INSTITUTE OF PHYSICS PUBLISHING METROLOGIA

Metrologia **41** (2004) S107–S114 PII: S0026-1394(04)83190-6

Charles Augustin Coulomb and the fundamental law of electrostatics

Isobel Falconer

Lumbo Farmhouse, St Andrews, Fife, KY16 8NS, UK

Received 9 April 2004
Published 16 September 2004
Online at stacks.iop.org/Met/41/S107
doi:10.1088/0026-1394/41/5/S01

Abstract
In his famous experiment on the inverse square law of electrostatics, Coulomb neither defined electric charge nor gave reliable measurements of the force–distance relation. Yet the experiment has often been viewed as the basis of the fundamental law of electrostatics. This paper discusses Coulomb's life, showing the context within which he was working, how he arrived at the experiment, and the use he made of it. Physics in France in the late 18th century was undergoing a transformation from a science of holistic observation and explanations to one of universal laws and exact measurement. Coulomb was both a subject of, and an important contributor to, this change, and these two aspects are evident in his approach to the experiment and to the later uptake of his results. The reaction in the rest of Europe was initially less favourable, and the ultimate fame of Coulomb's experiment was dependent on the triumph of French mathematical physics in the 19th century.

How is it that an experiment that neither defined electric charge nor reliably measured the inverse square law became the foundation of the theory of electrostatics? 'It is to . . . Coulomb that one owes the renaissance of true physics in France, not a verbose and hypothetical physics, but that ingenious and exact physics which observes and compares all with rigour.' wrote Biot ([1] p 230), placing Coulomb squarely in the context of the gradual triumph of a physics of universal laws and exact measurement over one of holistic observation and explanation in France in the late 18th century. The experiment that, more than any other, has often been viewed as signalling this triumph in the electrical sciences is Coulomb's of 1785 on the inverse square law of electrostatics, and understanding its context is essential to assessing the experiment and its impact.

Coulomb's father, Henry, had abandoned the army for a career as a petty Government administrator and was posted to Angoulême in south-western France, where Charles Augustin was born on 14 June 1736. Coulomb's mother, Catherine Bajet, had wealthy relatives, the de Senac family, and evidently some independent means of her own. Coulomb had two elder sisters, but hardly anything is known of his family life [1].

While Coulomb was still a child, his father was posted to Paris, where his mother, who wanted him to become a doctor, arranged for him to attend lectures at the Collège Mazarin. The Collège, the most prestigious of Paris' secondary schools, educated a number of noted French scientists, including Lavoisier, d'Alembert, and Legendre [2]. Coulomb must have been one of the many unofficial students, for his birth was not sufficiently aristocratic for full admission. When Charles Pierre Le Monnier, the astronomer, began lectures at the Collège Royal de France, Coulomb went to these as well. This may be where he received his grounding in newtonian mechanics, for Le Monnier had accompanied Maupertuis and Clairault on their expedition to Lapland to measure the length of an arc of a meridian, testing Newton's prediction that the Earth was flattened at the poles [3]. Le Monnier subsequently worked extensively on lunar motion and published an important manual of astronomy based on newtonian theory [4]. Coulomb was converted to astronomy and mathematics, quarrelled with his mother, and joined his father, who had lost most of his money in financial speculations and retired to his home town of Montpellier in southern France, where his family were prominent in the administration and politics of the Languedoc region. In March 1757 Coulomb was elected an 'adjunct' member of the Society of Sciences in Montpellier and read several early papers there on mathematics and astronomy. His position paid no salary, although there may have been a small fee for attending meetings, but he benefited from introductions to d'Alembert, Le Roy, and Le Monnier when he returned to Paris the following year to study for the entrance exams to the engineering school at Mézières.

Not well enough off to pursue science as a hobby, yet sufficiently a 'gentleman' to be looking for a career with professional status, there were few openings available in the 18th century for a man like Coulomb. However, France, ahead of any other country in Europe, did have a dedicated military engineering unit, the Corps du Génie, and a special training college, the École du Génie at Mézières, which was founded in 1749 and was already a raging success [1]. The Corps had become a distinctive institution, with its own officers and customs, and acted to some extent as a focus for research in engineering and a training ground for engineers, as well as applying itself to military construction, much of it innovative. This Corps Coulomb now determined to enter. It was an inspired choice: his experience with the Corps formed the basis for much of his later scientific work—it is noteworthy that his research on electricity and magnetism arose out of instrumental concerns based on his engineering experience—and, importantly, provided him with a position and salary for much of his life. The first hurdle, however, was the entrance exam, towards which Coulomb studied for nine months. He took the exam in 1759 and entered the École du Génie in February 1760 [1].

