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Replication of Coulomb's Torsion Balance Experiment

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1. Introduction

A long-standing issue in history of science is whether or not the fundamental law of electrostatics was justified experimentally in the late 1700s. By that time, following Isaac Newton's mechanics, physicists had come to understand that the gravitational force between two masses m_1 and m_2 may be expressed by

$$F_g \propto \frac{m_1 m_2}{r^2},$$

where r is the distance between the centers of the two bodies. The success of this inverse-square law led some individuals to investigate whether phenomena other than celestial motions could also be described by inverse-square equations. In particular, some scientists, Joseph Priestly and Henry Cavendish among them, argued that the repulsion and attraction between bodies charged by electricity are likewise described by an expression such as

$$F_e \propto \frac{q_1 q_2}{d^2},$$

where each q represents the particular quantity of electric charge on each body, and d is the distance between their centers. The remarkable formal coincidence between such force laws prompts the question: had scientists found substantive evidence to warrant that law of electrostatics or did they mainly presuppose that it matched Newton's law?

In June of 1785, Charles Augustin Coulomb, a retired military engineer, announced to the Paris Academy of Sciences that he had devised an innovative experimental apparatus, the torsion balance, an extremely sensitive instrument able to measure even minute forces to an unprecedented degree of accuracy. With it Coulomb claimed to have demonstrated that electrostatic repulsion indeed varies inversely at the square of the distance. That experiment, together with another presented in 1787, eventually led physicists to designate the fundamental equation of electrostatics as "Coulomb's law." Coulomb was admired increasingly as having helped to transform French physics from a descriptive field (plagued by dubitable speculative hypotheses) to a conceptually lean, experimentally grounded, and highly mathematized science.[1] His rigorous engineering mindset

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[1] See, e.g., Biot (1843), 60.

combined with mathematical analyses typical of rational mechanics served to raise a previously qualitative branch of physics, electricity, to the status of an exact science.

In turn, his torsion balance became one of the best known devices of experimental physics, and various versions of it were produced and sold by instrument makers in Europe and the United States.[2] Diagrams of the torsion balance, along with Coulomb's results, became standard elements in physics textbooks and courses.[3] It was said that Coulomb's instrument "exceeds all others in delicacy and the power of measuring small forces," and his investigations with it were recommended to students "as examples of the most refined, ingenious, and conclusive experiments" in natural philosophy.[4] Yet this delicate device proved to be notoriously difficult to operate, even today, and thus it became a scientific object that seems to have served more often the purpose of inanimate pedagogical illustration, rather than use or demonstration. Even some prominent physicists failed to operate the apparatus with the efficacy reported by Coulomb, as we will mention below.

In recent years, historians have argued accordingly that Coulomb's elegant report might not have been a literal description of his actual experiments. In particular, Peter Heering has argued that "Coulomb did not get the data he published in his memoir by measurement."[5] Since, it seems, no successful attempts to reproduce Coulomb's results have ever been reported, the early establishment of the fundamental law of electrostatic repulsion has become an exemplary case in the history of science: one in which it would seem that a leading scientist interwove experimental facts with idealizations using rhetorical devices proper to his idiosyncratic community. If this is correct, then perhaps the members of the prestigious Paris Academy accepted Coulomb's extraordinary claims partly because of the shared expectation that nature obeys mathematical laws such as those identified by Newton.

The present paper analyzes Coulomb's original work on the law of repulsion in light of a new series of replications of his experiment. We will argue, contrary to recent claims, that Coulomb's report of 1785 constitutes an accurate description of the material components, procedures, and results of his experimental researches.

2. Coulomb's Memoir of June 1785

As an engineer, Coulomb labored to solve problems of fortifications, flexure of beams, masonry rupture, earth pressure, the stability of arches, and more. In the 1760s, he carried out such works mainly while stationed at Martinique in the West Indies, to fortify the island against possible attacks from the English. After years of strenuous labors, and serious illnesses, he returned to France in 1771. He resumed work as a provincial military engineer, and increasingly spent time writing memoirs on physics, winning two prize contests held by the Academy of Sciences: one on the design of magnetic

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[2] Bertucci (1997), Schiffer (2003), 60.

