Science & Education **10:** 515–543, 2001.

"Weighing" the Earth: a Newtonian Test and the Origin of an Anachronism ⋆

ANTONIO MORENO GONZÁLEZ
Universidad Complutense, Departamento de Didáctica de las Ciencias Experimentales, Facultad de Educación, Rector Royo Villanova s/n, 28040 Madrid, Spain

Abstract. The measure G, the universal gravitational constant, is attributed to Henry Cavendish. Nevertheless, the intention of the English physicist was to measure the density of the earth, which at that time was necessary in order to decide between different theories about the composition of this planet. G was measured much later. In this article I will try to explain how Cavendish accomplished the famous experiment and what his results were. Likewise, I will consider the problems that can arise in the scientific training of students from maintaining anachronisms such as this.

1. Introduction

In our text books we learned, and we continue to teach, that the English physicist Henry Cavendish (1731–1810) carried out a legendary experiment using a torsion balance 'to measure G, the universal gravitational constant'. The experiment was done two hundred years ago, but until today it is still not recognized, or at least it hasn't been widely explained, that this was not the result of Cavendish's experiment. In reality, Cavendish measured the density of the earth. Perhaps in the first physics text books in the 19th century, when the formulation of the gravitational law began as an equality not as a proportion as Newton enunciated in his law, they did not keep in mind the real measurement of Cavendish, and since is easy to calculate G if you know the density of the earth, the authors preferred to attribute the calculation of G to the famous experiment and in this way to simplify the explanations. If this was the reason and this was recognized there would be no cause to worry, but judging from the many books consulted, past and current, the time frame for measurements – the density of the earth and G – is not clarified, and therefore the error should be corrected. Because this difference has not been made clear, the confusion has become doctrine, attributing Cavendish with an interest in calculating a universal constant which neither he nor anyone of his time could possibly have had, because this is a matter related to units of measurement and the homogeneity of the physical laws, neither of which were systematically

⋆ To my friend and teacher Alberto Galindo from whom I have learned to value scientific rigor.

practiced in physics until the middle of the 19th century, as a consequence of the application of the concept of dimensions to physical magnitudes (Palacios, 1964). Indeed, it was not introduced until 1822, by Jean Batiste Fourier (1768–1830) in his *Théorie analytique de la Chaleur*, where for the first time the need to use systems of units appropriate for equations associated with physical phenomena is proposed. Therefore the gratuitous attribution of the measurement G to Cavendish is an authentic anachronism. As such, it can be added to the list of other famous constants erroneously attributed to people who did not actually determine them: the speed of light, Avogadro's number, Boltzmann's constant, etc. From a historical point of view it is necessary to clarify these errors in the text books if the history of science is to be used in science teaching.

2. Suppositions, Doubts and Controversies

The widespread awareness during the 17th century that something *new* was going on (Rossi, 1997) regarding the conception of nature and the way to approach the knowledge of natural phenomena is evident even in the titles of outstanding publications of the time: *The New Attractive*, Robert Norman (1581); *Nova de universis philosophia*, Francesco Patrizi (1591); *De Magnete magneticisque corporibus, et de magno magnete tellure; Philosophia nova, plurimis et argumentis et experimentis demostrat*, William Gilbert (1600); *Novo teatro di macchine*, Vittorio Zonca (1607); *Astronomia nova*, Johannes Kepler (1609); *Novum organum, sive indicia vera interpretatione naturae et regno hominis*, Francis Bacon (1620); *Discorsi e dimostrazioni matematica intorno a due nuove scienze*, Galileo Galilei (1638). But the most relevant is without a doubt *Philosophiae Naturalis Principia Mathematica*, Isaac Newton (1642–1727), published in 1687 by the Royal Society of London at the expense of Edmund Halley (1656–1724), who was fully convinced of the brilliance of Newton's treatise which signified the first great unification of physics, eliminating the metaphysical distinctions of the ancients between the treatment of celestial and terrestrial phenomena. Pierre Simon Laplace (1749–1827) wrote:

> This admirable work contains the seeds of all the greats discoveries that have been made thereafter on the System of the world: the history of its development by the successors of this great geometer would be both the most useful commentary on his work and the best guide to making the new discoveries. (Laplace, 1904, p. 215)

And in another of his fundamental publications about Newton's law, the French astronomer and mathematicians said:

> There does not exist in physics a more incontestable truth, and better demonstrated in agreement with observation and calculation than this: All celestial bodies gravitates one upon the other. Newton, the author of this discovery, the most important ever made in natural philosophy, finds that the observable movements of the planets cannot be possible without a tendency towards the

> sun, proportional to their masses, and reciprocal to the square of the distance to this star. (Laplace, 1773, p. 212)

The same applies to the satellites with respect to the planets and to all material particles among each other. The basis of the *Principia* is therefore set upon what were then considered the effects of *attraction*, with reference to the body to which something is pulled, and *gravitation*, with respect to the body that is pulled. If one is talking about planetary gravitation the agreed term was *gravity*. The attraction between particles of matter was called *cohesion*. And although neither Newton nor many of his followers made hypotheses about the causes of such forces, preferring to leave the design of the world in the hands of God, they did try to measure them. Taking for granted that, thanks to universal gravitation, the celestial order was maintained – the Keplerian 'harmony of the spheres' – they attempted to prove, and if possible to measure, the same supposed gravitation attraction between ordinary objects, or between objects and the earth itself, especially to corroborate the still doubted but, at the same time, versatile inverse-square law of the distance, soon also associated with electrostatic and magnetic phenomena.

Attraction, for Newton, meant 'action at a distance', a concept that formed the foundation of his mechanics. Yet it posed a serious problem for the theory of gravitation to be accepted by continental Europe's scientific community, who were more linked to Cartesian mechanics and its concepts of 'contact action' in a world where, for René Descartes (1596–1650) and his followers, the void was inconceivable: the vortex of ether, that subtle material filling everything, was what transmitted every interaction between distant bodies. The controversy that arose regarding the two visions of the world became a nationalistic problem, especially in France (Lafuente & Delgado, 1984a), where the Academy of Sciences, the salons of the Enlightenment – places of scientific showing off and polemics – and the State itself demanded a convincing response for the cause of attraction. Although Newton himself had rejected occupying himself with such questions, to the eyes of the Cartesians convinced of having a very clear explanation of interactions, Newton's proposal was nearer the hidden qualities of Aristotelian and medieval physics, or at least the metaphysical causes for explaining movement, than the solid and testable reasons that scientific knowledge required.[1]

For the criteria of the Academy of Sciences of Paris, the determination of the shape of the earth could be conclusive in deciding between the opposing Cartesian, in particular Jean Dominique Cassini (1625–1712) and his followers, and Newtonian factions.[2] While some renowned Cartesians, such as the Dutch Christiaan Huygens (1629–1695), shared Newton's ideas on the shape of the earth, they rejected Newtonian 'actions at a distance' (a controversy that has been well covered: Lacombe & Costabel, 1988; Lafuente & Delgado, 1984; Greenberg, 1995). On the shape of the earth, Newton, in the Theorem 16 – 'That the axes of the planets are less than the diameters drawn perpendicular to the axes' – (Proposition 18) of the third book of the *Principia*, conjectures:

> The equal gravitation of the parts on all sides would give a spherical figure to the planets, if it was not for their diurnal revolution in a circle. By that circular motion it comes to pass that the parts receding from the axis endeavor to ascend about the equator; and therefore if the matter is in a fluid state, by its ascent toward the equator it will enlarge the diameters there, and by its descent towards the poles it will shorten the axis. So the diameter of Jupiter (by the concurring observations of astronomers) is found shorter between pole and pole than from east to west. And, by the same argument, if our earth was not higher about the equator than at the poles, the seas would subside about the poles, and, arising towards the equator, would lay all things there under water. (Newton, 1687/1993, p. 288)

Combining the principle of attraction with the measurement of centrifugal force established by Huygens in his *Horologium oscillatorium* (1673), Newton postulates that the earth is a spheroid with a flattening at the poles with respect to the equator as 229 is to 230. To compare the curvature of the surface of the earth in places of different latitude, measurements were taken of the respective longitudes of a degree of meridian in different places (Todhunter, 1873). If the earth were a sphere, the radius of the curvature of the arc of the meridian, wherever the chosen point was, would be the same: the radius of the earth; the differences in the length of a degree of the meridian would be greater where the curvature was less, which is to say, where the places studied were more shortened (Taton, 1988).