The course at Mézières was half theoretical, half practical. The practical included overseeing local building projects and rounding up labour gangs! The theoretical taught Coulomb to approach engineering from the principles of analytic mechanics, contrasting with the more empirical approach of, for example, the British [5]. Coulomb formed two lifelong friendships here, with a fellow student, the mathematician Jean Charles Borda, and with his mathematics teacher, Charles Bussot. Nominally, the mathematics syllabus covered the arithmetic, geometry and mechanics of Camus' *Cours de Mathematique*, but this was coming to be felt inadequate and Bussot extended it to calculus, perspective geometry, dynamics, and hydrodynamics [1]. Heilbron [3] has suggested that Bussot provided Coulomb with a role model. Bussot, a former student and collaborator of d'Alembert's, was a correspondent of the Paris Académie Royale des Sciences, entered prize competitions set by the Académie and won one in 1762 with research performed while Coulomb was his student, obtained a position in Paris, and was elected a member of the Académie in 1768. Coulomb's career was to follow a very similar path.

Less influential was Nollet's lecture course, which started at Mézières in 1761. Nollet had acquired fame for his lecture demonstrations, particularly the (perhaps apocryphal) entertainment of Louis XV by passing the discharge from a Leyden jar through a chain of 180 monks, who all jumped. He was a leading French exponent of a Cartesian view of electricity, conceiving all forces in terms of direct contact between corpuscles of matter in motion and discounting the idea of attractive forces as anything other than a secondary effect. He suggested that electrified bodies were surrounded by electrical atmospheres consisting of electrical matter in motion. Unlike many Cartesian theories of action, which depended on vortex motion, Nollet's electrical effluent streamed out as jets from pores in the surface of a conductor, explaining repulsion. Attraction was due to a second flow, the 'affluent', that emanated from all the surrounding bodies, including the air, and moved in towards the electrified body with an approximately isotropic distribution (see figure 1). This theory seems to have made very little impression on Coulomb, who adhered to newtonianism and action-at-a-distance theories throughout his life.

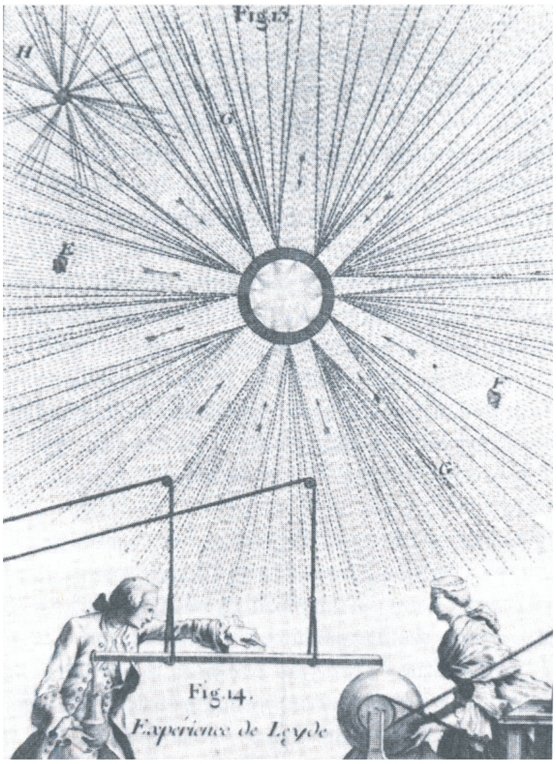

Figure 1. Nollet's system of electric forces showing the jets of effluent and the isotropic affluent. Below is a picture of a Leyden jar experiment [8].

Coulomb graduated in November 1761, his graduation report judging that 'he has a certain intelligence, but not that which will make him advance in the Corps' ([1] p 17). Despite this lukewarm praise, he was one of three, out of a graduating group of eight, to receive a cash bonus and be recommended for transfer away from Mézières rather than kept working locally under supervision. He was sent to Brest, from whence he was posted, in an emergency as the only engineer readily available when a colleague fell ill, to Martinique in the West Indies in February 1764. Martinique had a chequered history, having been claimed by France in 1658 but subjected to frequent attack by the Dutch and English, to whom it fell in 1762. It was returned to France the following year under the terms of the Treaty of Paris. A team of engineers were charged with securing the island; the death toll from illness was running at 60% to 70%, and Coulomb's own health never fully recovered. Coulomb, young and inexperienced as he was, ended up in charge of building the new Fort Bourbon, with 1200 men under him and at a cost of six million livres (his salary as a student had been 600 livres, his pension on retirement was 2240 livres).

The practical experience Coulomb gained in Martinique formed the basis of much of his later work on applied mechanics, including the first memoir he sent to the Académie Royale des Sciences on his return to France in 1772, *Sur une applications des régles, de maximis et minimis á quelque problèmes de statique, relatifs á l'architecture*, which is often considered to have laid the foundations of soil mechanics, investigating problems of friction and cohesion and developing a theory of the flexure of beams and the rupture and shear of brittle materials. Despite his many protestations of the practical use of his work, however, Coulomb's methods were little taken up by practising engineers at the time; he provided methods of determining stability, but no definite rules, and produced none of the ready compiled tables that were the customary basis for design [1, 7]. As a practising engineer himself, Coulomb must have known this, and it is worth remembering that this memoir was written to impress the Académie des Sciences, a body in which the rhetoric of utility typically validated a more theoretical approach that lacked the detail necessary for practical application [8]. More avowedly utilitarian than the Royal Society of London, yet similar in its lack of input from practitioners, the Académie contained only one or two men who inclined to a more realistic technological approach; Coulomb's work was in line with that of other 18th century mathematicians engaged in engineering problems, such as Euler and Daniel Bernoulli [7], and succeeded in its object to the extent that Coulomb was named 'Bussot's Correspondent to the Académie des Sciences', allowing him to communicate scientific news and the results of his own research through Bussot to the Académie. In practice, Coulomb began to cultivate the Academicians, visiting Paris whenever he could and presenting his papers in person [1].

