISSUE OF A NEW MONTHLY, the *Journal des Beaux-Arts et des Sciences*, opened an attack on Newton's law of gravitation. In the first article of the June number, Jean Coultaud described pendulum experiments carried out in the mountains of Savoy. The experiments, apparently conducted with great care, seemed to prove that, contrary to Newton's inverse-square law, the weight of an object actually *increases* with its distance above the surface of the earth. Coultaud's results were soon confirmed by Mercier, as well as by Father Joseph-Etienne Bertier, who performed a completely different sort of experiment in the Church of the Oratory in Paris. These refutations of Newton's law of attraction were in turn refuted by leading mathematicians and mechanicians such as Jean le Rond d'Alembert and Joseph-Jérôme de Lalande. The anomalous gravity experiments provoked an eight-year debate marked by controversy, rising tempers, and a fresh round of experiments by investigators all over France (see the Appendix). The debate appears all the more remarkable when one learns that the first round of experiments never took place and that Coultaud and Mercier, the authors of the first papers, were fictitious persons. Obviously, more than a detail of physics was at stake.

It is well known that Newton's theory of universal gravitation was resisted on the

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Continent by an alliance of Catholic clergy and Cartesian natural philosophers.<sup>1</sup> In the fourth and fifth decades of the eighteenth century, the Cartesian position eroded badly as Newton's principles received confirmation in three dramatic tests. First, the expeditions of the French Academy of Sciences to Lapland (1736–1737) and to Peru (1735–1744) confirmed Newton's argument that the earth should be flattened at the poles by its rotation. However, the inverse-square law still faced problems in celestial mechanics. In particular, the difficulty of accounting for the observed motion of the moon's line of apsides using Newton's principles suggested that the inverse-square law might be mistaken. Neither Leonhard Euler, nor d'Alembert, nor Newton himself had been able to calculate a result in agreement with the observed motion. In the late 1740s Alexis-Claude Clairaut showed how to do the calculation correctly. Clairaut also made a famous and successful prediction of the date of return of Halley's comet. In most accounts, it is therefore supposed that resistance to Newtonian principles largely ceased by the middle of the 1750s.<sup>2</sup>

While this is undoubtedly correct as far as the leading French mathematicians and astronomers were concerned, the Coultaud-Mercier affair demonstrates that the Cartesian rear guard kept up a low-grade sniping attack for another two decades. This episode of fraud, posturing, and self-delusion has a number of victims, whom we shall presently meet. Identifying the culprit is more difficult—though it shall be attempted near the end of the article. Whether we succeed in identifying a culprit is, of course, less important than exploring the history of this affair and understanding the reasons for such a late and surprisingly vigorous attack on the principles of Newton.<sup>3</sup> Besides asking who did it, we want especially to know why. In seeking an answer to this question we shall have to become acquainted with an underclass of late Enlightenment culture, the anti-Newtonian rear guard. We shall also need to consider the politics of scientific publication in France and the role of the periodical press in scientific debate.

### COULTAUD'S EXPERIMENT

In the *Journal des Beaux-Arts et des Sciences* for June 1769, an article by Jean Coultaud, "former professor of physics at Turin," described a series of pendulum experiments carried out in the mountains of Savoy. Coultaud had retired from teaching to his family home in Samoëns. He had long meditated on the mystery of Newton's attraction, but it was to his sojourn among the immense mountains of the Alps that he owed the idea for his experi-

<sup>1</sup> A good example is the opposition to Newtonianism by the teachers of physics in the Jesuit schools of France. See François de Dainville, S.J., "L'enseignement scientifique dans les collèges des Jésuites," in *Enseignement et diffusion des sciences en France au XVIIIe siècle*, ed. René Taton (Paris: Hermann, 1964), pp. 27–65.

<sup>2</sup> For an account of the expeditions see Tom B. Jones, *The Figure of the Earth* (Lawrence, Kans.: Coronado, 1967). References to recent scholarship are given in Mary Terrall, "Maupeituis and Eighteenth-Century Scientific Culture" (Ph.D. diss., Univ. California, Los Angeles, 1987), pp. 6–13. For a discussion of the geodetic operations see J. L. Heilbron, *Weighing Imponderables and Other Quantitative Science around 1800 (Historical Studies in the Physical and Biological Sciences, Suppl. to Vol. 24, Pt. 1)* (Berkeley: Univ. California Press, 1993), pp. 213–231. On Clairaut's calculations and the debate over the inverse-square law see Craig B. Waff, "Universal Gravitation and the Motion of the Moon's Apogee: The Establishment and Reception of Newton's Inverse-Square Law, 1687–1749" (Ph.D. diss., Johns Hopkins Univ., 1975); and Curtis Wilson, "Clairaut's Calculation of the Eighteenth-Century Return of Halley's Comet," *Journal for the History of Astronomy*, 1993, 24:1–15. For the end of resistance to Newton's principles see, e.g., Thomas L. Hankins, *Science and the Enlightenment* (Cambridge: Cambridge Univ. Press, 1985), pp. 37–41.

<sup>3</sup> Delambre, who was himself a member of the Academy, said that Pierre Bouguer, who died in 1758, was "the last apostle of Cartesianism at the Academy." See Jean-Baptiste-Joseph Delambre, *Histoire de l'astronomie au dix-huitième siècle* (Paris, 1827), p. 364. A brief synopsis of "the Bertier Controversy" was published in A. Stanley MacKenzie, *The Laws of Gravitation: Memoirs by Newton, Bouguer, and Cavendish together with Abstracts of Other Important Memoirs* (New York: American Book Co., 1900), pp. 47–49.

ment. He was aware of the demonstration by Jean Richer that a pendulum clock at Cayenne, near the equator, runs more slowly than a clock at the middle latitudes. Coultaud therefore provided himself with “two excellent pendulum clocks executed by one of the most able clockmakers of Geneva,” for which he paid dearly. The clocks were constructed according to the principles of Julien Leroy, to minimize changes in the oscillation rate with heat or cold, due to the expansion or contraction of the metals. In short, these clocks were the best possible. Moreover, Coultaud spent five months testing the clocks before satisfying himself that they would do.<sup>4</sup>

Furnished, then, with these clocks, Coultaud had a wooden cabin constructed on a mountain ledge, at an elevation of 1,085 *toises* above his own farm. One of the clocks, in the care of Coultaud’s friend, M. Andrier, an “intelligent, active, and educated man,” was transported to the cabin.<sup>5</sup> The other clock remained at Coultaud’s place of residence. The two men agreed to start the clocks at the midnight beginning 1 July 1767. To guarantee simultaneous starts, Coultaud placed one of his brothers on the mountain, at a place roughly halfway between Coultaud’s habitation and the cabin above. During the day of 30 June the brother placed a signal toward which the two observers aimed rules, so that they might know where to look during the night. Near midnight, the brother fired a gun, which was the signal to prepare for the visual signal to follow. Then he lit a large quantity of powder in the open air. The flash was the signal to let go the restraints holding back the pendulums. The two clocks were thus started simultaneously. Although both clocks had been constructed to minimize the effects of temperature variations, it was nevertheless agreed to keep each clock at 12 degrees above the ice point. Andrier had to keep a stove going to maintain the upper clock at the agreed-upon temperature.

Coultaud had expected the upper clock to run more slowly. The same prediction obtained whether one followed “Huyghens and the other partisans of centrifugal forces” or “the principles of the Newtonians.” After two months, at midnight on 1 September, the clocks were stopped according to a procedure similar to the one used to start them. The brother produced a powder flash, upon which Coultaud and Andrier noted the positions of the minute and second hands of the clocks. Coultaud writes: “One can judge how impatient I was to see my friend again. We awaited him in my house, where my brother and I had reunited. M. Andrier rejoined us there the next day. . . . On his arrival I compared our two notes, and I was astonished.”<sup>6</sup> *The upper clock had run more rapidly and was ahead by 27 minutes, 20 seconds.* The conclusion was inescapable. The weight of an oscillating bob was greater at the upper station than at the lower. The inverse-square law of Newton seemed to be refuted.

This was so contrary to what Coultaud had expected that he doubted his results. He spent the fall and winter running the clocks side-by-side in his house to verify again that

<sup>4</sup> Jean Coultaud, “Lettre de M. Jean Coultaud, ancien professeur de physique à Turin, à M. l’Abbé Aubert, auteur du Journal des Beaux-Arts & des Sciences,” *Journal des Beaux-Arts et des Sciences*, June 1769, pp. 389–419, on pp. 389, 394–395 (here and throughout, translations are mine unless otherwise indicated). The letter was dated “A Samoins en Faucigni, Province de Savoie, ce 15 Novembre 1768.” On Richer’s demonstration see J. W. Olmstead, “The Scientific Expedition of Jean Richer to Cayenne (1672–1673),” *Isis*, 1942, 34:117–128. The clockmaker Julien Leroy (1686–1759) introduced a temperature-compensating mechanism to keep the effective length of the pendulum approximately constant.

<sup>5</sup> Coultaud, “Lettre de M. Jean Coultaud,” p. 397. The *toise* was the unit of length commonly used in surveying in French-speaking lands before the metric reform of the 1790s. Roughly equivalent to the English fathom, the *toise* was divided into 6 feet, each of which contained 12 inches (*pouces*), each of which was divided into 12 lines (*lignes*). One *toise* was approximately 1.95 meters. See R. E. Zupko, *Revolution in Measurement: Western Weights and Measures since the Age of Science* (Memoirs of the American Philosophical Society, 186) (Philadelphia: American Philosophical Society, 1990), pp. 114–115.

<sup>6</sup> Coultaud, “Lettre de M. Jean Coultaud,” pp. 401, 403.

they marched at the same rate. The following summer (1768) he repeated the experiment, this time reversing the positions of the two clocks. Also, in spite of his opinion of Andrier's merits, Coultaud could not help suspecting that his friend had done something amiss. Therefore, "despite his infirmities," Coultaud himself climbed to the upper cabin and remained there for two months.<sup>7</sup> He placed his brother in the house in the valley bottom and relegated Andrier to the role of signaler. Nevertheless, the result of the previous summer was confirmed. *The upper clock gained 28 minutes, 25 seconds, in the 61 days.*

Now, one might suppose that the upper pendulum ran more quickly because the air was less dense at the upper station. Coultaud therefore used two portable barometers of his own invention to investigate the air pressure at the lower house, at the signal station (560 *toises* higher), and at the upper cabin (1,085 *toises* above the lower station). The signal station was about halfway between the house and the cabin. An extensive series of measurements showed that the air pressure at the signal station was always about halfway between the values measured at the upper and lower stations. Thus the air pressure decreased linearly with height, at about the rate of 1 *ligne* of mercury for every 12 *toises* of elevation. This implied that the successive layers of air all had the same density. The density of the air was therefore eliminated as a cause of the difference in the clock rates.

Coultaud pointed out that, in each of the two experiments, the difference in the readings of the two clocks was about 1/3,000 of the elapsed time (half an hour is about 1/3,000 of 61 days). Also, the difference between the elevations of the two stations was about 1/3,000 of the radius of the earth (1,085 *toises* is about 1/3,000 of 3,270,000 *toises*, Coultaud's figure for the radius of the earth). This seemed to imply that the weight of a pendulum bob does not vary as the inverse square of its distance from the center of the earth but, rather, increases in direct proportion to the distance. Coultaud concluded, "Attraction and the laws that one attributes to it find their coffin in these facts," but he refrained from engaging in "long reasonings" about the further implications of his experiments. "Men better instructed and shrewder than I will no doubt perform this task after repeating the experiments I have described."<sup>8</sup>

#### MERCIER'S CONFIRMATION AND THE RESPONSE OF THE NEWTONIANS

Coultaud's article was published in the *Journal des Beaux-Arts et des Sciences* for June 1769. The first public response came on 10 June, in the form of a paper read at the Paris Academy of Sciences by Jean le Rond d'Alembert.<sup>9</sup> D'Alembert accepted Coultaud's experiments as carefully made, but he could not accept Coultaud's conclusions for the figure of the earth and the system of gravitation. He calculated that, under the inverse-square law, the weight of an object would be the same at the top of a mountain as at its base if the density of the mountain were greater than the mean density of the globe in the ratio 4 to 3. Moreover, he showed that the acceleration of Coultaud's upper pendulum (28 minutes in 2 months) could be accounted for if the density of the mountain exceeded the mean density of the globe in the ratio of about 8 to 3.

Three days later, d'Alembert sent a letter to the *Journal* quoting the results of these

<sup>7</sup> *Ibid.*, p. 405.

<sup>8</sup> *Ibid.*, pp. 411, 413–414.

<sup>9</sup> See Jean le Rond d'Alembert, *Opuscules mathématiques*, Vol. 6 (Paris, 1773). Article 4, "Sur l'effet de la pesanteur au sommet & au pied des montagnes" (pp. 85–92), was the basis of the paper that d'Alembert read at the Academy on 10 June 1769. In 1772 d'Alembert wrote an "Addition à la article précédent" (pp. 93–97), which extended his argument and also took account of Mercier's claims.



calculations. It arrived in time to appear in the July issue.<sup>10</sup> D'Alembert also argued that, because the density of the surface layers of the earth might be greater than the mean density of the globe, it could easily happen that the ratio of the density of the Alps to the density of the earth at the foot of the mountains was much less than 8 to 3. This response must have been maddening to the anti-Newtonians: it dismissed Coultaud's results as due to regions of anomalously high density, while claiming in advance that these anomalies might not be detectable by density measurements of rock samples. It seemed to say that experimental attacks on Newton's system of attraction were simply not to be entertained.

D'Alembert concluded by pointing out that Coultaud's results certainly could not hold for *all* mountains and cited the well-known results obtained by Pierre Bouguer in the French expedition to South America.<sup>11</sup> The weight of a pendulum bob was less at Pichincha than at Quito, and less at Quito than at the edge of the sea. Pichincha is 2,434 *toises* above sea level; Quito, 1,466. Unfortunately and perversely, through a mistake of the editor or printer, *less* was changed to *greater*, and the July issue of the *Journal des Beaux-Arts et des Sciences* appeared with d'Alembert apparently citing Bouguer in support of Coultaud's results! This typographical error led some writers astray in the course of the controversy.<sup>12</sup>

If the Newtonians believed that the new evidence could be so easily dismissed, they were soon set right. Coultaud's call for others to repeat his experiments was taken up by Mercier. The original results were confirmed with striking clarity. Mercier's paper took the form of an open letter addressed to Johann Gessner of the University of Zurich.<sup>13</sup> Mercier's experiments were similar in type to Coultaud's but differed in some details.

Mercier lived in Valais. At first he did not require the construction of a special cabin at the upper station, as Coultaud had, but availed himself of a cheese house located 514 *toises* higher than his own residence. Mercier repeated the experiment several times, so that he might vary the height difference. Thus, he also used the house of his friend, a certain Captain Muller, located 210 *toises* above his own house. Finally, seeking the largest possible height difference, Mercier built an eight-by-ten-foot cabin of boards some 847 *toises* above his residence. Like Coultaud, Mercier had a temperature-compensating clock built according to the principles of Julien Leroy. But at first he had only had one clock. It was allowed to run for a few months at the lower station, then transported to the upper station for a few months. The starting and stopping were regulated by taking meridian observations of the sun at the lower station and signaling the moment of noon with a gunshot. Later, Mercier had a second clock built, in Lausanne, and was able to carry out experiments exactly like the original experiments of Jean Coultaud.

The results were essentially the same. The upper clock ran faster, and the difference between the clock rates was proportional to the difference in height. Mercier's results are summarized in the table below:  $\Delta r$  is the difference in elevation between the two stations;

<sup>10</sup> Jean le Rond d'Alembert, "Lettre de M. d'Alembert . . .," *J. Beaux-Arts Sci.*, July 1769, pp. 159–162.