On the other hand, during the 18th century, the controversies over the composition of the earth were polarized around two stances, so strongly defended that they even endangered the credibility of the incipient field of Geology as a natural science. The Prussian Abraham Gottlob Werner (1750–1817) considered that the ocean was the source of all terrestrial formation; the physical and chemical action of the water was the foundation of the mineral kingdom. The opposing theory to the Neptunian was the Plutonian or volcanist theory of the Scotsman James Hutton (1726–1797), which attributed the basic geological formation of the earth to fire or a central core of heat. Although his work (Hutton, 1795) was little appreciated in his lifetime, the work of his followers, especially John Playfair (1748–1819) author of *Illustrations of the Huttonian theory* (1802), and the adhesion of the Germans Alexander von Humboldt and Leopold von Buch, accomplished the implantation of Hutton's theories over those of Werner, thanks also to the support given by the recently created Geological Society of London, in 1807, which opted for the Plutonians while recognizing the inexorable geological action of water. The definitive acceptance of Plutonian theory was given by the Scottish geologist Charles Lyell (1797–1875) in *The Principles of Geology: being an attempt to explain the former changes of the Earth's surface, by reference to causes now in action* (1830–1833), written under the influence of Newtonian synthesis and with identical unifying intentions as those expressed in the *Principía* (Bowker,1995).

If the measurement of the arc of the meridian was accepted to elucidate the shape of the earth, measuring its density was considered at that time the first task to decide between the controversial positions on the composition of the planet, both attempts associated with the admission or not of Newton's alleged attraction.

3. In Search of Solutions: Attempts and Repetitions

The first geodesic operation sponsored by the Academy of Sciences of Paris was the measurement of a degree of latitude between Malvoisine and Sourdon carried out by the astronomer and abbot Jean Picard (1620–1682), Gassendi's collaborator in his early years and author of *La mesure de la Terre* (Paris, 1671), in which he gives a figure of 39,801 km for the circumference of the earth, being among the first to realize that the variation of the length of the seconds pendulum along with the latitude could help determine the shape of the earth. In the first edition of the *Principia*, Newton used Picard's measurement of a degree of latitude. In 1722 the Academy of Sciences of Paris got word of the geodesic measurements done between Dunkerque and Perpignan (France) by Jacques Cassini (1677–1756), who concluded that the earth is an ellipsoid of revolution slightly elongated at the poles.[3] This interpretation of an prolate earth,[4] soon rejected by even the most convinced Cartesian scientists, was maintained by other authors as a reaction to the influence of Newton on the Continent.[5]

One of the first decisions to be made in the geodesic investigations was to choose the measurement of longitudes of degrees of parallels or of meridian.[6] The Italian Giovanni Poleni (1683–1761) won the prize of the Academy of Paris in 1733 with a work in which he demonstrates that, to determine the shape of the earth, measurements done on the meridian were more trustworthy those done on the equator. Pierre L.M. de Maupertuis (1698–1759), Frenchman and Newtonian to the extreme of being classified by Voltaire as the Galileo of France in the battle to introduce Newtonianism in Paris, adopted Poleni's proposal and put it into practice on the Academy-sponsored geodesic expedition to Lapland, which he led in 1736, assisted by, among others, Alexis Claude Clairaut (1713–1765) and the Swedish astronomer Anders Celsius (1701–1744). A year before, another expedition, also sponsored by the Academy, had gone to Peru with the same purpose of providing data to clear up the controversy about the shape of the earth; an expedition on which the geographer Charles-Marie de la Condamine (1702–1774), the teacher of hydrography of the University of Le Havre, Pierre Bouguer (1698–1758), and the Spanish cosmographers and sailors Jorge Juan (1713–1773) and Antonio de Ulloa (1716–1795), both named by the Council of the Indies. From their arrival in Quito in 1736 until 1743, the expedition members set about determining a degree of meridian in the vicinity of the line of equinox, as well as other geographic anthropological, medical and botanical studies (Condamine, 1986). In spite of the disagreements between the results obtained in the expeditions to Lapland and Peru (where there were public discrepancies between La Condamine and Bouguer), the difference between their results and those obtained years before in France made some members of the Academy doubt the precision of the earlier ones, which led to new measurements and expeditions.[7] The geodesic operation done in Italy in 1750, ordered by Pope Benedict XIV, was also important, because it measured the degrees of meridian between Rome and Rimini. Participating in the project was the Jesuit Ruggero G. Boscovich (1711–1787), who believed in the theory of the flattening of the earth at the poles, although with more reservations than the French. The results of these expeditions led to drawing a new map of the Church States in Italy. These cartographic applications and other terrestrial measurements strengthened geodesic science, and improved astronomy by incorporation of new techniques and new instruments.

On the aforementioned expeditions, the measurement of time was done with a seconds pendulum, since there were no reliable clocks until the middle of the 18th century clocks.[8] To this end, on all expeditions and in successive latitudes they measured the length of the pendulum. At the same time, in order to determine vertical in any place, taking as a reference the position of stars, they used an instrument in which the basic element was a plumb-line. The expedition to Peru wondered whether if the plumb-line would always mark the vertical without the slightest defect or variation. This was a decisive question in order to make the appropriate corrections in measuring time as well as in establishing the latitude of the places chosen as observatories. The evidence of such variation, in the proximity to large masses such as mountains, was another confirmation of the Newtonian law of attraction. Furthermore, they considered the variation of gravity at different altitudes above sea level because of the influence it would have on the period of oscillation of the pendulum. Bouguer wrote a formula which in the end agreed very little with the observations done with the pendulum to calculate the acceleration of gravity at specific altitudes (Lafuente & Delgado, 1984b, p. 230):

$$g_h = g_0 \left(1 - \frac{5}{4}\frac{h}{R}\right)$$

where g_0 is the gravity at sea level; h, the altitude considered; R, the radius of the earth.

Admitting the gravitational influence of the mountains on the oscillation of the pendulum and on the weight of bodies, Bouguer commissioned various experiments to determine the density of a mountain, Chimborazoo in this case,[9] and the mean density of the earth. He obtained two different results: in one calculation, the earth was determined to be 4 times denser than the mountain; in another, 12 times denser (Poynting, 1894). In spite of the errors in the results, as (Poynting, 1894, pp. 5–6) wrote, ‘at least he deserves the credit of first showing that the attraction by mountain masses actually exists, and that the earth, as a whole, is denser than the surface strata. As he remarks, his experiments at any rate proved that the earth was not merely a hollow shell, as some had till then held; nor was a globe full of water, as others had maintained. He fully recognised that his experiments were mere trials, and hoped that they would be repeated in Europe’. Not to be forgotten is that Newton, in Proposition X, Theorem X of the third book of the *Principia*, conjectures that the earth is 5 or 6 times denser than water. The case of Bouguer is worth mentioning in that it touches on French reservations about Newtonian theories, in defence of Cartesian orthodoxy. When he had to extract measurement and conclusions about his work, he was confronted by the reality that he necessarily had to use Sir Isaac Newton's ideas, recognizing 'the complete sufficiency of the Newtonian gravitational system, which signified a great triumph for the English philosopher' (Lamontagne, 1964a, p. 132).

In England, in the year 1714, the British Government's Longitude Act was signed into law. They established a budget of 20,000 pounds for carrying out observations, measurements, experiments and design of material to improve astronomy and navigation. The members of the Board of Longitude, presided over by the first Lord of the Admiralty, were admirals, teachers of mathematics and astronomy from Cambridge, the President of the Royal Society and the royal astronomer. In 1765 Rev. Nevil Maskelyne (1732–1811) was named royal astronomer. In 1772 he read to the Royal Society the article entitled *A Proposal for measuring the Attraction of some Hill in this Kingdom by Astronomical Observations*.[10] If the Bouguer experiment were repeated, said the Reverend, not only would it provide irrefutable experimental proof of universal gravitation, as the French had admitted already, but it would also '... serve to give us a better idea of the total mass of the earth, and the proportional density of the matter near the surface compared with the mean density of the whole earth' (Howse, 1989, p. 131). He believed that such an experiment, as well as contributing to resolving the controversy about the internal composition of the earth, would be an honor for the nation that undertook it and for the Society that executed it. The Council of the Royal Society immediately set up a Committee for Measuring the Attraction of Hills composed of Maskelyne, Cavendish, Franklin, the Honorable Daines Barrington, and Rev. Samuel Horsley. With this newly created Committee, experiments to 'weigh the earth', as the experiments to measure the 'attraction of mountains' were also called, became important to English Newtonians. In the summer of 1773, the astronomer Charles Mason (1730–1787) was commissioned to find an appropriate hill in the highlands of Scotland to repeat Bouguer's experiment as Maskelyne has proposed. The mountain chosen was in Perthshire, in the center of Scotland. The neighbouring inhabitants called the mountain *Schehallien* (the name usually cited is Schiehallion).