The Académie Royale des Sciences was the model for many of the learned academies dedicated to the advance of science and technology that sprang up in the 18th century. They existed in a complementary relationship with the colleges and universities, characterized by Heilbron ([3] p 129) as 'the one taught the known, the other explored the unknown'. Founded in 1666 by Louis XIV, the Académie was, from the first, intended to be an instrument of the State in researching, validating, and promoting useful science and technology. Membership was limited to 44, all of whom had to reside in Paris and were expected to attend two meetings a week, at each of which two members presented original papers. However, only the longest serving members, the Pensionaries, normally received a salary, and support for research expenses was also limited; academicians needed some other income, and earning this often reduced their research output. What membership of the Académie did bring was prestige and access to influence and to a close-knit community of scientists, and it was towards membership that Coulomb now began to progress, following Bussot in winning a prize competition [3].

Prize competitions were a characteristic of learned academies' work for the advancement of science, and the prizes awarded by the Académie Royale were sizable, equivalent to around a year's salary of one of their Pensionaries. Prize questions were often set to advance the development of particular fields, and a number set in the 1740s had strengthened Cartesian vortex views of electric and magnetic action. The 1777 prize was for an explanation of the diurnal variation of the earth's magnetic force, together with improved compass needles for detection. A similar competition two years previously had produced no winners, but now Coulomb, entering a field entirely new to him, shared the prize with an established expert, van Swinden, known for having devoted ten years of his life to measuring the magnetic variation every hour of every day [3]. In work that was to lead directly to his experiment on electrostatic attraction, Coulomb solved the usual problem of friction in compasses by suspending the needle from a thread. This had been tried previously, but there was no established theory about their action or reliability. Coulomb's paper was almost entirely instrumental in its focus. He relied for his knowledge of magnetism on Musschenbroek's lengthy dissertation of 1729, perhaps attracted by Musschenbroek's careful description of the phenomena and avoidance of hypotheses about causes [9], and concentrated on establishing the theory of his proposed suspension system. Starting from his knowledge of ropes, Coulomb realized that by using a fine silk thread the torsion, or twisting force, of the suspension could be made negligible compared with the magnetic force. Much of the paper is devoted to experiments demonstrating that, within an elastic limit, the oscillation of the needle had a period that was independent of amplitude; Coulomb inferred that the motion was simple harmonic, with a torsion force proportional to the angle of twist. He was able to show that, for the small angles involved in variation measurements, the thread would offer very little resistance to the forces of variation if the needle was aligned with the normal magnetic meridian to start with. Coulomb followed this work up, in 1784, with a substantial memoir on torsion and the elasticity of metal wires, hoping to find a conducting replacement for the silk thread in his instrument, which was running into problems of electrification (see below). He seems to have been the first person to state a correct law of torsion and to show experimentally that a given solid has a material elastic constant, independent of the specimen and the density of the solid [3, 7].

Meanwhile, Coulomb had at last achieved his ambition of being elected a member of the Académie Royale des Sciences. He had spent the years 1773–1781 in various postings around France: Bouchain, Cherbourg, and, in 1779, Rochefort, where he collaborated with the Marquis de Montalembert in constructing a fortress made entirely of wood. Coulomb seized the opportunity of using the shipyards at Rochefort as laboratories for his studies of friction, and in 1781 won a second Académie prize for his resulting *Théorie des machines simples*. He probably knew that he had come close to being elected to the Académie in 1779 and now made a forceful case to the Corps du Génie for a transfer to Paris. He was successful, being posted as maintenance engineer for the Bastille, and his election to the Académie followed. Over the next 25 years Coulomb was to produce 310 committee reports for the Académie, largely on engineering. The most notorious was that on canal and harbour improvements in Brittany in 1783: Coulomb became the scapegoat when the committee commented negatively on the engineering feasibility of the project, was charged with a minor dereliction of duty, and ended up in prison for a week! In 1784, he was appointed 'Intendant of the Royal Waters', with responsibility for a large part of Paris' water supply. At the same time he continued his position in the Corps du Génie and was thus nominally resident engineer at the Bastille when it was stormed on 14 July 1789. In between this bread and butter work, Coulomb managed to produce 25 scientific memoirs for the Académie, of which his seven on electricity and magnetism are the most important. His experiment on electrostatic attraction was the first.

Coulomb's suspended magnetic needle had been installed at the Paris Observatory, but it proved too sensitive to external disturbances such as doors opening or carriages passing outside, and Coulomb was called in to stabilize it. Electricity was one of the suspect causes, and he suggested replacing the silk thread with a conductor [3]. Having achieved this by showing, in his memoir of 1784, that piano wires were suitable, Coulomb suggested the inverse use of his law of torsion—constructing a balance that could be used to measure various types of small force, and trying it out on electrostatics in 1785[1].