<sup>11</sup> Pierre Bouguer (1698–1758) was a member of the French geodetic expedition to Peru. He gave an account of the meridian survey and of his pendulum experiments in *La figure de la terre* (Paris, 1749). An extract devoted to the gravity measurements in Peru is given in English translation in MacKenzie, *Laws of Gravitation* (cit. n. 3).

<sup>12</sup> For example, Abbé Genet of the Collège Mazarin, who was the royal censor charged with examining each issue of the *Journal des Beaux-Arts et des Sciences*, contributed a piece to the debate: Genet, "Lettre de M. l'Abbé Genet . . . à M. Mercier . . . pour répondre à sa lettre insérée au mois de décembre dernier . . .," *J. Beaux-Arts Sci.*, Jan. 1772, pp. 5–35. In his discussion, Genet got tangled up by the misprint in d'Alembert's letter.

<sup>13</sup> Mercier, "Lettre à M. Gessner, professeur de physique dans l'Université de Zurich," *J. Beaux-Arts Sci.*, Dec. 1771, pp. 389–416. The letter was dated "A Sion en Valais, ce 15 Août 1771." On Johann Gessner (1709–1790) see James Roger Hansen, "Scientific Fellowship in a Swiss Community Enlightenment: A History of Zurich's Physical Society, 1746–1798" (Ph.D. diss., Ohio State Univ., 1981).

$r$ , the radius of the earth;  $T$ , the time elapsed;  $\Delta T$ , the difference between the clock readings at the two stations.

	$\frac{\Delta r}{r}$	$\frac{\Delta T}{T}$
First trial	$514\frac{1}{2}r = 1/6,362$	$21^m08^s/90^d = 1/6,132$
Second trial	$210\frac{1}{2}r = 1/15,571$	$15^m06^s/177^d = 1/16,879$
Third trial	$847\frac{1}{2}r = 1/3,861$	$21^m05^s/61^d = 1/4,166$

In each case,  $\Delta T/T$  was nearly equal to  $\Delta r/r$ . The astronomer Lalande published a short note in the *Journal des Sçavans*, dismissing Mercier’s results as due to local density anomalies.<sup>14</sup> Thus Lalande performed the same service for Mercier as d’Alembert had for Coultaud.

### THREE ANTI-NEWTONIANS

In the public debate three anti-Newtonians played major roles: J.-C.-F. de La Perrière, J.-E. Bertier, and J.-P. David. Each of these three had previously articulated a system of the world incompatible with Newtonian principles. Each felt he had been ignored or abused by the Newtonian establishment. And, as we shall see, each seized upon Coultaud’s pendulum experiment as affording decisive new evidence in support of his own views. Collectively, these three men nicely represent the anti-Newtonian rear guard of the 1760s and 1770s. Let us meet them and see what use they made of Coultaud’s paper.

#### *La Perrière*

The first to enter the fray was J.-C.-F. de La Perrière, the seigneur of Roiffé, a scientific dilettante and the inventor of an electrical system of the universe.<sup>15</sup> La Perrière’s credentials as an anti-Newtonian were amply established in 1761 when he sent letters to the Paris weekly, *L’Avant-Coureur*, calling into doubt the conclusions of the French academicians who had determined the earth to be flattened at its poles. La Perrière was not, however, a mere anti-Newtonian, for he opposed both the system of Newton and the system of Descartes and preferred to construct his own. At the beginning of his *New Celestial and Terrestrial Physics*, he placed these verses beneath his own portrait:

De Descartes et de Newton  
Osant attaquer les systèmes,  
De la Nature il prit le ton  
Et découvrit les lois suprêmes;  
Et de leur lumineux flambeau  
Il éclaira son système nouveau.<sup>16</sup>

<sup>14</sup> Joseph-Jérôme Le Français de Lalande, “Remarques sur de nouvelles expériences de pesanteur,” *Journal des Sçavans*, Aug. 1772, pp. 545–547.

<sup>15</sup> His full name is Jacques-Charles-François de La Perrière de Roiffé (1694–1776). Roiffé is in the valley of the Loire, south-south-east of Saumur. When Coultaud’s paper was published, La Perrière was living in Paris.

<sup>16</sup> “Daring to attack the systems/of Descartes and of Newton/he took his tone from Nature/and discovered the supreme laws;/with their luminous torch/he illuminated his new system.” These verses are quoted from the article on La Perrière de Roiffé in *Nouvelle biographie générale*, Vol. 29 (Paris: Firmin Didot Frères, 1859), col. 519, in which they are said to be from La Perrière, *Nouvelle physique céleste et terrestre à la portée de tout le monde*, 3 vols. (Paris, 1766). However, in the only two copies of *Nouvelle physique céleste et terrestre* that I have been able to consult, the portrait and the verses are not to be found. La Perrière’s letters to *L’Avant Coureur* appeared in the numbers for 1 and 6 June and 17 and 24 Aug. 1761.



La Perrière tirelessly expounded his new system of the universe, first of all in the two volumes of his *Mechanisms of Electricity and of the Universe*. When it dawned on him that few readers were willing to wade through these dense and obscure tomes, he produced an *Extract*, in a merciful sixty-one pages, which remains the most useful—and only readable—introduction to his electrical system of the universe.<sup>17</sup> The three volumes of his *New Celestial and Terrestrial Physics*, published five years later, sell the same system.

During the controversy over the anomalous gravity experiments, La Perrière published a *Burlesque Decree*, ostensibly delivered in the grand chamber of the Parnassus of the Hurons and Illinois. The joke goes like this. A delegation of Cartesians, Newtonians, and Cartesi-Newtonians, united for their common defense, has petitioned the court for action against the dangerous La Perrière, who threatens to undermine their doctrines with a systematic physics based on incontestable mechanical principles. The court therefore decrees that La Perrière shall be excluded for life from all prizes, titles, and literary honors. All members, associates, and correspondents of academies are forbidden to have any commerce, communication, or society with him or to enter into dispute with him, as this might lead to the subversion of the received systems. Moreover, the court decrees that the planets shall continue to move in elliptical orbits (although La Perrière has refuted them) and that light shall continue to accelerate in passing from air to water; the doctrines of the plenum and of the void (though mutually contradictory) are both declared infallible. The decree was dated “the forty-fifth day of June, year of the world 17770.”<sup>18</sup>

Not surprisingly, the Parisian Newtonians considered La Perrière a crackpot. Lalande especially found him nettlesome, and with good reason. La Perrière had accused Lalande and Pierre-Charles Le Monnier of stealing his ideas on the refraction of light by planetary atmospheres. Lalande and Le Monnier did not deign to respond to La Perrière’s complaints.<sup>19</sup> While the Newtonians thus formed a solid phalanx against La Perrière, the ranks of the anti-Newtonians were rent by internal squabbles. For La Perrière himself had been accused of plagiarism by one Charles Rabiqueau, who had invented his own electrical system of the universe, based, like La Perrière’s, on impulsion in a plenum.<sup>20</sup> Thus, at the time of the anomalous gravity experiments, we should picture La Perrière as a thoroughly alienated outsider, squabbling with the establishment Newtonians, who held him at arm’s length, as well as with Cartesians of various stripes.

<sup>17</sup> J.-C.-F. de La Perrière, *Mécanismes de l’électricité et de l’univers*, 2 vols. (Paris, 1756); and La Perrière, *Extrait du nouveau système général de physique et d’astronomie ou du système électrique de l’univers, de M. La Perrière de Roiffé, pour servir d’éclaircissement aux deux premiers volumes imprimés l’an 1756* (Paris, 1761).

<sup>18</sup> J.-C.-F. de La Perrière, *Arrêt burlesque donné sur requête et par défaut en la grand chambre du Parnasse illinois et huron, en faveur & pour le maintien des doctrines de Descartes et de Newton, contre celle de La Perrière, qui les contrarie* (Paris, 1770), p. 32.

<sup>19</sup> For the accusations of plagiarism see La Perrière, *Nouvelle physique céleste et terrestre* (cit. n. 16), Vol. 2, pp. v–ix; and *Plaidoyer de M. l’avocat général du Sénat littéraire* (Paris, 1768). The page following the title page carries the notice, “Plaidoyer pour M. de La Perrière . . . , demandeur, contre MM. Le Mosnier et de La Lande, académiciens, etc., accusés, défendeurs et défailans; et encore entre [sic] M. Charles Rabiqueau, avocat en Parlement, ingénieur opticien du Roi . . . intervenant et demandeur en complainte possessoire, contre mondit sieur de La Perrière de Roiffé, défendeur.” Lalande later had this to say about La Perrière’s *Burlesque Decree*: “One recognizes in this bad joke the style and the ignorance of the author”; see Joseph-Jérôme Le Français de Lalande, *Bibliographie astronomique* (Paris, [1803]), p. 520.

<sup>20</sup> Charles Rabiqueau, *Le spectacle du feu élémentaire; ou, cours d’électricité expérimentale . . . on y explique en outre la cause de la chute des corps au centre de la terre . . .* (Paris, 1753). Rabiqueau (fl. 1753–1783) was a lawyer by profession, but, as an *entrepreneur de spectacles*, he also gave public science shows in Paris. For this purpose he maintained a collection of physics demonstration apparatus at the Hôtel de Carignan, in the rue Bailleul. See Jean Torlais, “La physique expérimentale,” in *Enseignement et diffusion des sciences*, ed. Taton (cit. n. 1), pp. 619–645, on p. 641.

La Perrière's response to the anomalous gravity experiments was not what one might have expected from an anti-Newtonian. In fact, he sought to demonstrate that the results were not due to variations in weight at all but, rather, to temperature effects. Through all this, La Perrière showed great consistency. Some years earlier, he had objected on exactly the same grounds to the conclusions drawn about the shape of the earth by the French academicians who had gone to Lapland and Peru.<sup>21</sup> La Perrière's argument is quite clever, because it disposes of several kinds of observational evidence with a single principle.

If a pendulum is observed to run faster, there are two possible explanations: either the earth's attraction (as we would say, the value of  $g$ ) is greater, or the length of the pendulum is shorter. According to La Perrière, Coultaud's upper clock ran faster because the cold in the mountains had shortened that pendulum. Of course, Coultaud had controlled for temperature by keeping a stove going at the upper station. But, according to La Perrière, artificial heat is not as effective as natural heat in dilating materials.

Exactly the same principle explained the results of the academicians sent to Lapland and Peru. The surveys were conducted using standard metal rulers 2 *toises* long. According to La Perrière, the heat near the equator made the standard *toise* longer. Thus fewer *toises* were required to span a degree of latitude. The academicians were mistaken to infer a flattening of the earth from their measurements.

Jean Richer had shown that a pendulum runs more slowly at Cayenne, near the equator, than at Paris. Similarly, the academic expedition to Lapland had found that a pendulum runs more rapidly there than at Paris. According to La Perrière, the heat at the equator lengthened Richer's pendulum and so slowed it down. The cold contracted the pendulum at Pello, in Lapland, and made it run faster.

Thus, one principle sufficed to explain three different kinds of observations: Coultaud's and Mercier's experiments on the variation of pendulum rate with altitude (judged by others to be fatal to Newtonian attraction), the experiments of Richer and the academicians on the variation of pendulum rate with latitude, and the academicians' surveys in Lapland and Peru (both judged by others to be fatal to Cartesian physics).

What was La Perrière's game? La Perrière pointed out that there were two competing systems of central forces. One system (Newton's) required the weight to be greater at the base of a mountain and the other (the Cartesians') required it to be less. The results of the pendulum experiments could therefore, at least potentially, be fatal to one system or the other. But, said La Perrière, *if the weight were the same* at the base and the summit of a mountain, *this would refute both systems*. La Perrière maintained just this—that weight does not vary with height. Moreover, the earth is neither flattened at the poles, as the Newtonians say, nor elongated at the poles, as the Cartesians claim. Rather, *the earth is a perfect sphere*. La Perrière claimed, with some justification, that no Newtonian and no Cartesian could accommodate all of the experiments in a single system.

### **Bertier**

The writer who participated more earnestly than any other in the debate was Joseph-Etienne Bertier (1702–1783), a priest of the Congregation of the Oratory. Bertier was the author

<sup>21</sup> For La Perrière's response to the pendulum experiments see J.-C.-F. de La Perrière, "Lettre de M. de La Perrière de Roiffé . . . au sujet de celle de M. Jean Coultaud . . .," *J. Beaux-Arts Sci.*, Dec. 1769, pp. 299–402; La Perrière, "Observations de M. de La Perrière de Roiffé, sur l'accélération du pendule parisien à Pello, & sur celle des pendules de M. Coultaud . . .," *ibid.*, Feb. 1771, pp. 287–310; and La Perrière, "Observations de M. de La Perrière de Roiffé, sur l'explication des accélérations des pendules du pied au sommet des montagnes, proposée par M. Genet . . .," *ibid.*, May 1772, pp. 197–234. For the earlier objection to a flattened earth see La Perrière, *Nouvelle physique céleste et terrestre* (cit. n. 16), Vol. 1, pp. 48–54, Vol. 2, pp. 353–357.

of a dissertation on the question of whether the air of respiration passes into the blood, which won a prize from the Royal Academy of Bordeaux. In later life he turned from biology to physics, and he taught philosophy, chemistry, and physics at several houses of the Oratory in France. In 1748 he was named a *correspondant* of R.-A. Réaumur of the Academy of Sciences. Bertier's letters were occasionally read at the meetings of the Academy.<sup>22</sup>

From 1752 to 1756 Bertier taught at the house of the Oratory in Montmorency, northwest of Paris. He was at Montmorency when Jean-Jacques Rousseau arrived to live, first, at the Hermitage of Madame d'Épinay, a few miles from Montmorency, and later in Montmorency proper. This was the period in which Rousseau completed his most influential works: *La nouvelle Héloïse*, *Emile*, and *Le contrat social*. Bertier got on well with Rousseau, who, at least at first, found his simplicity and good nature appealing. In book 10 of his *Confessions*, Rousseau lists his friends from this stage of his life; he has this to say about Bertier's personality:

I had at Montmorency the Oratorians and among others Father Bertier, professor of physics, to whom, despite his light varnish of pedantry, I had attached myself because of a certain air of good-naturedness that I found in him. However, I had trouble reconciling this great simplicity with the desire and the art he had of pushing himself everywhere, among the Great, among the ladies, among the devout, among the philosophers; he knew how to be everything to everyone.

Rousseau is not an especially reliable witness on the characters of others, for he had scarcely a friend with whom he did not break. But his assessment of Bertier was apt. Bertier was generous and had little regard for money; but he wanted to be liked and he hungered for recognition.<sup>23</sup> His physical treatises are pedantic and reveal a thinker who was a generation behind the science of his times.

By the time of the Coultaud-Mercier controversy, Bertier had retired from active teaching. His order rewarded him by bringing him to Paris to live at the house of the Oratory in the rue St. Honoré. This was not a retirement for everyone. It was reserved for those elderly Oratorians who had earned it "by long and assiduous services, and for those who [could] honor it by their knowledge and their lights." At Paris, Bertier devoted himself to physics. He was inextricably attached to the system of Descartes. As he was present from time to time at court, he was known by sight to the king, Louis XV. On one occasion, when the king noticed him, he said, "*Voilà, the vortex man!*"<sup>24</sup>

<sup>22</sup> E. Bonnardet, "Joseph Etienne Bertier, prêtre de l'Oratoire . . .," *L'Oratoire de France*, Oct. 1937, 28:312–317. On the Congregation of the Oratory see Pierre Costabel, "L'Oratoire de France et ses colléges," in *Enseignement et diffusion des sciences*, ed. Taton (cit. n. 1), pp. 67–100. For the prize-winning dissertation see Joseph-Etienne Bertier, *Dissertation sur cette question: Si l'air de la respiration passe dans le sang . . .*, *Recueil des dissertations qui ont remporté le prix à l'Académie Royale des Belles-Lettres, Sciences et Arts de Bordeaux*, Vol. 5 (Bordeaux, 1739). For Bertier's status as *correspondant* see *Index biographique des membres et correspondants de l'Académie des Sciences du 22 décembre 1666 au 15 novembre 1954* (Paris: Gauthier-Villars, 1954), p. 42. For an example of Bertier's letters to the Academy see Bibliothèque Nationale (BN), Paris, MS Fr.n.a. 5151, fols. 46–47v. This letter from Bertier to the Academy of Sciences is dated 14 Aug. 1751. A penciled annotation indicates that it was "read at the Academy in December."