Without entering into particulars of the experiment (Maskelyne, 1775), which are minutely detailed in Maskelyne's biography (Howse, 1989), in articles (Leadstone, 1974) and textbooks,[11] the instruments (Figure 1) were placed on opposite sides of the mountain, on the north and south faces respectively, as is indicated in Figure 2. The main operations done were: (1) Find through observation of the skies the apparent difference of latitude between the two stations; (2) Find by triangulation the real difference in latitude between the two stations; (3) Determine the shape and dimensions of the hill, from which, knowing the variation obtained from the difference between (1) and (2), one could determine the mean density of the earth. The analysis of the results and their mathematic treatment (Hutton, 1779)

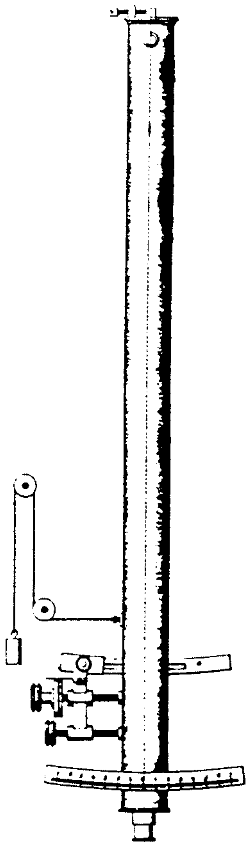

Figure 1.

were the responsibility of Charles Hutton (1737–1823), teacher of mathematics of the English Royal Military Academy, who, in his report, said he had used a method of analysis which he owed principally to Cavendish in order to resolve such complicated mathematical calculations.

The deflection of the plumb-line due to the mountain was estimated at 12". But, according to the calculations, if the mountain had the same density as the earth as a whole, the deflection should have been 21", which is to say 1.8 times the observed amount. Hutton and Maskelyne thought that this amount – 1.8 – was the difference between the densities earth vs. mountain. From rock samples, they estimated its density to be 2.5 times that of water, and therefore that the earth's density was 4.5 times the density of water. Years later, a mathematician and Presbyterian minister, the Scotsman John Playfair (1748–1812), appreciating the importance of the density of the earth to physical astronomy, did a detailed lithological study of Schehallien and thereby obtained a figure for the density of the earth as 4.713 (Playfair, 1811). Hutton himself, at the age of 84, published a revision of the work

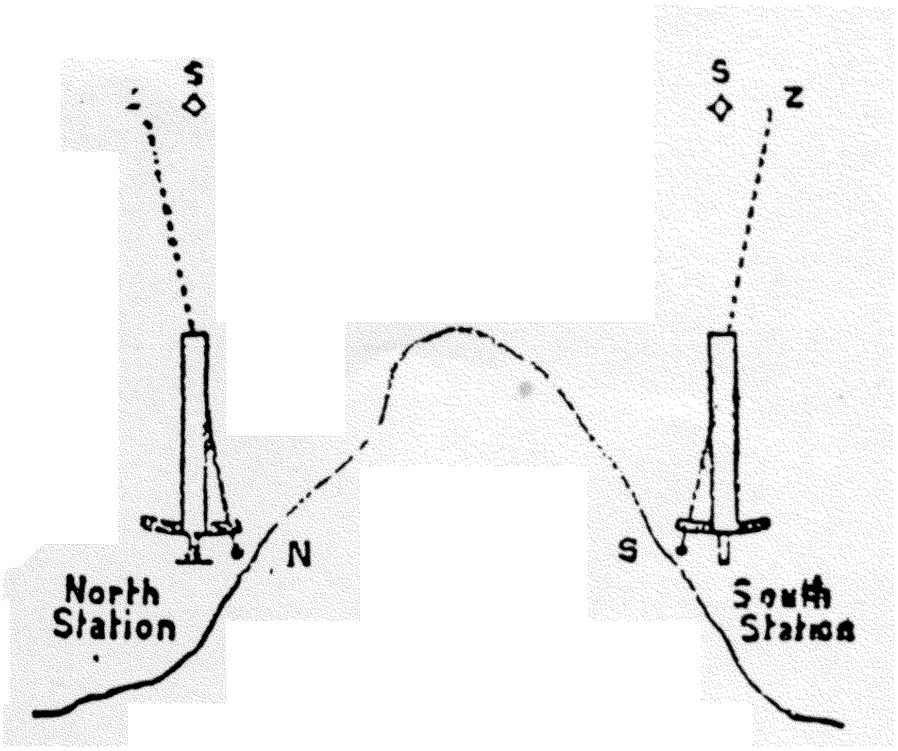

Figure 2.

done with Maskelyne (Hutton, 1821). There were also attempts to measure the mean density of the earth comparing the period of oscillation of a pendulum on the earth's surface and another situated inside a mine. An example is the attempt in the summer of 1826 in a coal mine in Cornwall[12] by William Whewell (1794–1866), at that time teacher of Mineralogy at Trinity College in Cambridge, and George B. Airy (1801–1892), then teacher of Mathematics at the same school, later Royal Astronomer.

Once it was recognized almost without argument that the earth had a flattened shape, there proliferated, as we can see, measurements of its density in order to resolve the controversy about its geological composition. Laplace, who presided over the French Board of Longitudes, began the forementioned article in this way: 'One of the most curious points of Geology is the relationship between the mean density of the earthly spheroid and that of a known substance' (Laplace, 1904, p. 215). And he dedicated part of his *Mécanique céleste* (1799–1825) to the shape of the planets, refuting ideas about the homogeneity of the earth, as these words of his sum up:

> In spite of the irregularities that the measured degrees of the meridians present, they nevertheless indicate a smaller flattening than that which would correspond to a homogeneous earth; and the theory proves that this flattening

> requieres a growing density in the layers of the earth from the surface to the center. In parallel, the experiences of the pendulum, preciser and more in agreement than measurements of degrees, indicate an increase in gravity from equator to the poles, greater than in the case of a homogeneous earth. (Laplace, 1904, p. 216)

4. Cavendish: The Crucial Experiment

The experiments cited so far are geodesic, astronomical or lithological, when not purely thought experiments, as in the case of Newton. The measurement in the laboratory of the earth's density was begun by Cavendish, although the initial idea to do the experiment did not belong to him but rather to Rev. John Michell (1724–1793), astronomer and teacher of Geology at Cambridge. He was author of the following publications, among others: *Conjectures concerning the cause and Observations upon the phaenomena of Earthquakes* (1760), *Proposal of a Method for measuring Degrees of Longitude upon Parallels of the Equator* (1766). Laplace (1898, p. 443) called the Cavendish's experiment a 'beautiful experience'; J.H. Poynting (1852–1914) in *The Earth: Its Shape, Size, Weight and Spin* (1913) considered that the experiment was made 'in a manner so admirable that its marks the beginning of a new era in the measurement of small forces'.

'The experiment of weighing the world, or, equivalently, of determining the density of the earth, held immense appeal for Cavendish. It united the principle of universal gravitation – the fixed point of his Newtonian philosophy – with the sciences of geology and astronomy. These interests that had come, very probably as a result of his friendship with Michell, to occupy the first place in his research from the late 1780s on', writes McCormmach (1968, p. 154) in a well documented article about the personal and scientific relationships between the two friends and colleagues in the Royal Society, both elected in the year 1760. Their correspondence shows an evident interest in astronomy in the years 1783–1784, and in geology in 1788. Michell and Cavendish's dedication to gravitational studies is considered to be an important contribution to a unified world view from a Newtonian perspective. In addition, Michell designed a plan in 1784 to weigh the stars through the gravitational delay of the light emitted by them. He even hypothesized that a star with the same density as the Sun, but 500 times greater in diameter, would not permit its light to escape to infinity. Without going into detail here, it is worth mentioning that we are talking about something which more than a century later was expressed as 'the weight of light', confirming the predictions made by Albert Einstein (1879–1955) in his general principle of relativity. At the same time, Michell anticipated what are known today as black holes, 'a strange fruit of Einsteinian theory of gravitation' (Galindo, Moreno, Benedí & Valera, 1998, p. 296). Nevertheless, with respect to Cavendish's interest in the density of the earth, there were different opinions. One was of the English astronomer and mathematician Francis Baily (1774–1844) who, judging from the number of experiments done by Cavendish, 'only 23', compared to the 2,153 done by Baily himself, believed 'that Cavendish's object in drawing up his Memoir was more for the purpose of exhibiting a *specimen* of what he considered to be an excellent method of determining this important inquiry, than of deducing a result, at that time, that should lay claim to the full confidence of the scientific world' (Baily, 1842, p. 197).[13] J.L. Heilbron (1994), after quoting this commentary from Baily, points out, in spite of the satisfactory results achieved by Cavendish providing that the force of gravity follows the same law for long distances as well as for short ones, that he had not convinced his contemporaries, who still harbored doubts about the universality of Newton's law. In any case, Cavendish's experiment did not take long in becoming a paradigm for whomever later measured the density of the earth. Airy (1834) himself wrote:

> the most notable experimental demonstration of reciprocal attraction between the bodies consists of a series of experiments performed in the last century by Cavendish. (Airy, 1834, p. 34)[14]

As McCormmach (1968, p. 127) said, 'Weighing the stars and weighing the world were ambitious schemes', that had two perfect protagonists: Michell, considered to be one of the most brilliant natural philosophers of the eighteenth century, and Cavendish, to whom were attributed abilities similar to Newton. 'The two were set apart from their colleagues in having a mastery of both the mathematical and experimental sciences' (McCormmach, 1968, p. 127). It is believed that their friendship began at Cambridge. Cavendish was probably attracted by Michell's reputation as an excellent philosopher and, already interested in natural philosophy, he must have attended some of the Reverend's lectures. In 1760, the year of their respective nominations as fellows at the Royal Society, Cavendish was admitted to the Royal Society Club, a very restricted scientific society in which the members met once a week to dine together. On his frequent trips to London, Michell was a habitual guest of the Club. Nevertheless, there is no evidence of their relationship until later, when they corresponded during the period 1783–1788. Of the diverse scientific topics contained in their letters their shared interest in precision instruments stands out. They hoped to build on Newton's accomplishments following in the footsteps of the philosophy inherited from their illustrious countryman.