The study of electricity had boomed in the 1740s following the invention of the Leyden jar. Various attempts had previously been made to measure and formulate the laws of electrostatic attraction and repulsion, the difficulty being in relating the macroscopic forces observed in real situations with a theory of how the underlying microscopic forces operated. With little evidence of whether charge was localized or extensive, electrification was generally thought to be due to either one or two fluids and the action between electrified bodies possibly due to electric atmospheres similar to those suggested by Nollet. The electrometer undoubtedly provided some sort of measure of the electrification of a body, but it was not clear what, nor how this related to other measures such as the number of turns of an electrical machine to charge it or the length of spark obtainable from it. Although many measurements had been made, their significance was uncertain. In 1756 Aepinus' invention of the air condenser had undermined the electric atmospheres explanation for they would have short circuited the condenser. Aepinus' solution was a newtonian and instrumental one: admit forces of attraction and repulsion and do not worry about trying to explain their mechanism [3]. His work had little impact while study of electricity slumped during the Seven Years War but revived as electricity again became a hot topic in the late 1770s following Alessandro Volta's invention of the electrophore and condensatore, which demonstrated the existence of atmospheric electricity. In 1782 Volta visited Paris in person to publicize his work. He addressed the Académie twice and worked with Laplace and Lavoisier on electricity from vapours [11]. Volta, who had recently abandoned attempts to discover the microscopic forces in favour of more promising (he thought) macroscopic quantification, relied on Aepinus' work in his formulation of the law relating the quantity or 'action' of electricity to capacity, $Q = CT$ (where Q is the 'action of electricity' of a body of capacity C and T is the 'intensity' or electric tension). Volta gave a clear account of his law in his lectures of 1782, 'Quantity is the sum of all the electricities contained in all the points making up the surface of the electrified body. *Intensity* is the force exercised by each one of these points ...' ([11] pp 128–9), and noted that electrical action was felt over distances far greater than the possible extension of any supposed electric atmospheres. It would be surprising if Coulomb, newly elected to the tight-knit community of the Académie, did not know of Volta's work, and he may also be presumed to have known something of Aepinus' approach, for apart from Volta's lectures, he had himself used Aepinus' method of magnetizing needles in 1777. Meanwhile many electricians had been encouraged by the success of Newton's law of gravitation (published in 1687) to look for, or even assume, similar laws for other attractive forces including electrostatics: Aepinus had guessed an inverse square law; Charles Stanhope, Lord Mahon, had given a flawed, but widely accepted, demonstration of it in 1779, which was translated into French and heavily promoted by Needham in 1781; Joseph Priestley had followed up Franklin's observation that an insulated ball inside a charged cup was not subject to electric force and inferred an inverse square law by analogy to gravity; and Henry Cavendish and John Robison had both given valid demonstrations, which, however, were not published until the 19th century [3]. An inverse square distance dependence seems to have been widely accepted, at least among newtonians, even before Coulomb's work.

None of this background was apparent in June 1785 when Coulomb, a newcomer to electrical science, described his torsion balance in a brief and entirely instrumental paper that is remarkable for its total lack of any external reference except to his own law of torsion. This paper is in three sections, the first describing the construction of the balance, the second describing its use in measuring the distance dependence of the repulsive force between two similarly charged spheres, and the third being concluding remarks outlining precautions taken and potential uses of the balance [12].

Coulomb's apparatus is shown in figure 2. ABCD was a glass cylinder (32 cm diameter × 32 cm high) covered by a glass plate with two holes in it, each 4.5 cm in diameter One of these, in the centre of the plate, had a glass tube 65 cm high rising above it, at the top of which was a torsion micrometer (b). Suspended from the micrometer was a silver wire 76 cm long and of diameter 0.04 mm, at the bottom of which an insulating needle made of a silk thread or a straw, soaked in Spanish wax and finished off by a cylindrical rod of shellac, was fixed. A pith ball 0.5 cm or 0.7 cm in diameter was attached to one end of the needle, and a counterweight in the form of a vertical piece of paper that also dampened oscillations was fastened to the other. The second hole in the cover plate was used for inserting a second pith ball, the same diameter as the first, fastened to the lower end of a small cylinder made of shellac. This occupied the same position as the movable ball when the wire was untwisted. A strip of paper, divided into 360°, ran around the glass cylinder at the height of the needle.

First, the fixed ball was charged by contact with a charged conductor, which was then removed. Since the two balls started in contact, the movable ball became similarly charged and the balls repelled each other. As soon as the oscillations stopped the position of the movable ball was read, the angle of deflection being an approximately direct measure of the distance between their centres (at the small angles Coulomb worked with). Then the torsion of the thread was increased, forcing the two balls back towards each other, and again the position of the movable ball was read. The calculations depended on balancing the torsion force against electrostatic repulsion.