<sup>23</sup> Jean-Jacques Rousseau, *Oeuvres complètes* (Bibliothèque de la Pléiade), Vol. 1 (Paris: Editions Gallimard, 1959), pp. 504–505. On one occasion, a property in which Bertier had an interest went bankrupt and he suffered a great loss in income. His superior in the Congregation offered to make up some of this loss, but Bertier declined to accept, saying, "I am attached to nothing, and since Providence has permitted this test, it is my duty to bear it. The Congregation is a good mother. She will provide for all my needs": BN, MS Fr. 25681, fol. 108v. (See note 24.)

<sup>24</sup> BN, MS Fr. 25681, "Bibliothèque des écrivains de l'Oratoire . . . Par M. Adry, de l'Oratoire: A Paris, 1790." This manuscript has a printed title page, but the rest of the volume contains handwritten entries, alphabetically arranged by name. For Bertier see fols. 108–109. The writer, Adry, indicates that he has drawn some of his information from Clairfontaine's obituary notice of Bertier in *Journal de Paris*, 31 July 1784.

Indeed, at Montmorency Bertier had begun writing a book on the physics of comets, treated from a Cartesian point of view. He sought permission from the Academy of Sciences to have this book published under its privilege. This request caused some minor embarrassment, as shown by the procès-verbal of the meeting at which the academy considered Bertier's request. The difficulty was that Bertier's book was "completely opposed to the Newtonian philosophy, almost universally adopted today." However, the academicians conceded that Bertier's work was defensible "in the hypothesis of the plenum and of vortices." Thus the academy, "which persists in adopting no system," was able to accept Bertier's homage and to permit the book to be published under its privilege. *Physique des comètes* was published in 1760, after Bertier had settled in Paris, and he sent copies to various physicists and friends, including Rousseau. However, Bertier's relations with Rousseau soon came to an end. After the publication of the scandalous *Emile*, in 1762, Bertier's fellow Oratorians took him aside and let him know he must have much less to do with Rousseau.<sup>25</sup>

At Paris, our Oratorian, Joseph-Etienne Bertier, was on good terms with the Jesuit Guillaume-François Berthier, the editor of the *Journal de Trévoux*. G.-F. Berthier (1704–1782) edited this journal during its most influential years, when it became one of the leading French literary journals and a powerful advocate for social, political, and philosophical conservatism.<sup>26</sup> In its early days, the *Journal de Trévoux* acquired a reputation for pugnacious and intemperate criticism. When G.-F. Berthier assumed the editorship in 1745, he resolved to change all that. His editorial policy, frequently expressed in the pages of the journal, favored impartiality and moderation—except, of course, in matters of church and state, in which the obligations of the journal were clear. G.-F. Berthier was an admirer of Voltaire's early work and was on friendly terms with Rousseau. But as the century reached its midpoint and the philosophes relentlessly called into doubt received religion and the organization of French society, all of that went sour. The *Journal de Trévoux* began to criticize Voltaire's irreligion. When the first volume of the *Encyclopédie* appeared, the *Journal* attacked its *Discours préliminaire*, provoking replies by d'Alembert and Diderot and a particularly savage response by Voltaire. Berthier and the philosophes became implacable enemies. Berthier's defense of the Jesuits and his attacks on the *Encyclopédie* in the pages of the *Journal de Trévoux* motivated one of Voltaire's most caustic satires, the "Report of the Illness, Confession, Death, and the Apparition of the Jesuit Berthier."<sup>27</sup> In the story, Berthier is transporting by coach some copies of the *Journal de*

<sup>25</sup> Procès-verbal of the Académie Royale des Sciences, 22 Aug. 1759; quoted in Joseph Bertrand, *L'Académie des Sciences et les académiciens de 1666 à 1793* (Paris, 1869), pp. 146–147. Joseph-Etienne Bertier, *Physique des comètes, dans le sentiment de l'impulsion & du plein* (Paris, 1760), was dedicated to the members of the Academy of Sciences. The preface praises the Academy for not endorsing or condemning systems. The copy to Rousseau was accompanied by a letter: Joseph-Etienne Bertier to Jean-Jacques Rousseau, 26 June 1760, in *Correspondance complète de Jean-Jacques Rousseau*, ed. R. A. Leigh, Vol. 7 (Geneva: Institut et Musée Voltaire; Madison: Univ. Wisconsin Press, 1969), no. 1036. On the end of Bertier's close friendship with Rousseau see "Bertier (Joseph-Etienne)," in *Biographie universelle, ancienne et moderne*, Vol. 4 (Paris: Michaud, 1811), pp. 362–363.

<sup>26</sup> This journal, more accurately called *Mémoires pour l'Histoire des Sciences et des Beaux-Arts*, was originally edited by the Jesuits of the Collège de Louis-le-Grand in Paris and printed at the establishment of the duc Du Maine in Trévoux, just north of Lyons. By 1734 the journal was being printed as well as edited at Paris. For the last three decades of its life it continued, nevertheless, to be referred to familiarly as the *Journal de Trévoux*. See John N. Pappas, *Berthier's Journal de Trévoux and the Philosophes* (Studies on Voltaire and the Eighteenth Century, 3) (Geneva: Institut et Musée Voltaire, 1957); Claude Bellanger et al., *Histoire générale de la presse française*, 5 vols. (Paris: Presses Univ. France, 1969), Vol. 1, pp. 219–240; and Pascale Ferrand, "Mémoires de Trévoux," in *Dictionnaire des journaux 1600–1789*, ed. Jean Sgard (Paris: Universitas, 1991), pp. 805–816.

<sup>27</sup> Voltaire, "Relation de la maladie, de la confession, de la mort, et de l'apparition du Jésuite Berthier," in *Oeuvres complète de Voltaire*, 2nd ed. (Paris, 1825), Vol. 61, pp. 71–88. This farce was first published in 1759.



*Trévoux* to present to his *protecteurs* and *protectrices*. He soon falls ill. A doctor determines that he has been poisoned and asks the coachman whether he has been transporting dangerous substances for an apothecary. The coachman replies that all he has with him are two dozen copies of the *Journal de Trévoux*. “Well, gentlemen,” says the doctor, “Was I wrong?” As a remedy, he prescribes that Berthier swallow a page of the *Encyclopédie* with some white wine. Berthier dies anyway, but first confesses, which gives Voltaire a chance to rehearse a long list of offenses by the Jesuits.

Our Oratorian, Joseph-Etienne Bertier, resided in Paris from about 1756. In 1762 the Society of Jesus was suppressed in France. When Bertier heard the news, he ran immediately to the dwelling of his friend G.-F. Berthier, the editor of the *Journal de Trévoux*, to express his regret and to offer whatever material aid he might. The Oratorian and the Jesuit embraced one another with tears in their eyes.<sup>28</sup>

During the Coultaud-Mercier controversy, Bertier was engaged in writing a treatise called *Physical Principles to Serve as a Continuation to the “Mathematical Principles” of Newton*.<sup>29</sup> Through three volumes, Bertier took up all the phenomena of physics and chemistry, one at a time. He described in detail how each phenomenon was explained by the Newtonians (or, as Bertier called them, the *vacuistes & attractionnaires*) and by the Cartesians (*éthériens & impulsionnaires*). Then he worked through each camp’s rebuttal of the other’s arguments. Bertier claimed to be an advocate for neither system and maintained that his job was simply to provide a neutral account of all the evidence. And he tried to avoid a direct affront to the authority of Newton by constantly distinguishing between “mathematical attraction” (which, according to Bertier, was all that Newton insisted upon) and “physical or real attraction.” Bertier did not object to the use of effective attraction as a mathematical convenience. What galled him was any claim of an actual physical attraction between separated bodies. Thus he intended his *Physical Principles* to complement and complete the *Mathematical Principles* of Newton.

The first three volumes of Bertier’s work appeared in 1764, just before the Coultaud-Mercier affair began. The fourth volume appeared in 1770, in the middle of the controversy, and incorporated the results of Coultaud’s pendulum experiments. On the title page, Bertier boldly announced: “Nature, consulted by new experiments, decides the questions which divided all modern physicists.” The indecision of the first three volumes has disappeared, and Coultaud’s experiment is presented as decisive new evidence against attraction and the void. Three years later, Bertier was referring to the “immortal experiments of the Alps,” from which it followed that “gravitation is not the effect of the physical attraction of the terrestrial mass.”<sup>30</sup> There is therefore no doubt about Bertier’s allegiance to Descartes or about the utility he saw in the pendulum experiments of Coultaud and Mercier. Pendulums in the mountains had destroyed one world view and vindicated another.

However, Bertier himself had no experimental evidence to offer in support of Coultaud’s and Mercier’s results. Rather, he attempted to prove by reason that objects must weigh more the higher they are placed, up to a certain height not yet known. Bertier’s proofs, which have the form of thought experiments, were repeated and elaborated in a long series

<sup>28</sup> BN, MS Fr. 25681. This story is also told in the article on Bertier in *Biographie universelle* (cit. n. 25).

<sup>29</sup> Joseph-Etienne Bertier, *Principes physiques, pour servir de suite aux “Principes mathématiques” de Newton*, 4 vols. (Paris, 1764–1770).

<sup>30</sup> Joseph-Etienne Bertier, *Principes physiques dans lesquels la nature consultée par des expériences nouvelles, décide les questions qui partageoient tous les physiciens modernes*, Vol. 4 (Paris, 1770); and Bertier, “Lettre du P. Bertier . . .,” *J. Beaux-Arts Sci.*, May 1773, pp. 325–340, on pp. 325–326.

of publications.<sup>31</sup> Here is one of the simplest. Imagine two bodies elevated above the earth, stacked vertically one above the other, and released at the same moment. These two bodies will fall together, and the upper one will still be in contact with the lower one when the lower one strikes the ground. The upper body has more centrifugal force, because it begins farther from the center of the earth and both bodies participate in the earth's rotation. Thus it is clear that the upper body must gravitate more strongly: its greater weight compensates for the excess of centrifugal force.

Bertier also claimed to see the system of attraction as a danger to religion. In a piece published in 1774, in the course of the controversy, Bertier attacked the advocates of attraction as "atheistic Epicureans." They were "authors of systems opposed to Nature, who would like to take away from us our Father and our God, by maintaining that bodies attract one another and move of themselves without a first mover."<sup>32</sup>

### David

Jean-Pierre David (1737–1784) was a professor of surgery and anatomy at Rouen. He had published works on cesarean section, on the treatment of abscesses, and on means for suppressing or diminishing milk in women. David's credentials as an anti-Newtonian were established in 1769 when he published a *Dissertation on the Figure of the Earth*, in which he argued that, contrary to the findings of the expeditions of the Parisian academicians, the earth is elongated at its poles.<sup>33</sup> Indeed, David drew upon the observations of the academicians themselves to turn their conclusions upside down.

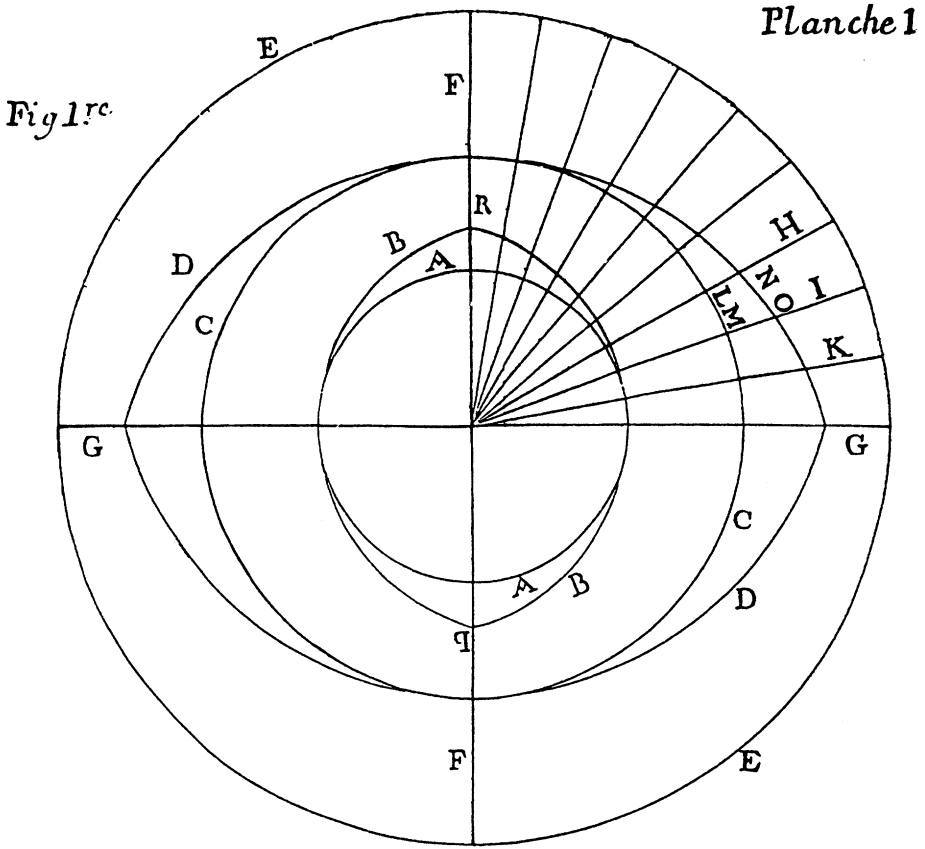
In Figure 1, *D* is an earth flattened at the poles, circumscribed around *C*, a spherical earth. The radial lines *H*, *I*, *K* passing through the center of the earth are spaced at 10-degree intervals. David points out that the 10-degree segments of the spherical earth are all the same length, equal to *LM*, but the 10-degree segments of the flattened earth increase

<sup>31</sup> Bertier, *Principes physiques*, Vol. 4; Joseph-Etienne Bertier, "Expériences nouvelles qui détruisent l'attraction, non pas l'attraction mathématique & hypothétique du célèbre Newton; mais l'attraction physique & réelle de quelques uns de ses disciples," *Journal Encyclopédique*, Feb. 1772, pp. 112–117; Bertier, "Lettre du P. Bertier, de l'Oratoire . . .," *J. Beaux-Arts Sci.*, Mar. 1772, pp. 489–500; Bertier, "Autre lettre du même . . .," *ibid.*, May 1772, pp. 267–270; Bertier, "Lettre du Pere Berthier [*sic*], de l'Oratoire, *ibid.*," July 1772, pp. 5–10; Bertier, "Lettre à M. L'Abbé Aubert, dans laquelle on répond aux réflexions de Genève sur les trois démonstrations du P. Bertier, contre l'attraction physique réelle & non Newtonienne," *ibid.*, Dec. 1772, pp. 511–517; and Bertier, "Lettre du P. Bertier" (cit. n. 30).