[3] The following textbooks, typical examples, discuss and illustrate Coulomb's experiment and cite only his original data: Olmsted (1844), 395–398; and Privat-Deschanel (1873), 519–522.

[4] Olmsted (1844), 395, 401.

[5] Heering (1992a), 991.

needles, and the other analyzing friction. In late 1781, Coulomb was elected to resident membership in the Academy, and hence continued to report on his various investigations.

Coulomb's researches on electrostatics stemmed from his studies on the torsion of wires, a field that had scarcely been incorporated into the experiments of electricians. By 1777, Coulomb had developed a theory of the torsion of thin silk and hair strands for use in suspending magnetic needles, based on extensive experimental work on magnetic compass designs. Subsequently, he analyzed the torsional behaviors of thin wires. By 1784, Coulomb found that the force exerted by any twisted wire against its torsion (that is, the reaction torque, or what he called "the momentum of the force of torsion") is describable by

$$F_\tau = w\frac{\alpha D^4}{l},$$

where l is the length of the wire, D is its diameter, w is a constant characteristic of the particular metal, and α is the angle of torsion.[6] Since the torque is proportional to the angle of torsion, Coulomb realized that he could use wires under torsion to counteract and hence measure any particular force (be it mechanical, magnetic, etc.) acting sensibly to twist the wire. Since wire could be manufactured to have a very small diameter (and considerable length), his law of torsion suggested that one could use very thin wires to measure extremely weak forces. Coulomb therefore designed a procedure to measure forces of electrostatic repulsion by exhibiting how much an electrically charged object repels another at the end of a wire-suspended lever.

Figure 1 illustrates Coulomb's first electrostatic torsion balance and its parts. This diagram was originally included in Coulomb's memoir (finally published in 1788), and has been characterized as the one diagram of an experimental device which has perhaps been "reproduced more often that any other."[7] Coulomb accompanied it with detailed descriptions that are worth quoting at length:

> On a glass cylinder *ABCD*, 12 pouces in diameter and 12 pouces in height [$\approx$ 32.5 cm][8], one places a glass plate of 13 pouces in diameter, which completely covers the glass vessel; this plate is pierced by two holes of nearly 20 lines [$\approx$ 4.5 cm] in diameter, one at the center, at *f*, above which rises a glass tube of 24 pouces in height [$\approx$ 65 cm]; this tube is cemented on the hole *f*, with the cement used in electrical devices: at the upper end of the tube at *h*, is placed a torsion micrometer which one sees in detail in *Fig. 2*. The upper part, *no. 1*, bears the knob *b*, the pointer *io*, and the clasp of suspension, *q*; that piece goes into the hole *G* of piece *no. 2*; that piece, *no. 2*, consists of a disk *ab* divided along its perimeter into 360 degrees, and of a copper tube Φ that goes into the tube *H*, *no. 3*, attached to the interior of the upper end of the tube or of the glass shaft *fh* of the 1^{st} *figure*. The clasp *q*, *Fig. 2*, *no. 1*, has approximately the shape of the tip of a solid pencil-holder, which can be tightened by means of the ringlet *q*; into the clasp of this pencil-holder is inserted the tip of a very thin filament of silver; the other end of the silver filament is gripped *(Fig. 3)* in *P*, by the clasp of a cylinder *Po* made of copper or iron, of which the diameter is but a line [$\approx$ 2.3 mm], and of which the extremity *P* is split, and constitutes a clasp that is tightened

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[6] Coulomb (1784), 247–248.

[7] Devons (1984).

[8] Units conversions: 1 pouce (Paris) $\approx$ 2.7069 cm; 1 line (Paris) $\approx$ 2.2558 mm.