Coulomb gave the following results:

> *First trial. Having electrified the two balls by means of the pin head while the index of the micrometer points to 0, the ball* a *of the needle is separated from the ball* t *by 36°.*

---

[1] Henry Cavendish, who used a torsion balance himself in his measurement of gravitational attraction, later claimed that the idea of the torsion balance was due to John Michell, who had outlined it to him [10]. However, Michell did not publish, and we have no evidence that his idea was common currency or known to Coulomb.

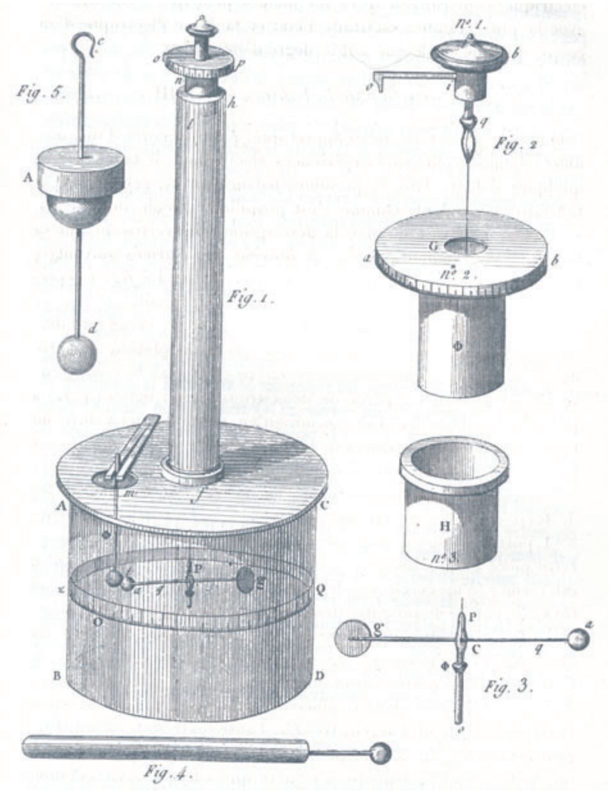

Figure 2. Coulomb's torsion balance for measuring the distance dependence of electrostatic repulsion [12].

> *Second trial. By twisting the suspension wire through 126° as shown by the pointer of the micrometer, the two balls approached each other and stand 18° apart.*
>
> *Third trial. By twisting the suspension wire through 567° the two balls approached to a distance of 8.5°* ([13] p 412).

Having established that the force required to twist the wire through 360° was 1/340 grains (1.53×10^{-6} N) [14][2], Coulomb calculated that the initial repulsive force, which had deflected the ball to 36° was $1/3400$ grains; that to halve the distance between the balls to 18°, four times the initial force was needed (18° plus twisting the wire through 126°, gives a total of $144° = 4 \times 36°$); and that to halve approximately the distance again to 8.5° required four times the previous force (i.e. 9° + twisting the wire through 567° makes $576° = 4 \times 144°$). Coulomb concluded,

> *it results then from these three trials that the repulsive action which the two balls exert on each other when they are electrified similarly is in the inverse ratio of the square of the distances* ([13] p 413).

These three trials were the only data Coulomb ever gave in proof of the inverse square law of repulsion. The experiment required great manual dexterity and the apparatus was notoriously unstable, yet the agreement with theory is remarkably good. Heering [16] has demonstrated, both by looking at the internal evidence and by reconstructing the apparatus, that even these trials may not represent real measurements. Coulomb discussed a number of sources of error, such as taking the angle of deflection as directly proportional to the distance between the centre of the balls, and the redistribution of charge around the balls, which resulted in diminishing the distance between them, and calculated their effects as insignificant. He further noted that in repetitions of the experiment unexplained oscillations through 2° or 3° of the balance made it difficult, if not impossible, to define the zero position accurately and suggested starting measurements at 30° to 40° of torsion to reduce the significance of the oscillation, rather than at zero [12]. Heering notes that this throws doubt on his claimed first trial, which starts at zero. Similar oscillations in the reconstructed apparatus made it impossible to say more than that the exponent lay between 1 and 3, and were traced to electrification of the experimenter. Coulomb does not mention taking precautions against this: the implication is that he did not, which seems surprising if Heilbron [3] is correct in asserting that his initial interest in electricity and motivation in researching the torsion of metal wires was to guard against electric disturbance of his magnetic instrument.

Further doubt is cast on the third trial of 567° by Coulomb's remark that he found his initial silver wire too fine and liable to break and subsequently used wires of twice the diameter, pointing out, however, that the twist of the thicker wires had to be kept less than 300°, otherwise the elastic limit was exceeded [16].

There are a number of indications that Coulomb produced his paper in a hurry. He opens by acknowledging that he has few runs of the experiment and that improvements need to be made to the instrument,

> *Although I have learned by experience that to carry out several electric experiments in a convenient way I should correct some defects in the first balance of this sort which I have made; nevertheless as it is so far the only one that I have used I shall give its description...* ([13] p 409).