<sup>32</sup> Joseph-Etienne Bertier, "Lettre sur la pesanteur des corps," *Suite de la Clef, ou Journal Historique sur les Matières du Temps, Contenant aussi quelques Nouvelles de Littérature, et Autres Remarques Curieuses*, Nov. 1774, pp. 384–385, on p. 385. Eighteenth-century writers often called the *Suite de la Clef* the *Journal de Verdun*. Although it was edited and published at Paris, it was the descendant of *La Clef du Cabinet des Princes de l'Europe* (1704–1706) and of the *Journal Historique sur les Matières du Temps* (1707–1716), which had been edited at Verdun. See Sgard, ed., *Dictionnaire des journaux* (cit. n. 26), pp. 234–235, 1105–1106. The editors of *Suite de la Clef* had earlier attributed exactly these sentiments to Bertier in "Expérience sur la gravité des corps," *Suite Clef*, Feb. 1774, pp. 148–154, on p. 150. (Bertier had submitted a memoir; the editors, deeming it too long to publish, printed a précis and introduced it with a detailed and balanced account of Bertier's claims and the evidence against them.) Bertier continued throughout the controversy to maintain that the notion of physical attraction violates the religious principle that God is the first mover. But he did not accuse Newton of holding a heretical opinion—for Newton subscribed only to "hypothetical attraction." See Bertier, "Jugement des physiiciens impartiaux et sans passion, sur dix-sept expériences . . . qui prouvent un excès de pesanteur des corps supérieurs sur les inférieurs," *Observations sur la Physique, sur l'Histoire Naturelle et sur les Arts et Métiers*, Apr. 1775, pp. 306–313, on p. 309. This periodical was often called the *Journal de Physique*.

<sup>33</sup> For the medical works see Jean-Pierre David, *Dissertation sur ce qu'il convient de faire pour diminuer ou supprimer le lait des femmes* (Paris, 1763); David, *Dissertatio de sectione caesarea* (Paris, 1764); and David, *Mémoire sur la manière d'ouvrir et de traiter les abcès dans toutes les parties du corps* (crowned by the Paris Academy of Surgery in 1764). The anti-Newtonian work is David, *Dissertation sur la figure de la terre, ou l'on tache de prouver . . . d'après les expériences mêmes faites au Perou & au cercle polaire, que cette planète est allongée par les poles* (La Haye, 1769). Two years earlier, David had published his *Dissertation sur la cause de la pesanteur* (Amsterdam, 1767).





**Figure 1.** Some hypothetical earths: perfectly spherical (A or C), elongated at the poles (B), or flattened at the poles (D). From Jean-Pierre David, *Dissertation sur la figure de la terre* (La Haye, 1769). (*History of Science Collections, University of Oklahoma Libraries.*)

in length as we move toward the equator. Now, the academicians found by observation that the length of the degree *diminishes* toward the equator. This, according to David, is the reverse of what would be observed on a flattened, Newtonian earth. Thus, the earth must be elongated at the poles: on an elongated earth (B), the length of the degree diminishes as one moves toward the equator.

David's mistake was in confounding the local vertical with a line drawn through the center of the earth. The local vertical—that is, the direction in which a plumb line hangs—is everywhere perpendicular to the tangent to the earth's surface. This is not the same as the line drawn to the center of the earth—except at the pole and the equator, where the two lines do coincide. Let  $ds$  represent a small distance traveled along a meridian of the earth. Let  $d\phi$  represent the resulting angular displacement, measured in David's way:  $d\phi$  is the angle subtended by the endpoints of the arc, as viewed from the center of the earth. Then, indeed,  $ds/d\phi$  does increase toward the equator on a flattened earth, just as David says. But the quantity relevant to the meridian surveys is actually  $ds/d\theta$ , where  $d\theta$  is the change in the direction of the local vertical as we travel a distance  $ds$  along the meridian.

On the flattened earth,  $ds/d\theta$  decreases as we approach the equator: the length of the degree is smaller at the equator, just as the academicians found.<sup>34</sup>

David was not unaware that he was using a different definition of the vertical than had the academicians. Indeed, he explicitly stated that a plumb line everywhere points to the center of the earth and does not lie on the perpendicular to the local tangent plane. As evidence, he pointed to the existence of ocean currents, which could not exist if the ocean surface were at right angles to the direction of the plumb line. David well knew that to reopen this question of the shape of the earth, believed to have been infallibly settled, was “to risk giving oneself over to ridicule among the *çavants*.”<sup>35</sup>

In May 1769 David’s book was reviewed by an anonymous writer in *Mercur de France*. This notice caught the eye of a leading Newtonian—no less than Charles-Marie de La Condamine, who was astonished to see a new book by an unknown writer on a question he regarded as already decisively answered. La Condamine had been a member of the expedition dispatched to Peru by the Academy of Sciences to help determine the figure of the earth, and in 1751 he had published a report of the survey. La Condamine fired off a letter to *Mercur de France* on 2 May. It was published in the June issue.<sup>36</sup>

David had written high praises of La Condamine’s observational work (although disagreeing with his conclusions). La Condamine began by expressing his affliction at reading praises of himself in a work that accused Newton and Huygens of error. It would have been less painful to have been attacked than praised in such a book. La Condamine went on to explain the error in David’s argument: it all reduced to one paralogism, the confounding of the local vertical with the line drawn to the center of the earth. This, said La Condamine, could have been seductive sixty years ago, but now it was easy to recognize the falsity of the argument, “even without being a geometer.”<sup>37</sup>

David wrote a letter in his own defense, but the editor of *Mercur de France* declined to publish it. Instead, and much to David’s consternation, *Mercur de France* published a letter in David’s defense written by . . . La Perrière, a writer whom David had never even heard of. La Perrière took credit for David’s arguments and claimed them as his own, citing his letters of 1761 to *L’Avant-Coureur* as evidence. Denied a voice by the editor of *Mercur de France*, David published his defense of his book in a separate pamphlet, *Reply to the Letter of M. de La Condamine by the Author of the Dissertation on the Figure of the Earth*.<sup>38</sup>

In 1769, Coultaud’s article in the *Journal des Beaux-Arts et des Sciences* suddenly provided David with new evidence for the elongation of the earth. Now it was clear that the weights of objects increase with height—a fact that must certainly have a bearing on the shape of the earth. David launched a second edition of his book. The *Dissertation on*

<sup>34</sup> A meridian of a flattened (Newtonian) earth is excellently approximated by an ellipse with the semi-minor axis  $b$  along the polar axis. Let the eccentricity be denoted  $e$ . Let the latitude  $\phi$  be measured, in David’s way, as the angular distance of a point from the equator, the angle being measured at the center of the earth. Then  $ds/d\phi = b[1 + (e^2/2)\cos^2\phi]$ , which increases as  $\phi$  decreases. Let the astronomical latitude  $\theta$  be defined as the angle between the local vertical and the plane of the equator. Then  $ds/d\theta = b[1 + e^2 - (3e^2/2)\cos^2\theta]$ , which decreases as  $\theta$  decreases.

<sup>35</sup> David, *Dissertation sur la figure de la terre* (cit. n. 33), p. 7.

<sup>36</sup> For the review of David’s book see *Mercur de France*, May 1769, pp. 89–90; for La Condamine’s response see Charles-Marie de La Condamine, “Réponse de M. de la Condamine à la nouvelle dissertation sur la figure de la terre,” *Mercur France*, June 1769, pp. 201–206. For the South American survey see La Condamine, *Mesure des trois premiers degrés du méridien dans l’hémisphère australe* (Paris, 1751).

<sup>37</sup> La Condamine, “Réponse de M. de la Condamine,” p. 202.

<sup>38</sup> J.-C.-F. de La Perrière, “Réponse à l’écrit de M. de la Condamine, sur la figure de la terre . . .,” *Mercur France*, Aug. 1769, pp. 197–202; and Jean-Pierre David, *Replique à la lettre de M. de la Condamine par l’auteur de la Dissertation sur la figure de la terre* (La Haye, 1769).

the *Figure of the Earth* was supplemented by La Condamine's letter to *Mercur de France* and by David's reply, previously published as a pamphlet.<sup>39</sup> But now, too, Coultaud's experiments of 1767 and 1768 were explicitly mentioned in the title of the book and incorporated into the argument that the earth is elongated at the poles. Coultaud had shown that a pendulum runs more slowly in a valley bottom than at the top of a mountain. Thus, the closer the clock is to the center of the earth, the more slowly it runs. Richer and the academicians had shown that a pendulum runs more slowly at the equator than at Paris. Thus, points on the equator are nearer to the center of the earth. The earth is elongated, just as David had always claimed. Moreover, said David, one could not have the least suspicion of Coultaud, since Coultaud had begun as a partisan of attraction and a believer in the flattened earth of Newton.

### LE SAGE'S DETECTIVE WORK

Georges-Louis Le Sage (1724–1803) of Geneva is best known as the inventor of a mechanical explanation of Newton's law of gravitation. For this reason, he is sometimes wrongly characterized as an anti-Newtonian. In fact, he was a dedicated Newtonian of the sort who still believed in the desirability of finding a mechanical system from which Newton's law of gravitation might be deduced. In this he was a little unusual among the Newtonians of his own generation, who had turned their backs on systems, but not very different from many physicists of the older generation, such as Daniel Bernoulli and Leonhard Euler, neither of whom felt any attraction to Le Sage's system, but both of whom felt considerable affinity for his goal.<sup>40</sup>

Le Sage never held an academic position and was too timid to compete for a chair at the Academy of Geneva. He made ends meet by tutoring in mathematics and physics and by living frugally. Le Sage wrote much but published little: mostly he wrote and rewrote his system of gravity. The basic idea is that the universe is filled with ultramundane corpuscles, traveling at great speeds in all directions. These corpuscles, which are perfectly inelastic, mostly pass right through gross material objects without effect. But a small proportion of them are blocked and brought to rest. Two gross objects therefore stand in each other's shadow, as it were: each slightly screens the other from the rain of corpuscles. The two objects are therefore pushed together. With the right auxiliary assumptions, one can deduce an effective force proportional to the product of the masses of the two objects and inversely proportional to the square of the distance between them. Le Sage's main way of promoting his system was personal correspondence. He was a prolific letter writer

<sup>39</sup> Jean-Pierre David, *Dissertation sur la figure de la terre . . . Nouvelle édition, augmentée d'une lettre de M. de la Condamine, & d'une réplique à cette lettre, dans laquelle on expose plusieurs faits probatoires de l'opinion de l'auteur; entre autres un précis & un résultat des ingénieuses expériences faites aux Alpes en 1767 & 1768 . . .* (La Haye, 1771).

<sup>40</sup> The most readable account of the system published during Le Sage's life is Georges-Louis Le Sage, "Lucrèce Newtonien," *Nouveaux Mémoires de l'Académie Royale des Sciences et Belles-Lettres*, for 1782 (Berlin, 1784), pp. 404–432. But the most systematic and best developed treatment of the theory is the posthumous treatise, *Physique mécanique*, which was pieced together from Le Sage's voluminous and haphazard notes by his former student Pierre Prevost. See Pierre Prevost, *Deux traités de physique mécanique* (Geneva/Paris, 1818). A useful short account, which includes a discussion of the nineteenth-century revival of Le Sage's theory, is Samuel Aronson, "The Gravitational Theory of Georges-Louis Le Sage," *Natural Philosopher*, 1964, 3:51–74. The best biography of Le Sage is Pierre Prevost, *Notice de la vie et des écrits de George-Louis Le Sage de Genève* (Geneva, 1805). For the opinions of Bernoulli and Euler about Le Sage's system see Daniel Bernoulli to Georges-Louis Le Sage, 28 Mar. 1761, 15 Apr. 1767, Bibliothèque Publique et Universitaire de Genève (BPU), MS Suppl. 512, fols. 70, 72; and Leonhard Euler to Le Sage, 16 Apr. 1763, quoted in Prevost, *Notice de la vie*, pp. 382–386.

and was in continual correspondence with any mathematician, philosopher, physicist, or astronomer who would write back. In 1761, the Paris Academy of Sciences named him a *correspondant* of Lalande.

Le Sage published a paper in the *Journal des Beaux-Arts et des Sciences* on the experiments of Coultaud and Mercier in which he tried to show that the experimental results could be made consistent with Newton's law of gravitation. Le Sage had been concerned that no one had yet given an argument sufficient "to entirely close the mouths of the superficial and obstinate detractors of universal gravitation."<sup>41</sup> In his first paper, Le Sage examined the attraction due to various pyramidal shapes.

But as Le Sage reflected further on the experiments of Coultaud and Mercier he became suspicious, and he began to investigate their particulars. Increasingly detailed inquiries failed to turn up any information about these two mountaineering physicists. In this detective work, Le Sage was greatly assisted by his friend Jean-André Deluc. Deluc had made a reputation with his efforts to improve the thermometer, barometer, and hygrometer. His *Researches on the Modifications of the Atmosphere* presented the results of extensive investigations of the variation of barometric pressure with height.<sup>42</sup> With his brother, Deluc had climbed in the Alps of Faucigny to boil thermometers and take barometer readings at the summits, to the astonished amusement of their guides. In short, Deluc was a perfect choice for detective—he knew the physics of instruments, was in sounder health than Le Sage, and knew the ground intimately.

Coultaud claimed to have retired to his family home at Samoëns and to have performed his experiment on a mountain near there. In August and September 1772, Deluc traveled through the area and interviewed many of the leading citizens. No one at Samoëns knew anything of any experiments made in the vicinity, except for those of Deluc himself some years before. The experiment described by Coultaud would have been a fairly public thing. It involved the construction of a cabin at a high altitude, the months-long residence of an observer and fire-tender in the cabin, and midnight signals by gunshot and powder flashes. Such goings-on could hardly take place near a small town without anyone taking note of them. As for Jean Coultaud himself, there was no one of that name in Samoëns. There was, to be sure, one person called Coultaud or Couteau, but his baptismal name was François, and he was a carpenter who had never studied physics. Jean Coultaud had claimed to have been helped by his friend Andrier, "an educated man." But in Samoëns and its vicinity, the only educated men by that name were a medical doctor and his brother, an architect. Doctor Andrier told Deluc that neither he nor his brother had participated in any such experiment.

Coultaud claimed to have ordered the construction of his pendulum clocks at the workshop of "one of the most able clockmakers of Geneva." Nothing was easier for Le Sage than to go around to the clockmakers. No one knew anything about Coultaud or his order for two clocks on the design of Julien Leroy, or any clocks ordered by anyone in Faucigny.

Moreover, Deluc pointed out that the average height of the barometer column at Samoëns reported by Coultaud in his article was too great by an entire inch, as he knew from his own measurements. But nothing more revealed the *maladresse* of Coultaud than his

<sup>41</sup> Georges-Louis Le Sage, "Solution des doutes de MM. Coultaud & Mercier contre la loi Newtonienne de la pesanteur . . .," *J. Beaux-Arts Sci.*, Apr. 1772, pp. 34–62, on p. 36.

<sup>42</sup> For the detective work on Coultaud and Mercier see Georges-Louis Le Sage, "Lettre de M. Lesage," *Observ. Phys.*, Apr. 1773, pp. 250–260. On Jean-André Deluc (1727–1817), of Geneva, see Jacques Trembley, ed., *Les savants genevois dans l'Europe intellectuelle du XVIIIe au milieu de XIXe siècle* (Geneva: Editions du Journal de Genève). Deluc's work on the variation of barometric pressure with height was described in *Recherches sur les modifications de l'atmosphère* (Geneva, 1772).

claim that the density of the air does not vary with height. The decrease in the density of the atmosphere with height was a long-established fact, well confirmed and explored in considerable detail by Deluc, among others.

Finally, Jean Coultaud had called himself “former professor of physics at Turin.” But the professor of botany at Turin told Deluc that there was not and never had been any professor of that name at Turin.

Mercier, the author of the second article on pendulum experiments published in the *Journal des Beaux-Arts et des Sciences*, turned out to be no less shadowy. Mercier had cast his article in the form of an open letter to Professor Gessner of the University of Zurich, with whom he claimed to have previously corresponded. Mercier claimed to have performed his experiments in the mountains near Sion, in Valais. But Gessner informed Le Sage that he did not know anyone named Mercier who was a physicist, that he did not know any physicist in Valais, that he had never received a letter dated at Sion and signed by Mercier, that he never read the *Journal des Beaux-Arts et des Sciences*, and that he had been completely unaware of Mercier’s article until Le Sage had asked him about it.