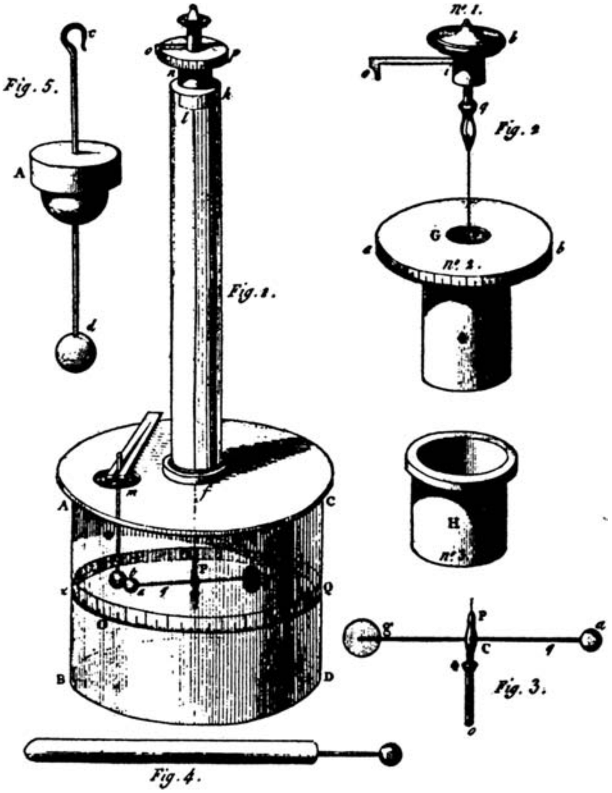

Fig. 1. Diagram of Coulomb's torsion balance of 1785. (Note: the parts were not drawn in their reported relative proportions, e.g., the pith balls should be smaller.)

> by means of the collar Φ. The small cylinder is flattened and pierced at *C*, to there insert *(Fig. 1)* the needle *ag*: it is necessary that the weight of the small cylinder be enough to put the silver filament in tension without breaking it. The needle that one sees *(Fig. 1)* at *ag*, suspended horizontally at about half the height of the big vessel that encloses it, consists either of a filament of silk covered in Spanish wax, or of a reed likewise covered in Spanish wax, and finished from *q* to *a*, 18 lines in length [$\approx$ 4 cm], by a cylindrical filament of gum-lac[9]: at the extremity *a* of that needle, there is a small ball of pith[10] of two to three lines in diameter [4.5 to 6.8 mm]; at *g* there is a small vertical plane of paper

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[9] Shellac, or *gomme-laque*, was manufactured from the sticky resin secreted by various species of lac insects, mainly in soapberry and acacia trees in India; it is a natural thermoplastic polymer similar to synthetic plastic. From India, Venetian merchants imported solid adhesive lac to Spain and France, where its compound became known as Spanish wax, *cire d'Espagne*, although it contains no wax; it was made from gum-lac plus additional pigments and resins to make it less brittle, and it was used mainly to seal private letters. Coulomb's prescriptions suggest that the gum-lac he used was a better insulator than Spanish wax.

[10] Pith is the lightweight spongy tissue inside the stems of vascular plants; it was commonly obtained from elderberry shrubs of the Sambucus genus, or from *chèvrefeuilles*.

> dipped in turpentine, which serves as a counterweight to the ball *a*, and which dampens the oscillations.
>
> We said that the lid *AC* was pierced by a second hole at *m*; it is into that second hole that one introduces a small stem $m\Phi t$, of which the lower part Φt is of gum-lac; at *t* is a ball likewise of pith; around the vessel, at the height of the needle, one traces a circle *zQ* divided into 360 degrees: for greater simplicity I used a strip of paper divided into 360 degrees, which I glued around the vessel at the height of the needle.[11]

The hanging needle (the waxed filament or reed) facilitated the measurement of minute forces without disturbance from effects such as friction. Once charged the two pith balls immediately repelled one another. Coulomb explained that the repulsion could be measured by turning the micrometer to force the balls closer together. He reported, in print, only three measurements:

> *First Trial.* Having electrified the two balls with the pinhead, the index of the micrometer pointing to 0, the ball *a* on the needle is displaced from the ball *t* by 36 degrees.
>
> *Second Trial.* Having twisted the suspended filament, by means of the knob *o* of the micrometer to 126 degrees, the two balls approached one another & stopped at 18 degrees of distance the one from the other.
>
> *Third Trial.* Having twisted the suspended filament by 567 degrees, the two balls approached one another to 8 degrees and a half.[12]

In a few sentences Coulomb then stated that these numbers show that when the distance between the electrified balls is halved, their force of repulsion is quadrupled–and that such relations reveal an inverse-square law. His argument may be simply put algebraically. Assuming that the spheres do not lose charge during a pair of measurements (and introducing explicitly a constant *k* of electric force), the successive forces between them may be described as

$$F_\alpha = k\frac{q_1 q_2}{(d_\alpha)^2}, \quad \text{and} \quad F_\beta = k\frac{q_1 q_2}{(d_\beta)^2}.$$

If the second separation between the two bodies is $d_\beta = \frac{1}{2}d_\alpha$, we then have $4F_\alpha = F_\beta$. Thus it is a property of inverse-square forces that when the distance between the centers of the two bodies is reduced by half the force between the bodies becomes four times as great. Accordingly, Coulomb could twist the micrometer to angles suited to test for this specific increase in force.