The brevity of the paper and the lack of data contrast strongly with his previous papers on mechanics, magnetism, and torsion. Why the hurry? Perhaps Coulomb was anxious to establish priority, but to what? A hint comes in his first mention of the torsion balance the previous year in his published torsion paper of 1784,

> *Since the reading of this memoir, I have constructed, following the theory of torsion that I am about to explain, an electric balance and a magnetic balance; but, as these two instruments, as well as the results relating to the electric and magnetic laws which they give, will be described in later volumes of our Memoirs, I believe that it is enough just to announce them here*[3] ([12] p 90).

---

[2] Using a conversion factor 1 livre = 9216 grains (force) = 480 200 dyn [14]. This differs from the conversion factor usually given for grains to newtons of 1 grain (force) $\approx 6.35 \times 10^{-5}$ N as the French grain was smaller than the British (Imperial) grain [15].

[3] Depuis la Lecture de ce Mémoire, j'ai construit, d'après la théorie de la réaction de torsion que je viens d'expliquer, une balance électrique et une balance magnétique; mais, comme ces deux instruments, ainsi que les résultats relatifs aux lois électriques et magnétiques qu'ils on donnés, seront décrits dans les volumes suivants de nos Mémoires, je crois qu'il suffit ici de les annoncer.

Mentioned here specifically are the balances, and their use to demonstrate the laws, but not the form of the laws. In the 1785 paper Coulomb emphasized the sensitivity of his balance both in his opening remarks and in a lengthy remark at the end, where he described its potential use as an electrometer. If Coulomb were trying to establish his priority in inventing the torsion balance, and its validity as a measuring instrument, this might explain the form of his paper; the cursory experimental data and the total lack of any theoretical discussion of the force law, which seems extraordinary if he was trying to prove the inverse square law. Perhaps, instead, he was aiming to validate and promote his instrument, which itself relied on a novel theory, by stressing its demonstration of an already widely believed law.

Whatever Coulomb's motivations in his first memoir, he was, himself, largely responsible for subsequent interpretations as a proof of the inverse square law. The second memoir (1787), on the attraction between dissimilarly charged bodies, shows Coulomb's newtonian programme. It is the first to state explicitly that the force was proportional to the product of the 'electric masses' of the two bodies, a proposition that Coulomb frequently asserted but never attempted to demonstrate ([12] p 118). Nor did he ever define a unit electric charge, relying on relative electric 'masses' or 'densities'. Throughout, his treatment depends on the unstated assumption that electric mass is localized and analogous to gravitational mass, an assumption that may have been suggested by Volta's definitions of quantity and intensity (see above), although it is far from being an automatic consequence of them. However, conceptions of charge were implicit in the type of force–distance relations physicists expected, and discussions of Coulomb's work focused mainly on the inverse square distance relation and its implication of action at a distance.

An alternative and less exact arrangement, shown in figure 3, was needed for measuring the attraction between dissimilarly charged bodies since two attracting balls had a habit of rushing together and touching in the original configuration. A shellac needle 3.4 cm long was suspended by a silk thread from a wooden rack that enabled its position to be changed both horizontally and vertically. A gilt disc, 1.6 cm diameter, was fixed perpendicularly to one end of the needle and an insulated copper globe of diameter 32.5 cm was placed at a measurable distance from the disc. The globe was charged by a spark from a Leyden jar while the disc was oppositely charged by contact with a grounded conductor. Then the needle was set oscillating with an amplitude of less than 30° and the time for a given number of oscillations was measured. The distance between the ball and disc was increased and the experiment repeated. Coulomb hypothesized that, assuming the disc acted as though its electric mass were concentrated at the centre, the torsional force of the silk thread was negligible and the restoring force (ϕ) of the displaced ball would be inversely proportional to the square of the distance (d) between the centres (i.e. $\phi \propto 1/d^2$). The time for a given number of oscillations (T) of simple harmonic oscillation being inversely proportional to the square root of the restoring force gives $T \propto 1/\sqrt{\phi}$, and so T should be proportional to d.

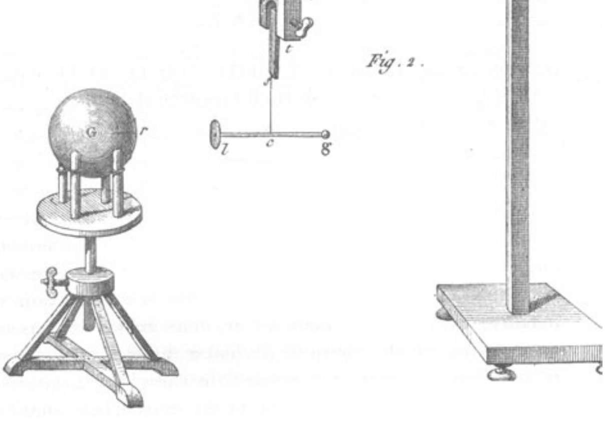

Figure 3. Coulomb's oscillatory apparatus for measuring the distance dependence of electrostatic attraction [12].

Once again, Coulomb gave only three results before concluding that

> *We have thus come, by a method absolutely different from the first, to a similar result; we may therefore conclude that the mutual attraction of the electric fluid which is called positive on the electric fluid which is ordinarily called negative is in the inverse ratio of the square of the distances* ([13] p 417),

thus incidentally revealing himself as a believer in two, rather than one, electric fluids.