Le Sage had a relative who was magistrate of Vevey, near Valais. This relative made inquiries of a former grand bailiff of Valais, of the current burgomaster of Sion, and of several other persons. No one knew of any Mercier in Sion or its environs, and no one knew anything of the slightest experiment made in the mountains near Sion. Le Sage had an English friend who had passed some time in Sion and had become friendly with the doctor and pharmacist of Sion, a certain Doctor Naterer. At Le Sage’s request, his friend inquired of Naterer. The result was the same.

Finally Le Sage remarked that in Mercier’s article there reigned a profound silence around the traits that might have lent it some authenticity. Mercier spoke of “trigonometric operations” that he made to establish the elevations of his stations, but he did not even name the mountain on which the experiment had been conducted. And, although Mercier said that he had been aided by one of his relatives and by a certain Captain Muller, he did not name any witness who was known in the district or who could be identified. Le Sage concluded that Coultaud and Mercier were two impostors, *impulsionnaires*, who believed that, in the effort to banish immaterial virtues from physics, everything was permitted.

As Le Sage was gathering his evidence, he informed Bertier through a mutual acquaintance of what was coming. But Le Sage was slow. He had proofs of fraud, but he had not finished writing them down. Bertier complained in print that Le Sage alleged fraud but would not provide a complete account. Finally, when Le Sage was ready, he sent his article not to the *Journal des Beaux-Arts et des Sciences* but to a new Parisian journal with a better reputation, the abbé J.-B.-F. Rozier’s *Observations sur la Physique*.<sup>43</sup>

#### THE ROLE OF THE PERIODICAL PRESS: A TALE OF TWO JOURNALS

The mid-eighteenth century saw a rapid proliferation of popular reviews, many of which proved to be ephemeral. The new journals had large appetites for material, and many had encyclopedic tastes. The material published in the *Journal des Beaux-Arts et des Sciences* was characteristic. The main focus was literary, but there were also articles on music, theater, politics, and agriculture, as well as the sciences.

Coultaud and Mercier (whoever they were) sent their papers to the *Journal des Beaux-Arts et des Sciences* because there they were sure of a sympathetic reading. What was the

<sup>43</sup> Bertier, “Lettre du P. Bertier” (cit. n. 30); and Le Sage, “Lettre de M. Lesage” (cit. n. 42).





**Figure 2.** Jean-Louis Aubert, the editor of the *Journal des Beaux-Arts et des Sciences* during the Coultaud-Mercier affair. (Courtesy of Bibliothèque Nationale, Paris.)

*Journal*, and who was its editor? The life span of the *Journal des Beaux-Arts et des Sciences* nearly coincided with the Coultaud-Mercier controversy: the first number appeared in 1768 and the last in 1775. However, the *Journal* had considerably older roots. It was, in fact, the continuation of the Jesuit *Journal de Trévoux*, now in non-Jesuit hands.

Its editor, the abbé Jean-Louis Aubert (1731–1814), was a literary hack known for the acidity of his criticism, for his opposition to “the false philosophy,” and for his prudence in political matters (see Figure 2). His political conservatism was often and amply rewarded by the intervention of noble patrons who helped advance his career. As a youth, Aubert entered the seminary and was tonsured, but he was attracted finally to a literary career rather than to the priesthood. This career had a doubtful debut in 1751 when, at the age of twenty, Aubert took up the editorship of *Annonces, Affiches et Avis Divers*, a semiweekly often referred to as *Affiches de Paris*. Eight pages appeared every Monday and Thursday, with advertisements of land and houses for sale or rent, as well as announcements of concerts and plays. All these notices were inserted free of charge. To this fare of workaday advertisements Aubert added a new section of book reviews and literary news, which became known for “articles full of malice, of taste, and of erudition.”<sup>44</sup>

<sup>44</sup> Eugène Hatin, *Bibliographie historique et critique de la presse périodique française* (Paris, 1866), p. 19. For Aubert’s opposition to “the false philosophy” see the introduction to Jean-Louis Aubert, *Fables nouvelles*



Aubert assumed direction of the *Journal de Trévoux* in 1767. After the suppression of the Jesuits in 1762, the *Journal* had staggered onward, suffering badly under two mediocre editors in just four years. When Aubert took over, as the third editor after Berthier, he changed the name of the journal, revised its editorial policies, improved its sloppy typography, and launched an advertising campaign.<sup>45</sup> All of this was an effort to make the newly named *Journal des Beaux-Arts et des Sciences* more up-to-date and more appealing to a broader audience.

The young Aubert's book reviews in *Affiches de Paris* had won him a reputation for erudite maliciousness. But by the time he assumed the directorship of the *Journal de Trévoux* he was in his mid thirties and his career was maturing nicely. Aubert had mellowed. P. C. Sommervogel describes him at this stage of his career as "a rather ordinary literary man," who "aspired only to a peaceful existence."<sup>46</sup> Because he was not a Jesuit, and because two other editors separated his tenure from the stormy last days of Berthier's editorship, Aubert was able to avoid restarting the squabble with the philosophes.

Nevertheless, the *Journal des Beaux-Arts et des Sciences* was socially, artistically, and politically conservative. Although its editor espoused neutrality and impartiality in scientific disputes, his journal was much more favorably disposed than many others to print anti-Newtonian views. In attempting to rehabilitate a journal that had fallen on hard times, Aubert must have enjoyed the attention that this controversy attracted: after all, he did make Coutauld's article the opening piece of its issue. He was also hard up for material.<sup>47</sup> Certainly, he allowed the participants in this controversy many more pages than did any other editor of a general-interest periodical.

Father Bertier also sent his first papers on weight to the *Journal des Beaux-Arts et des Sciences*. As we have seen, Bertier had old connections with the journal, having been an intimate friend of the former editor, G.-F. Berthier. Bertier, more than any other participant in the affair, also used and abused the eclecticism of the popular press. He fired off paper after paper—many of them scarcely distinguishable—to every publication that would grant him a few pages: the *Journal Encyclopédique, Suite de la Clef, Journal de Politique et de Littérature*, as well as to the *Journal des Beaux-Arts et des Sciences, Journal des Sçavans*, and *Observations sur la Physique*.

Midcentury also saw the birth of a number of independent scientific periodicals, among them *Observations sur la Physique*. The new scientific journals served a definite need. The old academic journals were too restrictive and too slow. The pages of the *Mémoires* of the Academy of Sciences were controlled by the members of the Academy. No doubt this kept standards up, but it also stifled the expression of heterodox views. Moreover, in the case of the *Mémoires*, there was often a lag of several years between the nominal year and the actual date of publication. For both these reasons, the academic journals were unsatisfactory for the resolution of scientific controversies. Indeed, the *Mémoires* of the

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(Amsterdam, 1756). The biographical material on Aubert is drawn largely from the article by Béatrice Fournel in Jean Sgard, ed., *Dictionnaire des journalistes (1600–1789)* (Grenoble: Presses Univ. Grenoble, 1976), p. 17. See also M. Prevost, ed., *Dictionnaire de biographie française*, Vol. 4 (Paris: Letouzey & Ané, 1948), p. 41. For the *Affiches de Paris*—and for all the other periodicals mentioned here—see Sgard, ed., *Dictionnaire des journaux* (cit. n. 26).

<sup>45</sup> A leaflet advertising the renamed journal is preserved in the Collection Anisson-Dupperon sur la Librairie et l'Imprimerie, BN, MS Fr. 22085.

<sup>46</sup> P. C. Sommervogel, *Table méthodique des Mémoires de Trévoux*, 2 vols. (Paris, 1864; Geneva: Slatkine Reprints, 1969), Vol. 1, p. xciii.

<sup>47</sup> On the decline of the *Journal de Trévoux* after 1762 and its difficulty in attracting good material see Pappas, *Berthier's Journal de Trévoux* (cit. n. 26), pp. 32–35.

Academy of Sciences played no part in the Coultaud-Mercier affair. Although d'Alembert made calculations addressing Coultaud's claims, and actually read the results of these calculations at the Academy, these papers were never printed in the *Mémoires*. D'Alembert and Lalande had to send communications to other periodicals in order to participate in the public debate. Rozier, the editor of *Observations sur la Physique*, pointed specifically to the long publication delays of the academic journals—as well as to their expense—in explaining the need for his new one.<sup>48</sup>

The chronological list of publications in the Appendix illustrates the general trajectory of the debate. The affair remained in the pages of the *Journal des Beaux-Arts et des Sciences* for several years, then spilled over into the newly founded and more prestigious *Observations sur la Physique*, where it was decisively settled. *Observations sur la Physique* functioned well, and more or less as its founder, Rozier, had intended. It facilitated the discussion and resolution of conflicting scientific claims. However, it is doubtful that the controversy would ever have reached such proportions without the willing participation—whether through naïveté or anti-Newtonian sentiment—of the popular press, and particularly of Jean-Louis Aubert and the *Journal des Beaux-Arts et des Sciences*.

### THE SECOND WAVE: BERTIER'S EXPERIMENTS

The experiments of Coultaud and Mercier had been revealed as frauds. Le Sage's proofs were accepted by Rozier (the editor of *Observations sur la Physique*), by Aubert (the editor of the *Journal des Beaux-Arts et des Sciences*), and even by such thoroughly unrepentant anti-Newtonians as La Perrière. The experimental evidence against Newton's principles had been eliminated. All that remained were Bertier's incomprehensible and incompetent thought experiments.

The situation changed in September 1773, when Bertier announced the results of a new kind of experiment.<sup>49</sup> In the vault of the Church of the Oratory in Paris, he placed an equal-arm balance 75 feet above the floor. In one pan (the left one, say), he placed a weight. In the right pan, he placed a rolled-up rope and enough additional weight to achieve a balance. Then he suspended the right-hand weight 74 feet below its pan by means of the rope. The objects supported by the two pans were the same as before. But now the left-hand pan sank: *objects weighed more when they were higher up*.

The same experiment was reported again and again by Bertier, in publications scattered through the scientific and the popular press.<sup>50</sup> At first the results were purely qualitative. That is, Bertier reported the weights he had used (3, 6, 12, or 31 pounds) and the length of the rope, but he did not measure the amount by which the upper weight overbalanced the lower one. However, he did claim that the heavier were the weights, the larger was the effect.

One of the first criticisms of these results came from La Perrière, who clung as ever to his own crazy system, but who was also a thoughtful analyzer of experiments. In a paper

<sup>48</sup> J.-B.-F. Rozier, "Avis," *Observ. Phys.*, Apr. 1773, pp. iii–vii.

<sup>49</sup> Joseph-Etienne Bertier, "Lettre écrite à l'auteur de ce recueil," *Observ. Phys.*, Sept. 1773, pp. 251–252.

<sup>50</sup> *Ibid.*; Joseph-Etienne Bertier, "Lettre du P. Bertier . . .," *J. Beaux-Arts Sci.*, Oct. 1773, pp. 122–124; Bertier, "Lettre du P. Bertier . . . sur une expérience qu'on peut faire sans frais à chaque instant & qui prouve que les corps pesent d'autant plus qu'ils sont plus élevés . . . jusqu'à une distance non connue," *Suite Clef*, Oct. 1773, pp. 301–302; Bertier, "Seconde lettre du P. Bertier . . .," *J. Beaux-Arts Sci.*, Nov. 1773, pp. 225–227; "Expérience sur la gravité des corps" (cit. n. 32); Bertier, "Lettre sur la pesanteur des corps" (cit. n. 32); and Bertier, "Jugement des physiciens impartiaux et sans passion, sur dix-sept expériences . . . qui prouvent un excès de pesanteur des corps supérieurs sur les inférieurs" (cit. n. 32).

sent to *Observations sur la Physique*, La Perrière argued that the weight of the lower object had not really decreased.<sup>51</sup> Rather, it *appeared* to have decreased because it was plunged into an air that was more dense than the air surrounding the upper weight. By the ordinary law of hydrostatics, the buoyant force on the lower object was greater, because the weight of the air it displaced was greater.

This objection, among others, pushed Bertier to repeat his experiments and to make them quantitative. In the fall of 1774 he returned to his old residence at Montmorency for a short visit. While there, he repeated the experiments, in the vault of the Oratory of Montmorency. After Le Sage's unmasking of the Coultaud-Mercier fraud, testimony was all the more readily doubted. Rozier had criticized Bertier's reports of his first experiments at the Oratory of Paris because Bertier had not supplied the names of witnesses. "Coultaud" and "Mercier," perhaps writing in the mode of the turn of the century, when gentle standing might be enough to assure credibility, had contented themselves with citing "intelligent, active, and educated" men. But now the reliability of witnesses depended not so much on their social standing as on their special training. Thus Bertier performed the new round of experiments in the presence of seven Oratorians, "all physicists"—though he stretched the definition to include those who taught Cartesian physics to adolescents. Added credibility came from producing a witness with an academic affiliation, who might therefore be known outside the local district. Thus Bertier mentioned by name Father Louis Cotte, Oratorian of Montmorency and *correspondant* of the Academy of Sciences. The escalating controversy also called for escalations in weight. A weight of 150 pounds was put on one side of the balance. On the other side were placed weights and a rope totaling 150 pounds. When the weights were let down to within a foot of the pavement, they were overbalanced by the upper weight by a bit more than 2 pounds.<sup>52</sup>

Le Sage, who had revealed the Coultaud-Mercier fraud, contributed a paper to *Observations sur la Physique* in response to Bertier's claims. First of all, Le Sage criticized Bertier for not having done his literature search. Le Sage cited experiments of exactly the same type, which had been performed a century before by Robert Hooke and other members of the Royal Society of London at Westminster Abbey, at St. Paul's Cathedral, and at other places—often (though not always) with a null result. Le Sage also pointed out that Bertier's reported weight differences were some 940 times larger than they should be under his assumption that the effect was proportional to the difference in height: the heights used by Bertier were tiny fractions of the radius of the earth. In short, Bertier's claims were as easy to believe as the proposition that a pear gets heavier when it is peeled. What then to make of them? "A formal exception to the most constant laws of Nature is called a Miracle. Or, rather, I would say that, without doubt, the same Pseudonyms who in 1769 and 1771 related some pretended experiments tending toward the same goal have been pleased to impose on us once again, under a better known name than those they had borrowed before."<sup>53</sup> This joking suggestion that the forgers of the papers by "Coultaud"

<sup>51</sup> J.-C.-F. de La Perrière, "Observations sur l'expérience du Pere Bertier . . .," *Observ. Phys.*, Nov. 1773, pp. 374–378.

<sup>52</sup> Bertier, "Lettre sur la pesanteur des corps" (cit. n. 32), p. 385. On changing views of the reliability of witnesses see Steven Shapin, *A Social History of Truth* (Chicago: Univ. Chicago Press, 1994), pp. 74–86.

<sup>53</sup> Georges-Louis Le Sage, "Reflexions sur une nouvelle expérience du révérend Pere Bertier . . .," *Observ. Phys.*, Nov. 1773, pp. 378–381, on pp. 380–381. On 3 Dec. 1662 a paper by Henry Power (1623–1668) was read at the Royal Society, describing experiments made in a coal mine. A weight suspended from one side of a balance by a string 68 yards long appeared to lose an ounce on the pound. See Thomas Birch, *The History of the Royal Society of London*, 4 vols. (London, 1756–1757), Vol. 1, pp. 133–134. Similar experiments were reported, by Hooke and others, over the next two years with more ambiguous results. See *ibid.*, Vol. 1, pp. 163–165, 433, 465–466, Vol. 2, pp. 66, 69–72.

and “Mercier” had also written the articles published under Bertier’s name was the closest that Le Sage came to accusing Bertier of dishonesty. It is clear, though, that Le Sage doubted Bertier’s good faith. In this he was not alone. The discussion of Bertier’s experiments was marked by rapidly rising tempers.