They occupied themselves not only with forces decreased by the inverse of square of the distance (gravitational, electric, and magnetic forces) but also with short-range forces. This is demonstrated in Cavendish's unpublished work[15] investigating mathematical properties of the forces in dynamics, heat and pneumatics. However, Cavendish did not come to any conclusions about the philosophy of the matter.

> When Cavendish's researches are viewed as a whole, certain features are characteristically predominant, notably a wide range of interests, a passion for accuracy, and a remarkable gift of making suitable choice of subjects worth

> investigating. Though his activities were almost wholly within the eighteenth century, his outlook would appear to have extended into the future. The words of Sir Edward Thorpe are significant: 'In most of his work his trend of thought seems to have straight towards the course of the subsequent progress of science'. (Berry, 1960a, p. 191)

Thus ends Berry's biography of the English aristocrat of whom, in the year of his death, Humphry Davy said:

> Since the death of Newton, if I may be permitted to give an opinion, England has sustained no scientific loss so great as that of Cavendish. Like his great predecessor, he died full of years and of glory. His name will be an object of more veneration in future ages than at the present moment. (Berry, 1960b, pp. 25–26)

But let us return to the historic experiment: 'If your health does not allow you to go on with that (the telescope) I hope it may at least permit the easier and less laborious employment of weighing the world', wrote Cavendish (27/5/1783) to Michell (quoted in McCormmach 1968, p. 153), lamenting his friend's illness, with a bit of humour, which in McCormmach's opinion, was the only example of jocular behaviour known in all his enigmatic life. Cavendish adds, 'for my own part I do not know whether I had not rather hear that you had given the experiment a fair trial than you had finished the great telescope'. The experiment to which Cavendish refers was the plan for 'weighing the world', which some time before the Reverend had told him about. In the end the telescope was finished and acquired by the astronomer William Herschel (1738–1822) at Michell's death. Nevertheless, 'weighing the world' would remain a glorious legacy for his friend and confident the *Honorabilis Henricus Cavendish, viri Honoratissimi Domini Caroli Cavendish Filius natu maximus*, as was written in the admission book in Peterhouse of Cambridge, the 18th of December, 1749, when Cavendish was 18 (Figure 3); he is called by J.B. Biot 'the wisest of the rich and the richest of the wise' (Jungnickel & McCormmach, 1999, p. 1).

To measure the density of the earth, Michell built a torsion balance that, by observing the attraction between small quantities of matter, was based on the principle suggested and used by him around 1768 (Mertz, 1896/1914). It was the same principle that Charles A. Coulomb (1736–1806) employed to measure weak electric attractions and repulsions.[16] It seems that Michell, when he conceived his experiment, did not know about the balance and Coulomb's experimental methods, according to Cavendish's affirmation in the article where he published the famous experiment that has gone down in history with his own name (Cavendish, 1798) and which is almost the only thing for which he is known. Likewise, in his early works, Michell, interested in the Newtonian principle of attraction, came near to establishing, for the first time,[17] the inverse-square law for magnetic attraction and repulsion.

Figure 3.

At Michell's death, the balance passed to the hand of Rev. Francis John Hyde Wollaston, Jacksonian Professor at Cambridge, and who had once provided Cavendish with data for his study on the height of the aurora borealis. Not considering Wollaston able to carry out Michell's experiment, he gave the balance to Cavendish. Using Michell's balance as a reference – because the original balance was deformed and had some problems in its design and materials – Cavendish built another, which he installed in a building in the yard of his house in Clapham. Of this house, it is said, only one woman had ever entered and in whose surroundings there walked at dusk such a strange neighbour, always alone and walking in the center of the path in order to avoid meeting other strollers (Aykroyd, 1935).

Worried over the accuracy of his measurements, Cavendish modified the balance in an attempt to avoid disturbances due to flows of air. Indeed, air currents became the main stumbling block to the experiment, followed by temperature and pressure changes, magnetic effects, and even electrization. In order to isolate the balance from the external environment, he placed it in a wooden case *gghhgg* (Figure 4) and calculated the gravitational effect the mahogany box might have on the suspended masses. Cavendish's balance consisted of two light metal masses

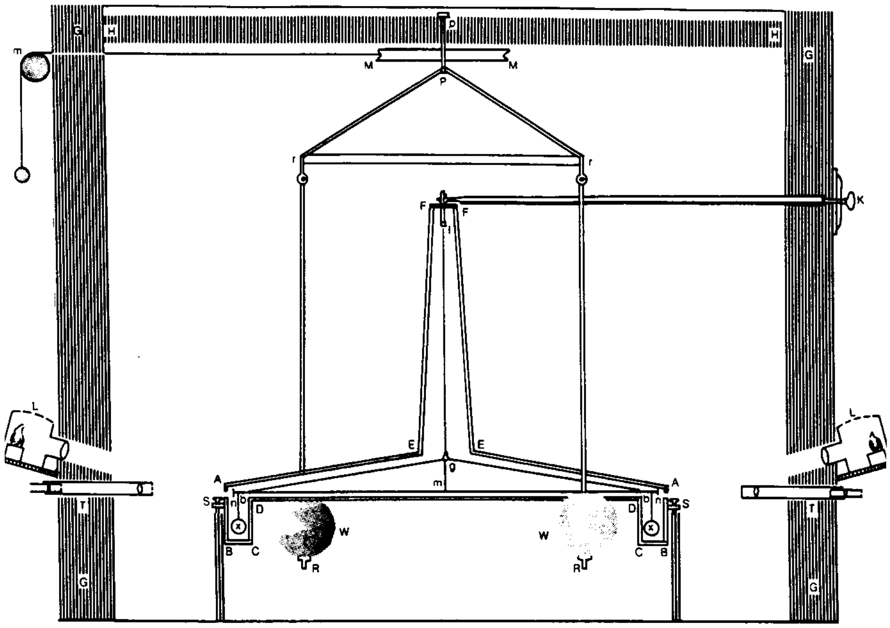

Figure 4.

x attached at the end to a rigid wooden structure $ghmh$, hanging from a thread lh that twists measurably during the experiment. Gravity pulls the lesser masses toward the large metal masses W, which are set near the balance and moveable by means of pulley M. In order to see the balance turn, Cavendish set up light beams from source L, mirrors n set at the end of balance beam hmh, and telescopes T. The principle of the method consists of measuring the angle of torsion of the horizontal bar $ghmh$. Describing his task, Cavendish noted:

> In order to determine from hence the density of the earth, it is necessary to ascertain what force is required to draw the arm aside through a given space. This Mr. Michell intended to do, by putting the arm in motion, and observing the time of its vibrations, from which it may easily computed (Cavendish, 1798, pp. 249–250).

From August 5, 1797 to May 30, 1798, Cavendish took 17 measurements and made the corresponding calculations laid out in the section *On the Method of Computing the Density of the Earth from these Experiments*. Although the details fall beyond the aims of this paper,[18] Cavendish did conclude that

> the density of the Earth is some 5.48 times greater than that of water. (Cavendish, 1798, p. 284)

A repetition of the measurements with a different torsion line produced analogous results. As cautious as Cavendish was with avoiding disturbances and respecting his predecessors' results, he concluded that

> According to the experiments made by Dr. Maskelyne, on the attraction of the hill Schehallien, the density of the earth is $4\frac{1}{2}$ times that of water; which differs rather more from the preceding determination than I should have expected. But I forbear entering into any consideration of which determination is most to be depended on, till I have examined more carefully how much the preceding determination is affected by irregularities whose quantity I cannot measure. (Cavendish, 1798, p. 184)

It is not known whether Cavendish repeated the measurements under improved experimental conditions as he aimed to do. In his *Scientific Papers* (note 15), which includes work unpublished until then (1921), there is nothing about the density of the earth, nor do those who have studied his life and work mention that his proposed revision was ever carried out.