In practice, the distances he had recorded were in the ratio 3 : 6 : 8 and the time for 15 oscillations in an approximate ratio of 3 : 6 : 9. Coulomb ascribed half the large deviation from hypothesis of the last reading to leakage of electricity over the 4 min of the experiment and followed this paper up with a third, much more extensively measured, investigation of loss of electricity to the surrounding air, stating that in his first two papers 'we have seen . . . [that the action between two electrified bodies]. . . is composed of the electric densities and the inverse square of the distances of the two globes'[4] ([12] p 155). By his fourth paper, of 1786, in which he confirmed experimentally that electrification was confined to the surface of a conductor, he felt sufficiently confident to open by stating that he had 'proved' the inverse square law ([12] p 173).

Coulomb's work made sense only to those, such as the Academicians, who understood and accepted the newtonian analogy on which it was based and believed in a universe governed by a limited number of simple laws [17]. Coulomb made no attempt, for example, to explain the theory that allowed him to consider the charge on the balls as though it was concentrated at their centres, the basis of the spherical, astronomical looking configuration of his experiment. For believers in action by electrical atmospheres, it would have made sense to vary the gas in the experiment and to measure between plane surfaces. His work was widely accepted in France but contested in Germany, particularly by Paul Louis Simon, and in Italy by Volta. Volta was characteristic of a number of electricians who believed in quantification but not in universal laws. He did not doubt Coulomb's

---

[4] Nous avons vu, . . . leur action réciproque était en raison composée des densités électriques et de l'inverse du carré des distances de ces deux globes.

measurements but pointed out that they applied only to a very limited experimental set-up [18], and he viewed Coulomb's assumption of a microscopic law of attraction as a retrograde step. Moreover, Coulomb's main demonstration was based on measurements of repulsion, which Volta believed to be a secondary effect, and did not necessarily apply to the primary attraction between electric fluid and ponderable matter [11]. Finally, Volta regarded Coulomb's balance as unnecessarily complicated and unreliable, the charge laid against it by Simon also, who wanted to use it for lecture demonstrations. He devised an alternative apparatus and attempted to repeat Coulomb's experiments, finding unexpectedly that his results suggested a $1/r$ law. His work was extensively taken up in Germany, for a $1/r$ law provided an analogy to Boyle–Mariotte's gas law, justifying a belief in action by electric atmospheres, just as a $1/r^2$ law was opposed for its analogy to gravitational action at a distance. However, Simon had taken his measurements between the surfaces of his balls. Development of his work came to an end in 1825 when P N C Egen, a believer in action at a distance, showed that if one measured to the centre of the balls the results agreed with Coulomb's. As Heering ([16] p 993) noted, 'A scientist's conception of the substance of the electric matter also influenced the relations that seemed plausible to him'.

In France, the climate of electrical science had changed since Nollet's researches in the 1740s. A new generation of Academicians were pioneering a new way of doing physics, discovering the laws of nature through a new type of experiment–theory relation, one that matched closely controlled measurements of isolated variables against mathematical theory of idealized situations. The enterprise was validated by the precision of agreement between experiment and theory, rather than by the presence of witnesses or by utility as in previous eras [19]. Coulomb's experiment did not 'prove' the inverse square law, but it rendered the analogy between electrostatics and gravitation visible and demonstrable by measurement and by subsequent definition of electric charge [17]. It appealed to the mathematicians in the Académie, especially Laplace, for it enabled him to extend his recent reformulation of gravitational theory to electricity also. Coulomb's instrumental approach, which made no attempt to explain the mechanisms of the forces, was accepted and was reinforced by his colleague Haüy's *Exposition* of Aepinus' work in 1787 and by his own further investigations of the distribution of charge on conductors of various shapes. Pancaldi [11] suggests that the consensus among the Academicians was sufficient to marginalize Volta's alternative research programme, which led to the invention of the voltaic cell, until 1801, when an uneasy acknowledgment of it was forced upon them, partly by Napoleon Bonaparte, who perceived Volta's symbolic usefulness in promoting Paris as the scientific 'capital' of Europe.

By this time Coulomb had not only weathered the French Revolution but also set up home with a girl 30 years his junior. His first son was born in 1790 and a second in 1797, and he finally married their mother, Louise Françoise Le Proust Desormeaux in 1802. He had retired from the Corps du Génie when it was reorganized along lines he disapproved of, in 1791, with the rank of lieutenant colonel, holder of the Croix de St Louis, with 31 years service and a pension of 2240 livres a year. His reports for the Académie Royale continued unabated until it was disbanded in 1793, and, along with his friend Lavoisier, he was purged from the committee on weights and measures the same year. He retired with his friend Borda to a house he had bought from Lavoisier near Blois, returning to Paris in 1795 with the refounding of the Académie as the Institut de France. Amongst his later work for the state, he was Inspector General of Public Instruction, playing a significant role in the setting up of the lycées (high school) system across France [1].