Bertier composed a lengthy reply to Le Sage’s paper and sent it to Abbé Rozier, the editor of *Observations sur la Physique*. Rozier sent it back, asking him to shorten it to four pages or else to give it to some other journal since, as Bertier claimed, other editors had asked for it. On the day of the public ceremonies marking the return of the Academy of Sciences after Easter, Rozier and Bertier encountered one another in the courtyard of the Louvre. Rozier told Bertier directly that he would not publish his reply to Le Sage, on the grounds that it was too long and that it merely repeated what Bertier had already said in the fourth volume of the *Physical Principles*. Then, according to Rozier, Bertier became very angry. Finally, after calming himself, Bertier said that he would force Rozier to print it by means of orders from superiors. Rozier reported this whole encounter in print and then scolded Bertier for threatening to bring political influence to bear on his journal: “Pere Bertier, to introduce into physics *lettres de cachet* for attraction, for centrifugal or centripetal force! *Ce n’est pas bien*.”<sup>54</sup>

Because Bertier’s experiment was easy to perform and bore on an important question, it was soon repeated by investigators all over France:

1774: David, in the Church of St. Ouen, Rouen  
 Jean Adam, in the Church of the Holy Sepulchre, Caen  
 Rozier, in the dome of the Invalides, Paris.<sup>55</sup>

1775: Mathieu Tillet, in Paris  
 A commission of the Academy of Dijon, in the Church of St. Bénigne, Dijon  
 D.-S.-G.-T. de Gratet de Dolomieu, in the mines of Montrelay, Brittany.<sup>56</sup>

One of the first attempts to confirm Bertier’s result was made by Jean-Pierre David, the medical doctor and anti-Newtonian of Rouen. David placed his balance 170 feet above the pavement in the church of St. Ouen. He confirmed Bertier’s effect; that is, the upper mass appeared to weigh more. But the size of the effect measured by David was vastly smaller than Bertier’s result. David claimed an increase of weight for the upper mass of 1

<sup>54</sup> J.-B.-F. Rozier, “Observations sur la lettre du Pere Bertier . . . à l’auteur du Journal de Politique et de Littérature,” *Observ. Phys.*, Dec. 1774, pp. 456–463, on p. 457. This is Rozier’s response to Bertier, “Lettre du Père Bertier . . .,” *Journal de Politique et de Littérature, Contenant les Principaux Evénements de Toutes les Cours; les Nouvelles de la République des Lettres, etc.*, 25 Nov. 1774, pp. 154–155.

<sup>55</sup> “Académie Royale des Sciences, Belles-Lettres & Arts de Rouen: Séance publique du mercredi 3 août 1774 pour la partie des sciences,” *Observ. Phys.*, Oct. 1774, pp. 340–341 (the section of this journal called “Nouvelles Littéraires” included accounts of the public sessions of the various academies; a synopsis of David’s report on his experiments is item 4 of the literary news from Rouen in this number); Jean-Pierre David, “Lettre . . . sur la pesanteur des corps,” *ibid.*, Feb. 1775, pp. 129–139; Jean Adam, “Lettre à l’auteur de ce journal,” *J. Polit. Litt.*, 15 Dec. 1774; and Rozier, “Observations sur la lettre du Pere Bertier” (cit. n. 54). Jean Adam (1726–1795) was a doctor of theology and canon at the Church of the Holy Sepulchre in Caen. He also held the chair of philosophy at the University of Caen.

<sup>56</sup> Bertier, “Jugement des physiiciens impartiaux et sans passion, sur dix-sept expériences . . . qui prouvent un excès de pesanteur des corps supérieurs sur les inférieurs” (cit. n. 32), p. 307 (citing Tillet); [Commission of the Academy of Dijon], “Mémoire dans lequel on indique les causes qui peuvent changer accidentellement les effets apparens de la pesanteur des corps à des hauteurs inégales, lu à l’Académie de Dijon,” *Observ. Phys.*, Apr. 1775, pp. 314–326; and Dieudonné-Sylvain-Guy-Tancrede de Gratet de Dolomieu, “Expériences sur la pesanteur des corps à différentes distances du centre de la terre, faites aux mines de Montrelay en Bretagne,” *ibid.*, July 1775, pp. 1–5. This was Dolomieu’s first scientific publication.

*gros* for 175 *livres*. In another trial, he measured an increase of 1 *once* for 1,120 *livres*.<sup>57</sup> Thus, David was getting fractional weight increases of about  $3 \times 10^{-7}$  per foot of height difference. Bertier claimed to have measured effects larger than this by two or three orders of magnitude.

In his next paper, David agreed with Le Sage that Bertier's weight differences were too large to be believed. He pointed out that Bertier's balances were not sensitive enough to permit the measurement of the weight differences one might expect if the weight varied in proportion to the distance from the center of the earth. But the same objection could be made to the balances used by Hooke and the other English experimenters a century before. So the ambiguous and even the null results of Hooke and his collaborators need not be taken too seriously. According to David, Bertier's method—the direct measurement of weight differences—could not be made adequate. The only reliable method was the use of pendulum clocks, by means of which small differences could be accumulated over time. "This, at least, is what was perceived by MM. Coultaud and Mercier, whom Le Sage treats as *romanciers*." According to David, Coultaud and Mercier deserved credit for pointing the right way to a resolution of one of the greatest questions of physics. Where David lived, there was hardly a hill of 100 *toises* elevation. So he concluded by directly challenging Le Sage, who lived in Geneva within sight of the Alps: "I would not have failed to verify these experiments if I had found myself in a situation as favorable as M. Le Sage." But David knew Le Sage would not do it: undertaking such experiments would presuppose doubts in need of clarification, "and M. Le Sage has none on this point."<sup>58</sup>

The most heroic repetitions of Bertier's experiment were certainly those that Dolomieu performed in May 1775 in the shafts of two coal mines at Montrelay, in Brittany. The shafts had depths of 342 and 570 feet, and the weights suspended varied from 50 to 150 pounds. In a series of ten trials (halted finally when the wire broke) the differences in nominal weight required for equilibrium varied from 2 to 14½ *onces*. But there was no consistency. In four of the ten cases, the equilibrium was achieved with the larger nominal weight in the upper pan, which was contrary to Bertier's results. In the other six cases, the sign of the difference was consistent with Bertier's results. But even in these cases, there appeared to be no proportionality between the weight used and the apparent difference in weight that was measured. What was the origin of these apparent differences in weight? Dolomieu concluded, "It is surely some accident or some causes which have not been perceived, but which are independent of weight."<sup>59</sup>

The most careful and extensive investigations were those conducted by a committee of the Academy of Dijon. Rozier's assault on Bertier's credibility and his account of his own experiments in the dome of the Invalides were discussed at a meeting of the Academy of Dijon on 5 January 1775.<sup>60</sup> Those present resolved to undertake an investigation of all the causes that might produce apparent changes in weight and appointed a committee of five to do the work and prepare a report. The experiments were performed in one of the towers

<sup>57</sup> "Académie Royale des Sciences, Belles-Lettres & Arts de Rouen: Séance publique du mercredi 3 août 1774 pour la partie des sciences" (cit. n. 55). The *livre de Paris*, or pound, was divided into 16 *onces*, each of which was divided into 8 *gros*, each of which contained 72 *grains*. One *livre* was roughly 489.5 grams. See Zupko, *Revolution in Measurement* (cit. n. 5), pp. 333, 335, 346, 356.

<sup>58</sup> Jean-Pierre David, "Réponse aux réflexions de M. le Sage . . . sur les expériences du R. P. Bertier . . ." *Observ. Phys.*, Dec. 1774, pp. 433–443, on pp. 436, 443.

<sup>59</sup> Dolomieu, "Expériences sur la pesanteur des corps à différentes distances du centre de la terre, faites aux mines de Montrelay en Bretagne" (cit. n. 56), p. 5.

<sup>60</sup> [Commission of the Academy of Dijon], "Mémoire dans lequel on indique les causes qui peuvent changer accidentellement les effets apparens de la pesanteur des corps à des hauteurs inégales" (cit. n. 56). The academy had discussed Rozier, "Observations sur la lettre du Pere Bertier" (cit. n. 54).



of the church of St. Bénigne in Dijon. The balance was placed on the floor of the belfry, 121½ feet above the pavement. The balance could handle a weight of 250 pounds with a sensitivity of ½ *gros*. That is, when the weights in opposing pans were balanced, the addition of ½ *gros* to one pan would cause an obvious shift in the balance. The measured differences in apparent weight of the upper and lower masses were very small and were often inconsistent. The experimenters noted a number of complicating factors. For example, the weight of the rope increased on foggy days. They also noted, as had others before them, that the apparent difference in weight between the upper and lower masses was smaller when a thin iron wire was used in place of the more voluminous rope. The experimenters placed thermometers and barometers high and low in the church tower. They also attempted direct measurements of the density of the air by evacuating glass containers, opening them at each of the two platforms, and weighing them. The air in the upper part of the tower weighed measurably less per unit volume. But calculations showed that the measurements of temperature, pressure, and density were not mutually consistent. It was no wonder, the experimenters noted, that the leading authorities disagreed so widely about the density of the atmosphere. The clearest indication of the importance of the variation in the density of the air came when the committee replaced the iron weights with much more voluminous wooden ones. Then the apparent increase of weight with height became large enough to be easily and repeatably measured, at about 7 *gros* for 211 pounds.

#### BERTIER'S RETRACTION

Among the half dozen experimental groups reporting results, Bertier had not a single supporter. Even those who had detected some effect found it vastly smaller and far more unstable than Bertier had claimed. In 1776 reports of new gravity experiments from Great Britain made Bertier's cause completely hopeless. In 1772 Nevil Maskelyne, the astronomer royal, had proposed to the Royal Society of London that it seek to measure the attraction due to a single mountain or hill. Plumb lines placed just north of and just south of the same mountain should reveal an apparent deflection of the zenith, owing to the gravitational attraction of the mountain on the bob of the plumb line. This deflection could be revealed by measuring the zenith distances of stars at their meridian passages at the two stations. If the difference between the zenith distances measured at the two stations exceeded the difference attributable to the latitude difference, the discrepancy could be assigned to the attraction of the mountain. Of course, this was not a new idea. Newton had suggested that the largest mountains might deflect a plumb line by as much as 2 minutes.<sup>61</sup> Bouguer and La Condamine had actually attempted to make such measurements during the French expedition to Peru. But Bouguer's results had been ambiguous. He had, indeed, reported a deflection of the vertical due to Mt. Chimborazo (Chimborazo), but the deflection was much less than expected, and the measurements showed a great deal of scatter.

Maskelyne's measurements were carried out in the summer of 1774, near a mountain called Schehallien (now Schiehallion), in Perthshire, in the center of Scotland. The results

<sup>61</sup> Nevil Maskelyne, "A Proposal for Measuring the Attraction of Some Hill in This Kingdom by Astronomical Observations," *Philosophical Transactions* (London), 1775, 65:495–499. On Maskelyne's operations see Derek Howse, *Nevil Maskelyne: The Seaman's Astronomer* (Cambridge: Cambridge Univ. Press, 1989), pp. 129–141. For Newton's suggestion see *Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World*, English trans. of Andrew Motte, revised by Florian Cajori (Berkeley: Univ. California Press, 1934), "The System of the World," sect. 22, p. 570.



were reported in the *Philosophical Transactions* for 1775.<sup>62</sup> Maskelyne measured a deflection of the vertical, attributed to the attraction of the mountain, of about 6 seconds. He argued that the experiment strongly supported the validity of the inverse-square law. For it was only because of the mountain's much greater proximity to the plumb line that it was able to produce any sensible effect in comparison with the attraction of the earth itself. Finally, using a rough estimate of the volume of the mountain, Maskelyne concluded that his results were consistent with an estimate that the surface density of the earth is about half the mean density of the globe.

In November 1775 the Royal Society awarded Maskelyne a medal for this work. At the award ceremony John Pringle, the president of the Royal Society, delivered a speech on the attraction of mountains that summarized Maskelyne's work and set it in the context of the long dispute over the reality of attraction. Pringle's account was translated into French by Jean-Baptiste Leroy and was published in *Observations sur la Physique* in May 1776.<sup>63</sup>

One year later, Father Bertier retracted his claim to have measured an increase of weight with height.<sup>64</sup> He did not mention the English experiments, but he could not have been unaware of them. Leroy's translation of Pringle's speech appeared in the very journal that had been the main forum of discussion of Bertier's claims for the past several years. And it was to the same *Observations sur la Physique* that Bertier sent his retraction. It was, in any case, a very glaring retraction.

Bertier apologized for being so slow to retract, saying that he had been busy with other matters. Then he praised the retraction of erroneous claims as morally more laudable than the publication of correct claims, because it involved the vanquishing of self-love. However, Bertier's retraction was only partial. He withdrew the conclusions he had drawn from his own experiments, but he did not abandon his view that weight does increase with height. In admitting that his experiments were inconclusive, he mentioned the telling result of Rozier's experiments. Rozier (among others) had replaced the voluminous rope with a thin wire and had seen the apparent weight differences go away. Bertier claimed to have duplicated this experimental variation and to have obtained the same result as Rozier. The conclusion seemed inescapable that the apparent weight differences were due to the buoyancy of the air—that is, to the fact that the density of the air diminished with height.

How could Bertier have made such a mistake? He now blamed it all on Jean-André Deluc of Geneva. Deluc had misled him, Bertier insisted, by asserting that the density of the air is more or less constant from the lower to the upper reaches of the atmosphere. This had seemed plausible to Bertier, since the expansive force of the heat at low elevations could be compensated for by the compressive effect of the weight of the upper layers of the atmosphere. In fact, Bertier had not mentioned any hypothesis about the density of the atmosphere in his early papers. He had not really thought about density variation until after the fact, when La Perrière pointed to it as a possible explanation of his results.<sup>65</sup>

<sup>62</sup> Nevil Maskelyne, "An Account of Observations Made on the Mountain Schehallien for Finding Its Attraction," *Phil. Trans.* (London), 1775, 65:500–542.

<sup>63</sup> Jean-Baptiste Leroy, "Lettre du M. Le Roy, de l'Académie Royale des Sciences à l'auteur de ce recueil," *Observ. Phys.*, May (?) 1776, pp. 416–417; and John Pringle, "Discours sur l'attraction des montagnes, prononcé dans l'assemblée annuelle de la Société Royale de Londres du 30 novembre 1775 par le Président M. le Chevalier Baronet, Pringle . . . Traduit par M. Le Roy," *ibid.*, pp. 418–434. In the copy I consulted, the cover sheets and indexes for the individual months were missing, so it was impossible to be certain of the month of publication. The page count seems to indicate May or June. Jean-Baptiste Leroy (ca. 1725–1800), a physicist and member of the Academy of Sciences from 1751, was a son of Julien Leroy, the clockmaker.

<sup>64</sup> Joseph-Etienne Bertier, "Rétraction du Pere Bertier de l'Oratoire, sur la conséquence qu'il a tirée de son expérience d'un corps, pesant plus dans un lieu haut que dans un bas," *Observ. Phys.*, June 1777, pp. 460–466.

<sup>65</sup> La Perrière, "Observations sur l'expérience du Pere Bertier" (cit. n. 51).

Moreover, Deluc was, as we have seen, one of the principal investigators of the variation of air density with height.

Bertier concluded that the whole round of experiments had nothing to say *for or against* the proposition that weight increases with height. The same was true of “the experiment of Samoens in the Alps”—Coultaud’s experiment—for the same density effects would have produced variations in the rates of the pendulum clocks. Bertier now restated his belief, based on certain “information,” in the essential validity of Coultaud’s experiment, though he admitted that the account of the experiment had been “embellished by a man of wit” and was untrue in its details.<sup>66</sup> Bertier finished his “retraction” by offering new proofs from reason that bodies must weigh more the higher they are raised above the surface of the earth, up to some limiting distance.

#### WHO WAS JEAN COULTAUD?

The obvious suspects are the three anti-Newtonians who participated most vigorously in the debate: La Perrière, Bertier, and David. As we have seen, each of these men felt badly abused by the Newtonian establishment. And each made use of Coultaud’s and Mercier’s results in support of his own previously articulated system.