5. The Anachronism: Measuring *G*, the Universal Gravitational Constant

'Newton did not fully grasp the notion of gravity at once, but it did start him off on a long and complex mathematical journey that ended with his law of gravity. Even after reaching this point, it still took another 100 years before the eccentric physicist Cavendish managed to determine the value of G, the constant of gravitation. Nonetheless, the fact that the formula was incomplete did not keep Newton from drawing all the attention he could to his formula'. This is said in a recent publication (Strathern, 1999, p. 28) and in many others, some of which I will subsequently be citing. What it means is that Cavendish determined the value of the universal constant of gravitation G, (sometimes calling it the 'Cavendish constant'), when what his work really did was, as we have seen, to provide a new methodology for measuring the controversial density of the earth. His method gained a reputation that sparked the interest of the scientific community to such an extent that it was repeated again and again throughout the 19th and 20th centuries, with only slight variations that detracted little from the original idea.[19]

The earliest instance I myself have found of the constant being included in the expression of Newtonian gravitational forces appears in the 4th edition of *Cours élémentaire de Physique* (Paris, 1844), by Nicolás Deguin; the constant of gravitation does not appear in the 1st edition of 1836. Nevertheless, it must have been taken into account in some intermediate edition, since the Spanish translation *Curso elemental de Física* (Madrid, 1841) by Venancio González Valledor, professor of physics at the Estudios de San Isidro in Madrid does use the constant of gravitation in calculating the density of the earth. In this translation, the constant is designated as f without being assigned any numerical value, and defined as 'the pull of one unit of mass at one unit of distance'. Moreover, he did not write

Newton's formula of universal gravitation in its currently acknowledged form, but rather only partially. He writes f M/R^2 for what was then known as force of attraction, represented by the letter g and corresponding to the 'speed achieved by a falling body at one unit of time due to its heaviness', i.e., what was later dubbed *gravitational acceleration*.

The appearance of the constant of gravitation G in the equation of Newton's law of gravitation came much later, which I can so far state as dating to 1873, in a paper presented to the Academy of Science in Paris. 'Détermination nouvelle de la constante de l'attraction et de la densité moyenne de la Terre' by A. Cornu and J. Baille (*Comptes Rendus*, 1873, 76(15), 954–958).[20] The aforementioned authors state:

> After Newton's discovery of the law of universal attraction, an experimental problem of great consequence was naturally posed to physicists and astronomers. That is, determining the numerical value of the constant that expresses the mutual attraction between two bodies of unit of mass each other at unit of the distance .... The determination of this constant designated by f holds particular interest to Astronomy. Indeed, Kepler's Third Law makes it directly possible to calculate the value of the total mass of two bodies acting one against the other, after determining two elements about their movement – the major semi-axis a of the orbit and the time T of one revolution – if the precise value of f is known, since the following relation applies:[21]
>
> $$\frac{a^3}{T^2} = f\frac{(m + m')}{4\pi^2}$$
>
> For lack of knowing this constant with sufficient accuracy, astronomers only determine the relationship between the masses of various elements in the solar system, either by doubling the application of the formula on planets with moons, or by calculating the changes in their orbits. The absolute value of the masses of heavenly bodies is necessary for determining their density, but is only possible by determining the absolute mass or the average density Δ of the Earth tied to the constant of attraction by the formula
>
> $$f\Delta = \frac{3}{4}\frac{g^2}{R}.$$

In the deduction of this expression, they apply Newton's law, written now as an equation known to all

$$F = f\frac{mm'}{r^2}$$

Accepting that the density of the earth is 5.48 as obtained by Cavendish, whom authors refer to as 'the illustrious English physicist', they deduce that '$f : g^2 = 0.0^{14}682$' (units of measurement being meters and grams; the number 14 meaning fourteen zeros before 682).[22]

After referring to the measurements of the density of the earth as done by Reich (1838, 1849) and Baily (1843), Cornu and Baille state that

> the importance of the question for Physics as well as for Mechanics and Astronomy seems to us to deserve new study in such a way that the experimental method can be applicable not only to measuring the constant of attraction, but also to a whole set of other physical determiners .... We have thus begun a thorough study on torsion balance, especially from the viewpoint of *absolute measurements*.

Such *absolute measurements* can only be obtained through mathematical equations, not proportionally as Newton proposed in his law of gravity. If, as some authors erroneously attribute to Cavendish, the constant G stands for the gravitational force exerted on units of masses set at one unit of the distance apart, the misanthropic Sir Henry could not fathom including G in Newtonian proportionality – 'closing it off' – and thus concluded with an equation that turned it into the knowledge of G, since G plays a dimensional role that was never taken into account in the laws of physics of that day and age, when no one yet had even defined what the units of force magnitude were.

Since it is not my aim to concern myself here with the constant of universal gravitation, represented by G at the end of the 19th century[23] and from here on in this paper as well, but instead with the anachronism involved in crediting Cavendish for its measurement, there is little need to dwell much further on the constant itself. Thus, I will keep to a few comments on the erroneous way the 'Cavendish experiment' is dealt with in textbooks and other publications on universal gravitation. Nor is it necessary to produce an exhaustive list of such anachronisms: it is enough just to leaf through the physics texts at hand to find that very few make mention of Cavendish's density of the earth, and that many contradict the facts outrightly by mistakenly attributing the famous experiment with the measurement of G as a prior step to calculating the mass of the earth: 'once the value of G has been determined, it can be used to calculate what is often called weighing the Earth' can be found in (Holton, Roller, 1972, p. 207); 'this experiment has been called 'weighing the earth'. Cavendish claimed he was weighing the earth, but what he was measuring was the coefficient G of the law of gravity. This is the only way in which the mass of the earth can be determined. G turns out to be 6.670 $\times\ 10^{-11}$ newton $\cdot$ m^2/kgm^2' (Feynman, Leigthon, Sands, 1971, chapter 7, p. 13); Segré (1983, pp. 153–154) writes, 'The outcome of this experiment provided the constant of universal gravitation that completes Newton's law on the inverse of distance squared so that gravitational acceleration g can be calculated to find the density of the Earth'. Examples such as these abound, and once the ambiguity is understood, the misunderstanding – and the anachronism – is cleared up. As we saw in Cornu and Baille, the relationship between the density of the earth and G is such that one can be calculated if the other quantity is known.[24] Perhaps this easy identification was what led to speaking about both almost indistinctly: most authors might prefer to refer to G, taking the density of the earth for granted. If this were so, nothing more than a warning would be in need.[25] Remember that before the 20th century, scientists were more interested in properties of the earth than in ‘fundamental constants’.

And once that matter is understood, an even more confusing one can be resolved regarding the undue priority given to Cavendish. For instance, the London Royal Society and Astronomical Society placed a plaque in Schiehallion to commemorate the 250th anniversary of Maskelyne’s birth. On the plaque, it says about the experiment: ‘At the time, this was called ‘the attraction of mountains’. In effect, it became the first determination of Newton’s universal gravitational constant’. This is utterly false, and any attempt to calculate G from the density of the earth determined by this experiment (some 4.5 times that of water) would have been overruled by Cavendish’s results.

Even if we accept that talking about the mass of the earth is the same as talking about G (a notion which I share, as long as we remain clear on the meaning and historical moment when each was determined), there are cases which, though done for the sake of simplicity, are completely inadmissable. For example: ‘Cavendish found K (as the author dubbed the constant of gravitation) to be $6.7 \cdot 10^{-8}$ dynes’ (Álvarez de Ron, 1932, p. 203); ‘The first experimental determination of G was done by Henry Cavendish in 1798 .... He used the law of universal gravitation and found the value of G to be around 10^{-10} N m^2/kg^2’ (Cetto, 1991, p. 286); ‘He (meaning Cavendish) found that the gravitational attraction between two one-kilogram masses at one meter apart is $6.67 \cdot 10^{-10}$ newtons. Cavendish determined the value of G mentioned in the preceding paragraph. After Cavendish’s successful measurement of the force of gravity, it became possible to find the mass of the Earth’ (Blanc, Fischler & Gardner, 1967, p. 132); ‘Having at his disposal a reliable value for G, Cavendish used it in the equation $G = gR_t^2/M_t$and solved for M_t : $M_t = gR_t^2/G$. His result for the mass of the Earth was almost as precise as his measurement of G’ (Orear, 1989, p. 103).