Coulomb died on 23 August 1806, leaving his scientific papers in the hands of Jean-Baptiste Biot, Laplace's protégé, who further developed the mathematics of electrostatics [14]. His work was beginning to be acknowledged outside France, being brought to wide attention for the first time by Robison's *Encyclopaedia Britannica* article on electrostatics in 1801, which was popularized in Britain by Thomas Young in 1807. Two things finally set the seal on acceptance of Coulomb's work. The first was Poisson's 1811 theory of electric potential, which united his inverse square law and charge distribution measurements with Laplace's mathematical methods and potential function [3]. Writing in 1845, William Thomson made this endorsement explicit and exemplifies the interpretation of Coulomb's experiment, which had, by then, become fixed:

> *In the papers of Poisson on electricity we find the analytical solution of the problems that are combined with the most important parts of Coulomb's experimental researches; the correspondence of the results is very satisfactory, and the strength and beauty of the analysis are placing the theory of electricity next to the theory of gravitation, through mathematical correspondence at the first place of natural science* ([16] p 991).

The second advance was the development of his instrument. The 19th century saw a proliferation of galvanometers and magnetometers based on the torsion principle. In particular, Dörries *et al* [20] has shown how Gauss and Weber's international campaign in the 1830s to measure terrestrial magnetism all over the world embedded standard torsional instruments into physical practice.

Coulomb's experiment seems to have been important more for the power of the newtonian analogy he drew than for the quality of its proof by measurement. His apparatus became an icon; copies were regularly exhibited at French lecture demonstrations in the 19th century, although they were seldom used to generate real results [21]. Coulomb may have confused the sensitivity of his apparatus with precision, but he gave a method that was, in principle, valid if one accepted the newtonian analogy, and subsequently used his instruments to show how electricity could be brought within the framework of French mathematical physics in the late 18th century. His long term reputation was ensured by the dominance of this tradition over physics in the 19th century.

Acknowledgment

I am grateful to the Faculty of Arts, Open University, for research support.

References

[1] Gillmor S 1971 *Coulomb and the Development of Physics and Engineering in Eighteenth Century France* (Princeton, NJ: Princeton University Press)

[2] Gurlac H 1973 *Dictionary of Scientific Biography* vol VII, pp 66–91

[3] Heilbron J L 1979 *Electricity in the 17th and 18th Centuries* (Berkeley: University of California Press)

[4] Hankins T L 1973 *Dictionary of Scientific Biography* vol VIII, pp 178–80

[5] Heilbron J L 1990 *The Quantifying Spirit in the Eighteenth Century* ed T Frângsmyr *et al* (Berkeley: University of California Press) pp 1–23

[6] Nollet J A 1750 *Essai sur l'Électricité des corps* (Paris)

[7] Truesdell C 1973–4 *Eighteenth-Century Studies* **7** 213–25

[8] Briggs R 1991 *Past and Present* **131** 38–88

[9] Musschenbroek P van 1729 *Physicae experimentales, et geometricae, de magnete, tuborum cappillarium vitreorumque speculorum attractione, magnitudine terrae, coherentia corporum firmorum dissertationes* (Leyden)

[10] Cavendish H 1798 *Phil. Trans. R. Soc.* **88** 49

[11] Pancaldi G 2003 *Volta: Science and Culture in the Age of Enlightenment* (Princeton, NJ: Princeton University Press)

[12] Coulomb C A 1884 *Collection de Mémoires Relatifs à la Physique* vol I (Paris: Société Française de Physique)

[13] Magie W F 1935 *A Source Book in Physics* (New York: McGraw-Hill)

[14] Potier A 1884 *Collection de Mémoires Relatifs à la Physique* ed C A Coulomb (Paris: Société Française de Physique) pp vii–xiii

[15] Jerrard H 1972 *Dictionary of Scientific Units* 3rd edn (London: Chapman and Hall)

[16] Heering P 1992 *Am. J. Phys.* **60** 988–94

[17] Heilbron J L 1994 *Restaging Coulomb: Usages, Controversies et Réplications Autour de la Balance de Torsion* ed C Blondel and M Dörries (Firenze: Istituto e Museo Storia della Scienza) pp 151–62

[18] Fregonese L 1994 *Restaging Coulomb: Usages, Controversies et Réplications Autour de la Balance de Torsion* ed C Blondel and M Dörries (Firenze: Istituto e Museo Storia della Scienza) pp 85–98

[19] Licoppe C 1994 *Restaging Coulomb: Usages, Controversies et Rèplications Autour de la Balance de Torsion* ed C Blondel and M Dörries (Firenze: Istituto e Museo Storia della Scienza) pp 67–84

[20] Dörries M 1994 *Restaging Coulomb: Usages, Controversies et Rèplications Autour de la Balance de Torsion* ed C Blondel and M Dörries (Firenze: Istituto e Museo Storia della Scienza) pp 121–50

[21] Blondel C, Dörries M and Pestre D 1994 *Restaging Coulomb: Usages, Controversies et Réplications Autour de la Balance de Torsion* ed C Blondel and M Dörries (Firenze: Istituto e Museo Storia della Scienza) pp 11–16