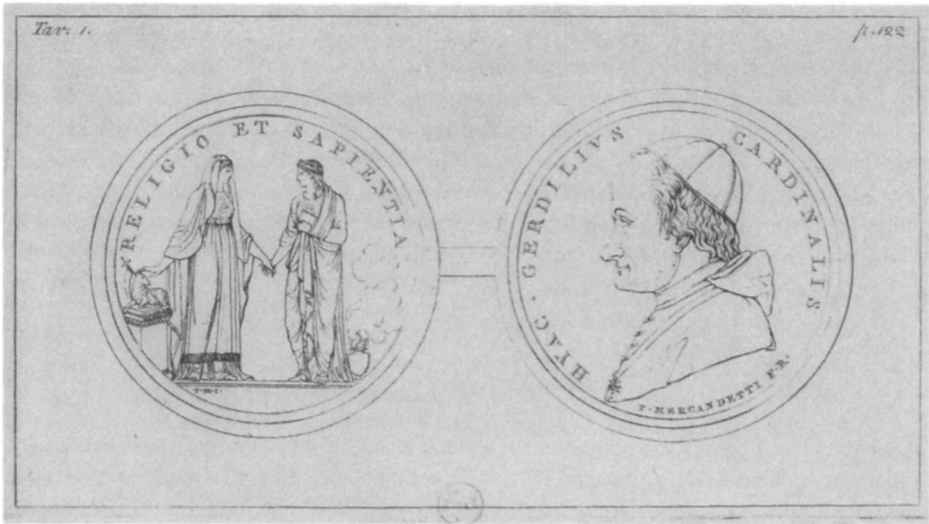
Of the three, La Perrière seems to have had the least to gain from the fraud. In his published responses to the pendulum experiments, La Perrière made very little capital for his own system. La Perrière pointed out that, in principle, the pendulum experiments should be capable of showing whether the weight is smaller or greater on the tops of mountains and, thus, of confirming or refuting Newton’s system of attraction. However, he instead chose to argue that the temperature controls had been inadequate and that the experiments therefore had nothing to say either for or against Newton. La Perrière himself seems to have believed that the weight really stayed the same no matter where the pendulum was placed.

David saw in Coultaud’s experiment confirming evidence that the earth is elongated at the poles, a point David emphasized in his papers, as well as in the second edition of his *Dissertation on the Figure of the Earth*. And, like many other anti-Newtonians, he was on record as believing that the weight of a body increases in proportion to its distance from the center of the earth. But in his own repetition of Bertier’s experiment, David found weight differences that (although smaller than Bertier’s) were a good deal too large to be consistent with Coultaud’s and Mercier’s results.<sup>67</sup> One would think that if David were the author of the fraudulent papers he would at least have made his weight measurements consistent with the pendulum experiments. Moreover, in his correspondence with Le Sage just before the controversy began, as well as in an article he published during the course of the affair, David continued to stress his own personal definition of the vertical—a preoccupation without any obvious echoes in the papers of Coultaud and Mercier.<sup>68</sup>

<sup>66</sup> Bertier, “Rétraction du Pere Bertier de l’Oratoire” (cit. n. 64), p. 462.

<sup>67</sup> For a claim that the weight increases in proportion to a body’s distance from the center of the earth see David, *Dissertation sur la cause de la pesanteur* (cit. n. 33), p. 106. In Mercier’s three trials, the fractional change in clock rate per foot of elevation difference was about  $5 \times 10^{-8}$ . Coultaud’s experiments produced the same result. The implied fractional changes in weight per foot of elevation difference would be twice these (if correctly computed) or equal to these (as mistakenly computed by Coultaud and Mercier). As we have seen, David’s direct measurement of weight differences gave fractional changes in weight per foot of elevation of about  $3 \times 10^{-7}$ . Thus, David’s results are from three to six times too large to be consistent with Coultaud’s and Mercier’s.

<sup>68</sup> Jean-Pierre David to Le Sage, 27 Feb. 1768, BPU, MS Suppl. 512, fols. 212–214; and David, “Lettre . . . sur la pesanteur des corps” (cit. n. 55).



**Figure 3.** Hyacinthe-Sigismond Gerdil, as pictured on a commemorative medallion struck after his death. Was he Jean Coultaud? The motto on the reverse is *Religio et Sapientia*. (Courtesy of Bibliothèque Nationale, Paris.)

Bertier is the most attractive suspect of the three. He was a staunch Cartesian, opposed to Newtonian attraction for philosophical as well as religious reasons. He based the whole fourth volume of his *Physical Principles* on Coultaud's experiment. Ultimately, he confirmed Coultaud's results by means of balance experiments conducted in the Church of the Oratory. Moreover, Bertier's contemporaries doubted his good faith. Le Sage joked that Bertier's papers must have been composed by the same counterfeiters who had invented Coultaud and Mercier. Here Le Sage came as close to accusing Bertier of fabricating his results as good manners would allow. Rozier, the editor of *Observations sur la Physique*, addressed Bertier in print: "Be of good faith, Father Bertier." Rozier reported that in his own repetitions of Bertier's experiment, in the dome of the Invalides, the results were variable. He asked Bertier whether he had reported all of his results, or only those that were favorable to his hypothesis.<sup>69</sup> Lalande doubted that Bertier had really performed these experiments at all.

However, Bertier's confused behavior when the fraud was unmasked seems to speak for his innocence. He acknowledged that Le Sage's proofs were adequate. Then, after admitting that Coultaud and Mercier were fictitious persons, he continued to speak of his experiments as confirming theirs. And yet the weight differences Bertier reported were hundreds of times too great to be consistent with the experiments of Coultaud and Mercier. Finally, Bertier's competence in physics was minimal. His arguments from everyday experience in support of Coultaud's results are obscure, difficult to follow, and often simply incompetent. The fraudulent papers by Coultaud and Mercier were written with luminous clarity and meticulous attention to detail. The person who wrote them was a better physicist and a better writer than Bertier (better, too, than La Perrière or David). We must view Bertier as the most pitiable victim of this fraud—and of his own self-delusion. He was completely taken in and based a system of physics on the fraudulent experiments.

<sup>69</sup> Rozier, "Observations sur la lettre du Pere Bertier" (cit. n. 54), p. 460.

In a detective story, it is bad manners to introduce a new suspect late in the plot. However, in this case, all our leads have played out. Who were Coultaud and Mercier? The evidence suggests that there was but a single swindler. The two papers are similar in style. The details of Mercier's experimental arrangements are only slightly different from those of Coultaud. Moreover, both writers commit the same error in mathematical reasoning.

As we have seen, both Coultaud and Mercier find that the difference between the rates of two clocks is proportional to the difference in elevation between the two stations. Let  $\omega$  denote the angular frequency of the pendulum and  $r$  its distance from the center of the earth. Let  $\Delta\omega$  denote the small change in  $\omega$  associated with a small increase  $\Delta r$  in  $r$ . Coultaud's and Mercier's data give  $\Delta\omega/\omega = \Delta r/r$ . However, if the weight of the bob really does increase in direct proportion to the distance from the center of the earth (i.e., if  $g \propto r$ ), they *should* have found  $\Delta\omega/\omega = \Delta r/2r$ . The  $1/2$  arises from the fact that  $\omega$  varies as the square root of  $g$ .<sup>70</sup> The empirical result of Coultaud and Mercier (that  $\Delta\omega/\omega = \Delta r/r$ ) actually leads to the conclusion that  $g$  increases as the square of the distance from the center of the earth! So it is clear that the forger began with the idea that weight ought to be proportional to distance from the center of the earth and that he contrived data to support this view; but, because he was not well enough schooled in analysis, his invented data do not actually support the conclusion he wanted to draw.

Our forger was an able and literate writer, broadly though superficially educated in physics, but he was not a user of any but the most elementary mathematics. That Coultaud's experiments were set in Savoy and Mercier's in Valais may point to an Alpine connection for the forger. A resident of Savoy or of Valais would also have had no trouble making the posting of his communications to Paris look plausible. Who was Jean Coultaud? We cannot overlook the possibility that he was someone outside the circle of anti-Newtonians who participated in the public debate. "Jean Coultaud" may have lobbed his two bombs in from outside and then sat back to watch the fun. Indeed, I shall argue that he was Hyacinthe-Sigismond Gerdil—theologian, Cartesian philosopher, later a cardinal of the Catholic Church, and, in the last years of his life, a candidate for the papacy (see Figure 3).<sup>71</sup>

Hyacinthe-Sigismond Gerdil (1718–1802) was born at Samoëns in Savoy to parents of modest station. As a boy Gerdil studied in the schools of the Barnabite order, and he later decided to enter the priesthood as a Barnabite himself. While pursuing his theological studies at Bologna, he attracted the attention of Archbishop Lambertini, later Pope Benedict XIV, who became his protector and helped advance his career. The young Gerdil so distinguished himself by his knack for languages and his flair for philosophy that he was named, at the age of nineteen, to the chair of philosophy at the University of Macerata, from which he soon moved on to Casal. During this period, Savoy, Piedmont, and the island of Sardinia made up one kingdom, known as the Kingdom of Sardinia and ruled by the House of Savoy from the capital city of Turin. Gerdil found favor with the court, which in 1749 gave him the chair of philosophy at the University of Turin and, later, the chair of moral theology. At the suggestion of Pope Benedict XIV, King Charles-Emmanuel

<sup>70</sup> If  $L$  is the length of the pendulum, elementary mechanics gives  $\omega = (g/L)^{1/2}$ . Taking differentials, we find  $d\omega/\omega = dg/2g$ . If, following Cartesian sentiment, we suppose that  $g$  varies directly as the distance  $r$  from the center of the earth, then  $dg/g = dr/r$ . So, by substitution, Coultaud and Mercier should have found  $d\omega/\omega = dr/2r$ .

<sup>71</sup> When Gerdil moved from a French-speaking to an Italian-speaking culture, he Italianized his name to Giacinto Sigismondo Gerdil. His writings are most conveniently consulted in *Opera edite e inedite del cardinale Giacinto Sigismondo Gerdil*, 20 vols. (Rome, 1806–1821).

III of Sardinia made Gerdil the tutor of his son, the Prince of Piedmont, later King Charles-Emmanuel IV. In 1777 Gerdil was made a cardinal and called to Rome by Pope Pius VI, who named him bishop of Dibbon, consultor of the Holy Office, corrector of oriental books, and prefect of the Propaganda. On the death of Pius VI, Gerdil was among those considered for the papacy at the conclave of Venice in 1800, but he was passed over because of the opposition of the German Emperor.<sup>72</sup>

Now, let us recall that Jean Coultaud dated his letter to the *Journal des Beaux-Arts et des Sciences* “at Samoëns in Faucigny, Province of Savoy,” and that he described himself as a “former professor of physics at Turin.” During the Coultaud-Mercier affair Gerdil lived in Turin, where he taught at the university and later acted as tutor to the king’s son. Moreover, Gerdil was a native of Samoëns. Gerdil is mentioned by Le Sage as one of the persons he consulted in his search for the mysterious Coultaud. But Le Sage seems to have had no suspicions of Gerdil.

Throughout his career, Gerdil was a defender of spiritual philosophy and an opponent of materialism and deism. He was also the author of an *Anti-Emile* directed against the educational philosophy of Jean-Jacques Rousseau. Gerdil’s Cartesian sympathies are clear in his *History of the Sects of the Philosophers*.<sup>73</sup> In this survey of philosophy from antiquity to the eighteenth century, probably compiled in conjunction with his teaching, Gerdil praises Newton as a great man, but he devotes three times as much space to Descartes as to Newton.

For Gerdil, attraction was the most objectionable aspect of the Newtonian world view, and he dealt with it head-on in several publications that asserted the incompatibility of attraction with the phenomena. The first of these was a paper on the nature of cohesion, published in the *Journal des Sçavans* in 1752. Gerdil also claimed to find in the phenomena of capillarity compelling arguments against the reality of attraction. For example, if one applies a little grease to the inside of a capillary tube, water will not rise in it. We could hardly say that the attraction of the glass for the water is eliminated by the presence of the grease; so it is clear that the so-called attraction never existed in the first place. Gerdil’s attack on attraction was elaborated into a book-length *Dissertation on the Incompatibility of Attraction with the Phenomena, and on Capillary Tubes*, published at Paris in 1754. His alternative to attraction was elucidated in the closing sections of this book: vortices. Le Sage regarded Gerdil as one of the principal opponents of Newton’s system—along with Louis-Bertrand Castel, Bertier, and *les journalistes de Trévoux*.<sup>74</sup>

The evidence against Gerdil is, to be sure, mostly circumstantial. He was a Cartesian natural philosopher devoted to vortices and opposed to attraction. Like the fictitious Jean Coultaud, Gerdil was a native of Samoëns who had taught at the University of Turin. Gerdil thus had both motive and opportunity. Moreover, there is a certain similarity in

<sup>72</sup> The biographical information on Gerdil is based on the articles in *Biographie universelle* (cit. n. 25) and in *The Catholic Encyclopedia* (New York: Encyclopedia Press, 1913).

<sup>73</sup> H.-S. Gerdil, *Réflexions sur la théorie et la pratique de l’éducation contre les principes de M. Rousseau* (Turin, 1763) (in some later editions this was given the title *Anti-Emile, ou Réflexions sur la théorie . . .*; it is reprinted in *Opera*, Vol. 1, pp. 1–126); and Gerdil, *Histoire des sectes des philosophes* (*Opera*, Vol. 1, pp. 224–282).

<sup>74</sup> H.-S. Gerdil, “Mémoire sur la cause physique de la cohésion des hémisphères de Magdebourg . . .,” *J. Sçavans*, May 1752, pp. 273–284; and Gerdil, *Dissertations sur l’incompatibilité de l’attraction et de ses différentes loix, avec les phénomènes; et sur les tuyaux capillaires* (Paris, 1754) (*Opera*, Vol. 5, pp. 181–253). For Le Sage’s view of Gerdil’s prominence as an anti-Newtonian see Le Sage, draft of a letter to David (undated, but probably 1775), BPU, MS Suppl. 517, fol. 192v. The Jesuit Louis-Bertrand Castel had been a pugnacious and frequent contributor to, and effectively a member of the staff of, the *Journal de Trévoux* until he was squeezed out by G.-F. Berthier.



style between the papers of Coultaud and Mercier and the physical treatises of Gerdil. In its simple directness and clarity, as well as in its use of relatively short sentences, Gerdil's writing is much closer to that of Coultaud and Mercier than are the obscure, lengthy, and complex sentences of Bertier and La Perrière. But what might have prompted him to send a paper to the *Journal des Beaux-Arts et des Sciences* in 1769, fifteen years after writing his treatise on the incompatibility of attraction with the phenomena?

Gerdil was not permitted to stop thinking altogether about the phenomena of weight after the publication of his book against attraction in 1754. For Le Sage tried, though without much success, to engage him in a correspondence about ultramundane corpuscles. But the precipitating event was probably a paper by Lalande, published in the *Journal des Sçavans* in 1768. Here Lalande criticized Gerdil's treatment of capillarity and his claim to have found evidence against attraction. In response to Gerdil's experiment with the greased capillary tube, Lalande pointed out that drops of water, placed on the top of a dry glass tube in the air, are drawn in; but the drops will not descend, or even enter, a greased tube. We would hardly infer the unreality of gravitation from the failure of the drops to descend in this situation. Indeed, according to Lalande, the whole body of experiments on capillarity seemed rather to confirm the reality of attraction. Lalande went so far as to say, "The elevation of the fluid that one observes in these tubes appears to me, of all the phenomena of experimental physics, the one which best proves attraction."<sup>75</sup>

Lalande's paper, which turned Gerdil's arguments upside down, must have set him to thinking once again about the need for an experimental refutation of attraction. Lalande's paper was published in the issue of the *Journal des Sçavans* for October 1768. Coultaud's letter to the *Journal des Beaux-Arts et des Sciences* was dated November 1768. Did Gerdil sit down, a month after reading Lalande's attack on his own treatise, to write out a new proof of the incompatibility of attraction with the phenomena?