Similarly, we can find equally anachronic conclusions induced by the anachronic measurement of G, such as the following: ‘Let us re-consider the Newtonian gravitation equation in the light of this discussion. Cavendish, more than a century after Newton, performed an experiment to measure the force of attraction between pairs of heavy metal spheres, and confirmed the $F = G\,Mm/R^2$ relationship. Since the experiment was concerned with direct operational measures of the terms in the relationship itself, it seems reasonable now to call the relationship Newton’s Law of Gravitation (not *Theory*)’ (Gardner, 1975, p. 21). This idea, associated with the obvious fact that determining G completes the expression of the law of gravity homogenizing the dimensions of equality, may be in and of itself true as long as one accepts Gardner’s premises on the difference between *laws* and *theories*. Nevertheless, it is misplaced in time, and thus needs to be addressed. Putting two issues together at the same time when one did not really coincide with the other, and in fact was not even an object of consideration, is a falsification of history that, as we have seen, only leads to more errors. Those errors, like the one presented here, have decided effects on the epistemological aspects of the physical sciences.

6. Gloss: On the History of Science and its Consideration in Teaching

The detection of the anachronism outlined here got its start during a search through historical records for a teaching unit on 'Gravitational Interaction' to be included in a second-year high school text (*Física* 2° Bachillerato LOGSE, cited above). Its origin was thus meant for pedagogical ends in order to give a historical framework to the conceptions, models, and methods that were being developed in the physics 'of the heavens' from the earliest civilizations on up to Newton's momentous synthesis of *Principia* and their most immediate consequences.

My predisposition to the way history was dealt with in that lesson was neither by chance nor by a quirk in methodology. I have been doing the same since the long-ago times of being a school teacher. At that time, and for years afterwards, hagiography held more sway than history. The lack of the kind of scholarship on the pedagogical possibilities of history we have today meant that history has no more support than the teacher's own esteemed but limited intuition. While looking for more reasons to support or reject such a view, I found further examples of how the history of science has often been distorted in education. Although abridged and perhaps familiar to many, the following considerations should be kept in mind.

It is worth noting that the Pidal Plan of 1845, drawn up by the liberal progressive politicians Gil de Zárate, Revilla, and Guillén, included *History of Sciences* in the required coursework for doctorates. That plan formed the basis for the creation of the Secondary Education Institutes in Spain, and has been noted as one of the most advanced steps in secularizing the education system.[26] The high regard held for the history of science is a case in point, as were others later on. An even stronger proposal comes from the 'Questionnaires of Professional Grade Normal Schools in Primary Education' (*Boletín*, 1932). In its section on Methodology in Mathematics, it states:

> Nor should we leave out a bit on the history of the science we study, since the evolution of Mathematics over the centuries has been the same as the evolution of our spirit, making it worthwhile to study the History of Mathematics as an increase in our students' cultural level as well as a necessary basis for the study of its Methodology.

This view is handed down from the doctorate level to the level of teacher training, and then on down to primary and secondary school students, as can be seen in the Renewed Programs of EGB (Spanish public primary school) of 1981 and in the current curriculum design for high schools.

Learning more of what went on 'elsewhere' (meaning abroad), which in my case came late and gradually, only reinforced my belief in historical resources, not anecdotally or as something scribbled in the margins or in captions under illustrations, but as part and parcel of curriculum content. Publications in the form of dialogues or Galilean confessions on the historical and philosophical aspects of science gave way to more formalistic and structured texts and reference books in the classroom, where history became a subject for study and subsequent examination. Among them are: Hogben, L. (1938), *Science for the Citizen: a Self-Educator based on the Social Background of Scientific Discovery*, London, George Allen; Taylor, L.W. (1941) *Physics: the Pioneer Science*, USA, Cambridge University Press; Swenson, H.N. and J.E. Woods (1957), *Physical Science for Liberal Arts Students*, New York, John Wiley. And especially, the works of James B. Conant (1893–1978), *On Understanding Science: An Historical Approach* (1947), and *Harvard Case Histories in Experimental Science* (1957), in collaboration with Nash and the Rollers, who with Gerald Holton co-authored *Fundamentals of Modern Physics* (1958), considered as a revised edition of Holton's *Introduction to the Concepts and Theories of the Physical Sciences* (1952), both of which were available in translation and well-known among Spanish teaching faculties interested in the history of science. Such books were antecedents to *Harvard Project Physics*, which Rutherford, Holton, and Watson began in 1964 and released in 1970 with the following three aims: (1) To design a physics course with a humanistic orientation; (2) To develop a course that draws a large number of higher secondary students to introductory studies in physics; and (3) To contribute to the knowledge of the factors influencing the learning of physics. These views were clearly shared by those who were searching for an approach to teaching science from a historical perspective.

One of the first evaluations done on the application of historical case studies on the teaching of sciences is *The Use of Case Histories in the Development of Student Understanding of Science and Scientists*, written in 1961 by Leopold E. Klopfer and William W. Cooley at Harvard University in order to win a contract for the United States Department of Education. They concluded that 'the method is definitely effective in raising the students' knowledge of science and scientists when it is applied to biology, chemistry, and physics classes in higher secondary education' (from typewritten paper). The results from the first applications of the Harvard Project were published in *A Case Study in Curriculum Evaluation: Harvard Project Physics*, of which I learned through commentaries made in Welch and Walberg (1972). The two writers compare traditional physics courses with PSSC (Physical Science Study Committee) projects and Harvard based on the assessment of the latter. Overall, both the traditional and PSSC programs are rated as more confusing and, in a way, more boring than the Harvard's, where 'students assess physics as more historical, humanistic, philosophical, social, artistic, and less mathematical and applied than the physics seen in the other courses'. This conclusion requires avoiding the risk of taking a historical perspective at the cost of the conceptual, theoretical, experimental, and inevitably mathematical nature of physics.

One example of the interest in making pedagogical use of the history of science can be found most recently in the activities developed from the Interdivisional

Group on the History of Physics of the European Physical Society Teaching Group and the International History, Philosophy and Science Teaching Group, who periodically hold conferences whose *Proceedings* record the multiple remarks made regarding the matter at hand. Another invaluable reference work, as much for its own content as for the profuse bibliographic information is Matthews (1994), as are the collected articles in Shortland and Warwick (1989). Similarly worthy of mention is the contribution to the history of science from periodic publications such as *Isis, British Journal for History of Science, Enseñanza de las Ciencias, Alambique, Science & Education: Contributions from History, Philosophy, and Sociology of Science and Mathematics*. This literature can be accessed through the *Isis Current Bibliography*, which is available back to 1975 in the electronic database RLIN. Also, the graduate and doctorate programs in institutions of Portugal and Spanish speaking countries are worthy of mention.

Still, despite the contributions made to the pedagogy of the history, philosophy, and sociology of science so far, the pickings are still slim, and rarely are they integrated into the real process teachers and students face in the classroom. One initial requirement that might be made is to keep as scrupulously close to the historical facts of each moment as possible.[27] I stress this requisite – above others that I could readily point out but instead omit here as being beyond the scope of this paper – in order to underscore this work from a pedagogic point of view rather than merely a historical one. The anachronism that has emerged, particularly in text books, regarding the constant of universal gravitation in Newton's law is an example, albeit an occasional one, of what amounts to the twisting of history. To avoid such misinformation, it is advisable for everyone involved in devising pedagogic activities to cross-check the historical references, or at least proceed with caution. Most helpful would be to view the process of configuring science as a human and thus social activity, with some specific rational characteristics and others considered as far out of what 'science' has been understood as over the centuries.[28]

Such strict historical accuracy is clearly not always within everyone's grasp, and even for those with more readily available sources the task is not easy. For that reason, it would be of the utmost benefit to provide support material to programs of both pedagogic research in this field as well as accredited curriculum design, where such reference material has been seriously lacking, particularly in Spanish-speaking countries. What is needed are translations of the main works of science, landmark experiments, historical readings, sensible biographies (but not the kind of hagiographies that turn science and scientists into a deformative list of the holy and blessed, so to speak), studies and other publications on the still rather nebulous interrelationship between science and society as a discipline or, more aptly, a field of knowledge. Without such direly needed translations and resources, it is impossible to view science in all its complexity, and citizens are needlessly deprived of part of their own cultural heritage, in which science is most certainly included.

Acknowledgments

This research was made possible by a grant from the Dirección General de Investigación Científica y Técnica de España (PR 95-084) for a stay in the University of New South Wales, Sydney, Australia, in 1995.

Notes

[1] Greenberg (1995) analyzes the eclectic position of French physicist Jean-Jacques Dortous de Mairan (1678–1771), who sought to reconcile Cartesian and Newtonian systems, although with a markedly Cartesian tendency, and the Newtonian-based rejection of the same by John Theophile Desaguliers (1683–1744), French by birth but raised in England after his family fled from religious persecution. His work is a well-documented study of the Newtonian-Cartesian controversy that has affected the perception of his scientific, philosophical, and even nationalistic views. Greenberg concludes that 'the papers by Mairan (Memoria read at the Academy of Paris in 1720) and Desaguliers (three published in *Philosophical Transactions*, 1725) involve matters and raise issues that are frequently misunderstood by historians' (Greenberg, 1995, p. 78). Nevertheless, there are contradictory opinions about the admission the action-at-a-distance by Newton; for example, (Heilbron, 1979) considers that Newton did not believe in action-at-a-distance but rather, that it was a misinterpretation by his readers.
[2] 'It was believed' – writes D'Alembert in the article 'Figure de la Terre' in *l'Encyclopedie* – 'that, a modern author states, a nation's honor was at stake if they allowed the earth to be given a strange shape, a shape dreamed up by an Englishman [Newton] and a Dutchman [Huygens], when it had long been a matter of national honor to defend the whorlwinds and subtle matter [of Descartes] and outlaw Newtonian gravitation. Paris and the Academy were divide between the two sides' (Lacombe and Costabel, 1988, p. 14). In the same volume, Chouillet (1988), it is stated that 'The matter in question was not trivial, since it meant the victory or defeat of Cartesian science, and that, beyond the problem of the pushed-in poles, the whole depiction of the world depended on its solution: a full world? An empty world? Sides had to be chosen' (Lacombe and Costabel, 1988, p. 187).
[3] Jacques Cassini was the son of Gian Domenico Cassini (1625–1712), a man who in 1683 initiated measuring the arc of latitude along the meridian over Paris, from Paris to Colliure; he firmly believed in an elongated earth, radically opposed Newton's gravitational theories, and rejected Kepler's eliptical orbits of the planets. Instead, he considered the orbits to be ovaloid, and has thus gone down in history for 'Cassini's ovals'. Jacques gave a detailed account of that opposition to Newton in *De la grandeur et de la figure de la terre* (1720). In contrast, his son Cesar-Françoise Cassini de Thury (1714–1784), a.k.a Cassini III (who began the first topographical map of France, finished by his son Cassini IV), dropped the 'family rejection' of Newtonian gravitation and accepted an earth flattened at the poles.
[4] Other contemporaries of Newton's who supported an prolate earth were the English theologian Thomas Burnet (1635–1715), author of *Telluris theoria sacra* (London, 1681); Alsacian mathematician Johann Gaspar Eisenschmidt (1656–1712), author of *Diatribe de figura telluris ellipticospheroide* (Strassburg, 1691).
[5] We should recall that Huygens's 'Discours de la cause de la pesanteur', published with *Traité de la lumière* (1690) coincides with Newton in that the earth bulges at the center and is flattened at the poles, but he supported the existence of Cartesian-like vortices upholding the existence of central forces of attraction, in rejection of Newtonian actions at a distance according to an inverse-square law of the distance (Huygens, 1992).
[6] Regarding the appropriateness of opting for degrees of meridian instead of degrees from the equator, see the speech given at the Public Assembly of the Academy of Sciences in Paris (Nov. 14, 1744) by P. Bouguer, titled 'Relation abrégée du voyage fait au Pérou par Messieurs de l'Académie

royale de Sciences pour mesurer les Degrés du Méridien aux environs de l'Equater et en conclure la Figure de la Terre' (Lamontagne, 1964b).
[7] Maheu (1966). This publication presents three letters from Bouguer to Euler with notes by Roland Lamontagne. They contain commentaries on the abbot of La Caille's (1713–1762) geodesic expedition to the Cape of Good Hope in order to measure the degree of meridian, which once again reinforced the flattened-poles view.
[8] The precise measurement of time greatly aided the emergence and development of modern physics. The use of the pendulum for measuring time was by no means precise. 'The method used to set up a pendulum clock (whose sweeps count the seconds) necessarily presupposed setting true solar noon by observing the sun's passage through the meridian of that location. To do so, they measured the time interval between the sun's passage through two points set on either side of the meridian, and whose height above the horizon was the same. This method is called that of *corresponding heights*. Let H_e and H'_e be the times marked by a pendulum when the sun is at the same height. Then, if the declination were constant, high noon would be determined by the formula $H_m = H_e + (H_e - H_e)/2$' (Lafuente and Delgado, 1984a, pp. 167–169). Bearing in mind that the earth's declination is variable, that temperature influences the length of the pendulum, and that the height above sea level affects the period of the swing, one would have to apply the corresponding corrections to the formula in order to get a precise measurement of time. Needless to say, the simple pendulum was soon discarded as a timepiece. For more information on how the expedition in Peru went about measuring time, see Marquet (1988).
[9] In *La Figure de la Terre* (1749), Bouguer reveals his observations and measurements made in Chimborazo on the phenomenon called 'the attraction of the mountains'. La Condamine had done the same in 'Letter à M. Dufay sur les observations faites à *Chimborazo*, Montagne de la Province de *Quito*, pour reconnaître par Expérience l'effect de l'attaction Newtonienne' (1738).
[10] *Phil. Trans.*, **65**, 1772, 495–499. Maskelyne had already looked into matters on the measurements of arcs of meridian years before. The 'Introduction to the following Observations, made by Messieurs Charles Mason and Jeremiah Dixon, for determining the Length of a Degree of Latitude, in the Provinces of Maryland and Pennsylvania, in North America' (*Phil. Trans.* **58**, 1768, 270–328), besides containing commentaries on those geodesic observations, includes a Post Script on the possible equivalences between French and English units of measure and reports on the results obtained from expeditions to Lapland, Peru, and the Cape of Good Hope and those obtained in France and Italy. He closes his comments on the differences by alluding to the Honorable Mr. Henry Cavendish, who, after close study, agreed that the measurements 'could be slightly affected by the attraction of the mountains'. For more details, see Cope and Robinson (1952).
[11] Among others, Poynting, J.H. and Thomson, J.J. (1924), *Text-Book of Physics*, London, Charles Griffen and Company; Galindo, A, Moreno, A., Benedí, A. and P. Valera (1998) *Física*, 2° Bachillerato LOGSE, Madrid, McGraw Hill.
[12] The authors announced the little-successful experiment in a private circular titled *Account of Experiments made at Dolcoath Mine, in Cornwall, in 1826 and 1828, for the purpose of determining the Density of the Earth*, reported in Todhunter, I., *William Whewell: an Account of his Writings*, Vol. 1, London: Macmillan (1876). The experiment was done in Cornwall because that was where the Royal Geological Society had been created in 1814 'to cultivate and spread knowledge of geology and minerology'. As was common at that time, debates were held there between Neptunians and Plutonians on the composition of the earth. The controversies seem to have been influenced strongly by the geology lessons given by the renowned scientist Sir Humphry Davy at the Royal Institution in London between 1805 and 1811 (Ospovat, 1978).
[13] Baily (1824) describes the extreme precautions to take when using the torsion balance, some of which were used by Cavendish, to obtain reliable results. Out of the 2004 experiments, he estimates 5.67 as the most acceptable value for the density of the earth. An interesting correction of Baily's measurements, not because of the numerical value itself (in terms of my aims in this paper, at least), but on account of the people who made them and their relationship with the anachronism I will

describe below, can be found in Cornu and Baille (1878).

[14] Airy (1857) comments on the results of his experiments. A version regarding the uneven results of these measurements (between 4.7 and 6.6) is given by Jacob (1857). Also noteworthy for measuring the density of the earth was the minerologist Ferdinand Reich (1799–1882), a professor of Natural Philosophy at the Freyburg Academy of Mining (Saxony) who claimed to have followed Cavendish's method by setting up a balance in the basement of the Academy of Mining (Reich, 1837).

[15] Cavendish, H. (1921). *Scientific Papers*, Vol. II, *Chemical and Dynamical*, Edward Thorpe (ed.), Cambridge University Press. This volume contains Cavendish's unpublished works, from original manuscripts belonging to the Duke of Devonshire, with commentary by Joseph Lamor (Mathematics and Dynamics), Archibald Geikie (Geology), Frank W. Dyson (Astronomy) and Charles Chree (Magnetism). Volume I, *The Electrical Researches*, published that same year, is the revised edition by J. Lamor of the prior one by James Clerk Maxwell in 1879. Maxwell (1831–1879) was the first to take a chair in experimental physics at Cambridge University, where he organized the famous Cavendish laboratory with a generous endowment from the excentric Englishman. On Cavendish's peculiar life and work, I have been able to consult (aside from other minor biographical references): Berry (1960), Young (1921), Cuvier (1961), Jaffe (1931), Crowther (1962) and, especially, Jungnickel and McCormmach (1999).