#### LAST LIGHT ON A WORLD VIEW

Bertier's retraction closed the affair, which had run for eight years. Neither Bertier's experiments, nor his thought experiments, nor his plea that the system of attraction was incompatible with religion, nor yet the fraud committed by Coultaud and Mercier—whoever they were—had sufficed to call Newton's theory of universal gravitation seriously into question. By the 1770s the French anti-Newtonians were relegated to the fringes of the scientific community. Theirs was the last generation of anti-Newtonians to receive serious attention in a specialized scientific journal (*Observations sur la Physique*). Their fate was perhaps already prefigured in the role played by a literary journal (*Journal des Beaux-Arts et des Sciences*) in the early stages of the controversy.

The marginality of the French anti-Newtonians in the 1760s and 1770s is evident in a number of traits they tended to share. First, most of them were already of advanced years. Thus, when Coultaud's paper was published in 1769, Gerdil was in his fifties, Bertier in his sixties, and La Perrière in his seventies. (The only youngster among them was the medical doctor, David, who was in his thirties.) The anti-Newtonians soon disappeared because they could not live forever. Second, none of them was well connected to the Parisian world of academic science. La Perrière was a freelance crank. David's credentials

<sup>75</sup> Joseph-Jérôme Le Français de Lalande, "Lettre sur les tubes capillaires . . ." *J. Sçavans*, Oct. 1768, pp. 723–743, on p. 724. That Le Sage tried to induce Gerdil to consider his ultramundane corpuscles is clear in an undelivered letter to Gerdil in which Le Sage makes reference to an earlier communication: BPU, MS Suppl. 517, fol. 328.



were due to his connections to a provincial academy, that of Rouen. Only Bertier could claim an affiliation to the Paris academy, and that as a mere *correspondant* who turned out to be an embarrassment to the academicians. Third, the die-hard anti-Newtonians were all hostile to mathematics. Bertier's *Physical Principles* was intended as an explicitly physical—that is, nonmathematical—complement to the *Mathematical Principles* of Newton. David, in his *Dissertation on the Figure of the Earth*, assured his readers that “algebra is less necessary to physics than is commonly believed.” La Perrière's dreamlike electrical system of the universe was expressed in entirely nonmathematical language. Moreover, in his *Burlesque Decree*, La Perrière accused the Cartesi-Newtonian establishment of enveloping their arguments “with all the apparatus of calculations, algebraic characters, scientific terms, and jargon, suited to forbid access to the vulgar layman and to prevent him from unfolding and unmasking their charlatanry.”<sup>76</sup> Gerdil's *Discourse on the Incompatibility of Attraction with the Phenomena* similarly had no need of mathematics.

The marginality of the anti-Newtonians of the 1760s and 1770s is also apparent in the confidence of the Newtonian establishment that second- and third-string Newtonians were up to the task of crushing them. The Paris Academy of Sciences never became directly involved. D'Alembert and Lalande made brief replies to the articles of Coultaud and Mercier but did not let themselves be drawn into a public dispute. After the first round, the academicians preferred to work behind the scenes. For example, Le Sage's letter in *Observations sur la Physique* unmasking the fraud was introduced by an unsigned, strongly worded statement that reviewed the controversy around the experiments of Coultaud and Mercier and concluded: “His [Le Sage's] letter . . . will decide what one ought to think of them.” The reader might easily assume that this preface was written by the editor; in fact, it was written by Lalande.<sup>77</sup> Nevertheless, the burden of the fight against the anti-Newtonians was borne by Le Sage, Rozier, and the members of the Academy of Dijon.

Finally, it is clear that in 1770 there was no such thing as a unified Cartesian physics; indeed, there never had been. There existed only a shared belief in vortices and a dislike for action at a distance. The anti-Newtonians did not have a unified position on any other major issue. They disagreed about the details of the mechanical cause of weight, and none of them could calculate anything of interest from the vague principles enunciated in their stream of discourses on the cause of weight. They even disagreed with one another about the shape of the earth: David held that it was elongated at the poles, Bertier that it was flattened, and La Perrière that it was perfectly spherical. The anti-Newtonians were defeated because their systems had nothing to offer in comparison with the demonstrated calculating power of Newton's physics.

What were the motives of the rear-guard anti-Newtonians? Bertier, Gerdil, and Aubert (the editor of the reactionary *Journal de Beaux-Arts et des Sciences*) were clerics. Bertier saw attraction as incompatible with true religion. Gerdil, a more sophisticated thinker than Bertier, is harder to assess. But the fusion of Cartesian natural philosophy with Catholic theology that characterized French thought at the beginning of the century also fully characterized Gerdil's world view. As for Aubert, although he was certainly disposed to conservatism in all spheres, he seems to have been no worse than a mere opportunist in this affair. For the conventionally religious among the anti-Newtonians, religion did play a part in shaping reactions to scientific developments. But it would be wrong to equate

<sup>76</sup> David, *Dissertation sur la figure de la terre* (1771) (cit. n. 39), p. 10; and La Perrière, *Arrêt burlesque* (cit. n. 18), pp. 5–6.

<sup>77</sup> Le Sage, “Lettre de M. Lesage” (cit. n. 42). Le Sage says plainly that the preface was written by Lalande in Le Sage to Jean-André Deluc, 31 May 1773, BPU, MS Fr. 2464, fol. 160.

conventional religiosity with anti-Newtonianism in the late Enlightenment. Rozier (the editor of *Observations sur la Physique*) and the astronomer Alexandre-Guy Pingré (who published a caustic review of Bertier's *Physical Principles* in the *Journal de Trévoux*) were also clerics. Moreover, many of the repetitions of Bertier's experiment were carried out in church towers. The members of the Academy of Dijon went out of their way to thank the local clergy for their helpfulness in making the church available to them.<sup>78</sup>

Robert Darnton has chronicled the rapid expansion of "Grub Street" in the mid-Enlightenment.<sup>79</sup> Young and not-so-young men converged on Paris, struggling to make names as writers, philosophes, or scientists. What characterized them as a group was not so much a political position (royalist or republican, *encyclopédiste* or *devot*) as the fact that they were outsiders wanting in—into the academies and into a pension. The popular press was their principal tool. Although La Perrière, Bertier, and David were certainly not members of the literary underground described by Darnton, there is rather an odor of Grub Street about this affair. La Perrière's and David's angry ranting against the establishment for attempting to silence them and Bertier's sycophantic flattering of the Academy of Sciences for not endorsing any system were two kinds of behavior familiar in Grub Street.

But it is important to note that, in many ways, the third-string Newtonians were not terribly different from their anti-Newtonian opponents. Le Sage's level of mathematical ability was higher than that of Bertier, David, or La Perrière, but he too had been left far behind by d'Alembert and the other leading mathematical physicists. Le Sage privately criticized d'Alembert's papers on the experiments of Coultaud and Mercier, saying they were "more full of calculation than of reasoning."<sup>80</sup> Of course, Le Sage did have a valid complaint; for in treating the experiments as real, d'Alembert had missed the point.

The similarity of the two groups led to a phenomenon we might call marginal validation. Figures, such as Le Sage, who were near but still inside the margin of respectability were willing to engage in discourse with figures, such as David and La Perrière, whom the Newtonian establishment considered to be definitely beyond the pale. Thus, while La Condamine had stiff-armed David, Le Sage was pleased to correspond with him and promised to discuss David's ideas in the great *History of Weight* he was perpetually writing. To some extent, this process of marginal validation was aided by the system of French provincial academies (as in David's case) and by the naming of *correspondants* to the Paris academy (as in Bertier's).

The common aspirations of the two groups show up very clearly in Le Sage's campaign to have himself named a foreign member of the Royal Society of London. In the fall of 1767, just before the Coultaud-Mercier affair began, Le Sage was nominated by three fellow Genevans who were already foreign members, as well as by three domestic members. But when the nomination was taken up for final decision in the spring of 1768, Le Sage failed. The statutes of the Royal Society stipulated that no more than two foreign members could be elected in a single year. Le Sage was squeezed out by two candidates who had been nominated two weeks ahead of him and who also had more distinguished records: the naturalist Lazzaro Spallanzani . . . and Father Bertier of the Oratory of Paris.

<sup>78</sup> Alexandre-Guy Pingré, "Lettre sur les *Principes physiques* . . . par le P. Bertier de l'Oratoire," *J. Trévoux*, Dec. 1763, p. 2938; and [Commission of the Academy of Dijon], "Mémoire dans lequel on indique les causes qui peuvent changer accidentellement les effets apparens de la pesanteur des corps à des hauteurs inégales" (cit. n. 56).

<sup>79</sup> Robert Darnton, *The Literary Underground of the Old Regime* (Cambridge, Mass.: Harvard Univ. Press, 1982).

<sup>80</sup> Le Sage to Deluc, 31 May 1773, BPU, MS Fr. 2464, fol. 162.

Among those who signed Bertier's certificate of nomination were d'Alembert and Lalande. One can only assume that they later felt regrets.

A few years later Le Sage tried again. In 1773 his friend Deluc settled in England, where he became reader to Queen Charlotte, wife of George III. Le Sage encouraged his friend to help the members of the Royal Society appreciate "the little service I have rendered to the system of Newton" by revealing the Coultaud-Mercier fraud as well as by criticizing the experiments of Bertier, "even though [Bertier] is a member of the Royal Society." Unwilling to leave matters to chance, Le Sage sent handwritten copies of his published papers to Matthew Maty, secretary of the Royal Society. Le Sage's goal was realized in 1775, when he was elected a foreign member of the Royal Society.<sup>81</sup>

Of all the participants in this affair, only Coultaud and Mercier aspired neither to fame nor to pensions. They published under pseudonyms and never stepped forward to claim their rewards. Whatever the motives of all the others might have been, Jean Coultaud was probably motivated by philosophical conviction.

La Perrière died in 1776, just before Bertier's retraction. Bertier himself was in decline, but unrepentant. A year after his "retraction," Bertier published his *History of the First Times of the World*, in which he demonstrated the complete agreement between Cartesian physics and the biblical account of the creation. This work achieved a certain celebrity for its claim that, to understand the Book of Genesis properly, one must read it backward. The Church of the Oratory, because of its proximity to the Louvre, continued to enjoy a special relationship to the court, as well as to the Academy of Sciences. When an important member of the royal family married or celebrated the birth of a child, fell sick or died, religious services were held at the Oratory. All these events were attended by members of the Academy of Sciences. Father Bertier officiated at some of these ceremonies and long retained the right of saying mass for the Academy itself.<sup>82</sup> At the Oratory, the academicians attended services they could not politely avoid. There, kneeling before Father Bertier, *correspondant*, they could contemplate the upper reaches of the church, where the most unrepentant Cartesian of them all had placed his balances in an effort to banish attraction from the lexicon of physics.

<sup>81</sup> Le Sage to Deluc, 16 Nov. 1773, BPU, MS Fr. 2464, fol. 164. For the history of Le Sage's two candidacies see Royal Society (London), *Journal Book of the Royal Society*, Vol. 26, meetings of 12 Nov., 26 Nov. 1767, 2 June 1768, Vol. 28, meeting of 1 June 1775. For Le Sage's correspondence with Maty see Royal Society, L & P [Letters and Papers], Vol. 6, p. 199.

<sup>82</sup> Joseph-Etienne Bertier, *Histoire des premiers temps du monde, prouvée par l'accord de la physique avec la Genèse, par les philosophes, etc.* (Paris, 1778). On Bertier's role at the Paris Oratory see Roger Hahn, *The Anatomy of a Scientific Institution: The Paris Academy of Sciences, 1666–1803* (Berkeley: Univ. California Press, 1971), p. 74.

## APPENDIX. Outline of the Published Debate

1769	June	Coultaud (cit. n. 4)	JBAS	
	July	d'Alembert (cit. n. 10)	JBAS	
	December	La Perrière (cit. n. 21)	JBAS	
1770	March	Anonymous letter <sup>a</sup>	JBAS	
		Bertier (cit. n. 30)	<i>Principes physiques</i> , Vol. 4	
1771	February	La Perrière (cit. n. 21)	JBAS	
	December	Mercier (cit. n. 13)	JBAS	
1772		David (cit. n. 39)	<i>Dissertation</i> , 2nd ed.	
	January	Genet (cit. n. 12)	JBAS	
	February	Bertier (cit. n. 31)	JE	
	March	Bertier (cit. n. 31)	JBAS	
	April	Le Sage (cit. n. 41)	JBAS	
	May		La Perrière (cit. n. 21)	JBAS
			Bertier (cit. n. 31)	JBAS
	July	Bertier (cit. n. 31)	JBAS	
	August	Lalande (cit. n. 14)	JS	
	November	Anonymous Genevan <sup>b</sup>	JBAS	
	December	Bertier (cit. n. 31)	JBAS	
	1773	February	Anonymous Genevan <sup>c</sup>	JBAS
April		Le Sage (cit. n. 42)	OP	
May		Bertier (cit. n. 30)	JBAS	
September			Anonymous letter <sup>d</sup>	JBAS
			Bertier (cit. n. 49)	OP
October			Bertier (cit. n. 50)	JBAS
			Bertier (cit. n. 50)	SC
November			La Perrière (cit. n. 51)	OP
			Bertier (cit. n. 50)	JBAS
			Le Sage (cit. n. 53)	OP
1774			d'Alembert (cit. n. 9)	<i>Opuscules</i> , Vol. 6
	February	Anonymous review (cit. n. 32)	SC	
	October		David (cit. n. 55)	OP
			Cotte <sup>e</sup>	OP
	November		Bertier (cit. n. 32)	SC
			Bertier (cit. n. 54)	JPL
	December		Adam (cit. n. 55)	JPL
			David (cit. n. 58)	OP
			Rozier (cit. n. 54)	OP
	1775	February	David (cit. n. 55)	OP
April			Bertier <sup>f</sup>	OP
			Bertier (cit. n. 32)	OP
		Academy of Dijon (cit. n. 56)	OP	
July		Dolomieu (cit. n. 56)	OP	
1776	January	Le Sage <sup>g</sup>	OP	
	May	Leroy (cit. n. 63)	OP	
1777		Pringle (cit. n. 63)	OP	
	June	Bertier (cit. n. 64)	OP	

Abbreviations: JBAS = *Journal des Beaux-Arts et des Sciences*; JE = *Journal Encyclopédique*; JPL = *Journal de Politique et de Littérature*; JS = *Journal des Sçavans*; OP = *Observations sur la Physique*; SC = *Suite de la Clef*.

<sup>a</sup>Anonymous, "Lettre à Monsieur l'Abbé Aubert . . ." *JBAS*, Mar. 1770, pp. 433–436.

<sup>b</sup>Anonymous savant of Geneva, "Réflexions sur la manière d'estimer l'action de la pesanteur à deux distances différentes de la surface de la terre; pour servir de réponse à la première des démonstrations proposées par la Pere Berthier . . .," *JBAS*, Nov. 1772, pp. 197–198.

<sup>c</sup>Anonymous savant of Geneva, "Réflexions . . . pour servir de réponse aux démonstrations 2e & 3e du P. Bertier . . .," *JBAS*, Feb. 1773, pp. 224–238 (this continues the letter published in the Nov. 1772 issue).

<sup>d</sup>Anonymous, "Lettre à M. le Chevalier de \*\*," *JBAS*, Sept. 1773, pp. 520–527.

<sup>e</sup>Louis Cotte, *OP*, Oct. 1774, pp. 340–341.

<sup>f</sup>Joseph-Etienne Bertier, "Lettre du Pere Bertier de l'Oratoire . . .," *OP*, Apr. 1775, p. 305.

<sup>g</sup>Georges-Louis Le Sage, "Expériences et vues, sur l'intensité de la pesanteur dans l'intérieur de la terre," *OP*, Jan. 1776, pp. 1–12.