[16] Coulomb's early research on torsion dates to 1777, in a Memoria awarded by the Academy of Sciences in Paris. In 1781, upon being elected Member of the Academy, he gave a speech on torsion and elasticity of metal wires. It was in 1785 that he read another Memoria at the Academy announcing his well-known law: *Construction et usage d'une Balance électrique, fondée sur la propriéteéqu'ont les Fils de métal, d'avoir une force de réaction de Torsion proportionelle à l'angle de Torsion*. See Gillmore (1971) and Crowther (1962). Aiming at describing electrostatic phenomena by means of Newtonian-like mechanical models, Cavendish established the inverse-square law of the distance for electrical attractions and repulsions, in research unknown until its publication by J.C. Maxwell (note 15). A preview of his interest in such phenomena can be found in the article 'An Attempt to Explain some of the Principal Phaenomena of Electricity by means of an Elastic Fluid', *Phil. Trans.* **61**, 1771, 584–677. For accounts of these events, see Bauer (1949) and Dorling (1974).

[17] This is affirmed by Hardin (1966) based on the commentaries on Michell's publication dated 1750, *A Treatise of Artificial Magnets; in which is shown an easy and expeditious method of making them superior to the best natural ones*.

[18] Of Spanish language publications, one simple way of finding out about the theory of torsion as applied by Cavendish is in the text *Física* 2° Bachillerato LOGSE (note 11); and developed in greater detail, even including part of Cavendish's original work translated into Spanish, in García Sanz, J.J. (1998), 'El Experimento de Cavendish', *Revista de la Facultad de Ciencias*, no. 1, Madrid, UNED.

[19] For an inkling of the credit given to Cavendish's experiment, I will cite others done in the 19th century to the same ends and with similar torsion balances: 1837, Reich; 1841, Baily; 1842, Reich; 1872, Cornu and Baille; 1894, Boys; 1894, Braun; 1896, Eötvös; 1901, Burgess. Some are commented on in this paper. Furthermore, the constant of universal gravitation G was and still is being measured by repeating Cavendish's experiment, making the pertinent modifications. Many of these measurements are reported in: *Proceedings of the Second International Conference Held at the National Bureau of Standards*, 'Precision Measurement and Fundamental Constants II', Taylor, B.N. and W.D. Phillips (eds) (1984), Washington: U.S. Government Printing Office. Also in Kestenbaum (1998), Schwartz et al. (1998).

[20] The optometrist Marie Alfred Cornu (1841–1902), educated at the École Polytechnique and later professor at the School of Mining in Paris, made use of the torsion balance along with J.B. Baille: 'Étude de la résistence de l'air dans la balance de torsion', *C. R.*, 1878, **86**(9), in which he and Baille insist on the measurements needed to avoid 'accessory influences' in measuring the density of the Earth by 'the Cavendish method'. Also, 'Influence des termes proportionels au carré des écarts, dans le mouvement oscillatoire de la balance de torsion', *C.R.*, 1878, **86**(16).

[21] This expression is what is currently known as Kepler's harmonic law or 1-2-3 law, usually written

as $G(m_1 + m_2)^1 = \omega^2 a^3$ to relate the major semiaxes (a) to the orbital period $T = 2\pi/\omega$ in the relative mouvement of two bodies m_1 and m_2.

[22] The authors insert this footnote: 'In effect, if Newton's law is applied to any two bodies whose masses are m, m', then $F = fmm' : r^2$; if one of them is the earth, then $p' = fMp' : gr^2$ or $Ff = gR^2 : M$. If we compare the mass of the earth M with that of an equal volume of water, and call the result Δ, the aforementioned formula is obtained'. In this comparison, they identify the volume of water with its weight, thus being $\Delta = M : V/g = M : 4\pi R^3/3g$. To obtain the relation $f : g^2 = 0.0^{14}682$, one must take the density of the earth obtained by Cavendish as $\Delta = 5.48 \cdot 10^6$ g/m^3. The value of g in Paris, given in Deguin's book cited in the paper by Cornu and Baille, is 9^{m}. 8088 (m corresponds to meters, recalling that the units of measurement still had not been included into the dimensional form adopted at the end of the 19th century). Cornu and Baille refer to a 'new determination of the constant of attraction'. I do not know what measurement they are referring to here, nor who it was that took it, since nothing is cited in the Memoria. A succinct account of the more than 20 laboratory measurements of G to date can be found in Chen and Cook (1993).

[23] This is the symbol appearing in Poynting (1892). The first use of G is most likely earlier, but I have not yet had the opportunity to consult Poynting's references to V. Jolly, *Wiedemann's Annalen*, Vol. 5, p. 112 and Vol. 14, p. 331; Koening and Richraz, *Nature*, Vol. 31, pp. 260 and 475; and a 'recent' publication by C. V. Boys of which he does not give full reference. Boys uses the symbol G in a later publication than Poynting's, where he compares his own results of determining the density of the Earth with those of Cornu and Poynting, in (Boys, 1895). Poynting had by then already used a chemical balance made in London by Oertling for the same purpose (Poynting, 1879).

[24] In the cited article (note 23), Poynting used a common balance for his experiments set up in the Cavendish laboratory in Cambridge, 'thanks to the kindness of Professor Clerk Maxwell', later moving it to Mason College in Birmingham, where the research was finished. The theoretical foundations of the experiment are based on the relationship for the average density Δ of the Earth: $\Delta = gR^2/GV$, with R and V being the radius and volume of the earth, respectively. In the textbook (Poynting and Thomson, 1924), a numerical case is developed, and the authors conclude that 'This example shows that the two problems, the determination of the gravitation constant G and the determination of the density of the earth Δ, are practically one ...'. In the section 'The Cavendish Experiment', pp. 36–39, they show a very accurate simplification of the procedure from calculating the density of the earth as done by Cavendish.

[25] A few examples of such a warning are: text and articles cited in note 18; Clotfelter (1987), in which, after describing Cavendish's experiment on determining the density of the earth, the author wonders, 'If Cavendish, or one of his contemporaries, had wished to calculate a gravitational constant, how could it have been expressed?' Such calculation, as Clotfelter acknowledges, was impossible at a time when weights and masses were measured in grains, since there were as yet no units of force; section 8.2.4, 'Cavendish y el peso de la Tierra', in Fernández-Rañada (ed.) (1993), and more recently, (Lally, 1999), who, after explaining the anachronism, finishes by saying 'In any event, it is clear that those who consider Cavendish the first to calculate G (or the mass of the earth) have simply not read his work. In 1798, the time was not appropriate for making any such calculation'.

[26] Moreno (1988) analyzes the process of secularization in schools in Spain, where cumbersome reforms in the way in which classrooms treated philosophy and the negligible contents of physics in the curriculum greatly affected any modernizing of scientific scholarship in both academics and research.

[27] Another example of historical accuracy is in Lelong (1998), on the discovery of the electron. 'All these elements' – Lelong concludes – 'contributed to the late emergence of the *discovery of the electron*. To a certain extent, it became a founding myth. A myth to be discarded nowadays if we are to understand how the electron came to shape our world'.

[28] Izquierdo, Sanmartí, and Espinet (1999), regarding the current change in the traditional model of science in which the *scientific method* – charged with a high dose of Baconian spirit – was identified with *rationality*, and *rationality* was equated with *science*, as the most powerful attribute

of the mind, state: 'These changes in focus have affected the concept of *scientific rationality* and *scientific method*. For that reason, new models of sciences have emerged in reference to moderated, contextual, or hypothetical rationality to explain how scientists advance the process of scientific creation. This new model of rationality stresses the human, tentative and constructive aspect of science'. An allegedly infallible rationality is what lies behind appeals to *scientification* in, for example, advertising messages designed to catch gullible and unsuspecting buyers by means of statements such as 'Wipp Progress detergent, with active oxygen', 'Dercos Anti-balding formula with aminexil acts to stop capillary fibrosis', and all the other examples based on the ignorance of the consumer, who is persuaded that any product touched by the 'divine hand' of science must be good. And even more disturbing is the arrogance of renown intellectuals in their promiscuous use of 'mystification, deliberately obscure language, misleading ideas, and misuse of scientific concepts', as Sokal and Bricmont (1999) denounce as a parody that is 'quite frequent in postmodern and cultural studies circles'. Such a parody brings to mind Colonel José de Cadalso (1741–1782), who, as a gift to 'those who wish to know much by studying little', published in Madrid in 1772 *Los eruditos a la violeta, o curso completo de todas las ciencias, dividido en siete lecciones para los siete días de la semana (High- brow Erudites, or a complete course in all sciences, divided into seven lessons for the seven days of the week)*; and *La derrota de los pedantes (The Fall of the Pedants)*, published in 1789 by Leandro Fernández de Moratín (1760–1828), 'a satire against the uncouth wits who at that time monopolized stage and book shops alike'. Whatever the period of time, the solution proposed has always been the same: to provide adequate schooling in science, in the many ways science has been understood throughout time.

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