Since the force of torsion balances the force of repulsion, and since we presume that the charges on the balls do not change, their initial angular separation after charging may be taken numerically to represent the repulsion proper. (This equivalence is only approximate because it relies on the angular measure of separation rather than the actual linear separation between the centers of the pith balls.) To decrease the separation by half, we would then have to twist the wire four times as much as that initial torsion. In Coulomb's example, the initial separation of 36 degrees suggests that we should have to

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[11] Coulomb (1785), 570–571; author's translation.

[12] Coulomb (1785), 572–573.

twist the wire $36^\circ \times 4 = 144^\circ$, which should bring the separation down to $36^\circ \div 2 = 18^\circ$. On the apparatus, the wire's total torsion is:

$$\textit{total torsion} = \textit{angular separation} + \textit{micrometer torsion}.$$

(Again, this total torsion may be taken as approximately equal to the force of repulsion.) For the numbers in question, 144 = 18 + *micrometer torsion*, so, to decrease the separation from 36 to 18 degrees, the micrometer should point to 126 degrees. But how did Coulomb obtain these numbers? Did he slowly turn the micrometer until he obtained half the separation down below? (In this case the separation is the controlled variable, and the consequent micrometer reading is the outcome.) Or, did he twist it to 126 and then observe that the ball came to rest at 18 degrees? (In this case the separation is instead the outcome.) Coulomb's procedure becomes apparent in his "Third Trial," for there the separation is just 8.5, not quite half of 18 degrees, whereas the torsion on the micrometer is 567 which is precisely 4 × 144 – 18/2. In practice, to perform measurements in the way that Coulomb seems to have carried them out, one may multiply the initial observed separation α, whatever it be, by 3.5 ($4\alpha - \alpha/2 = 3.5\alpha$) which gives the second position to which to turn the micrometer (e.g., 3.5 × 36 = 126). The resulting separation should then be close to $\alpha/2$. Next, multiply the initial separation by 15.75 $\left(4 \cdot 4\alpha - \frac{1}{2}\alpha/2 = 15.75\alpha\right)$, which gives the third position to which to turn the micrometer, and the third separation should become approximately $\alpha/4$.

The numbers reported by Coulomb match remarkably well the expectation that at half the distance, four times the force. He wrote:

> We find in our first experiment, where the pointer of the micrometer is at the point *o*, that the balls are separated by 36 degrees, which produces at the same time a force of torsion of 36d = 1/3400 of a grain [≈ .0156 mg]; in the second trial, the distance of the balls is 18 degrees, but since one has turned the micrometer 126 degrees, it results that at a distance of 18 degrees, the repulsive force is 144 degrees: thus at half of the first distance, the repulsion of the balls is quadrupled.
>
> In the third trial, one has twisted the filament of suspension 567 degrees, and the two balls find themselves no farther apart than 8 degrees and a half. The total torsion being consequently 576 degrees, quadruple that of the second trial, and it only requires one half a degree more for the distance of the two balls in that third trial to reduce to half of that which it was in the second. It results thus from these three trials, that the repulsive action that the two balls electrified with the same kind of electricity exert one upon the other is the inverse ratio of the square of the distances.[13]

The simple calculations, based on the inverse-square relation, constitute only an approximation of the physical relations. In particular, the separation between the centers of the two spheres is not given by an arc but by the (shorter) straight chord between them. Taking this and other relations into account, we may describe experimental results more exactly as follows. Coulomb did not report the actual value of the exponent n (in the presumed $1/d^n$ law) which follows from his results, but we will calculate it.

As we noted above, the total torsion of the wire is equal to the angle of twist α_m indicated on the micrometer plus the angle of separation α between the centers of the

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[13] Coulomb (1785), 573–574.

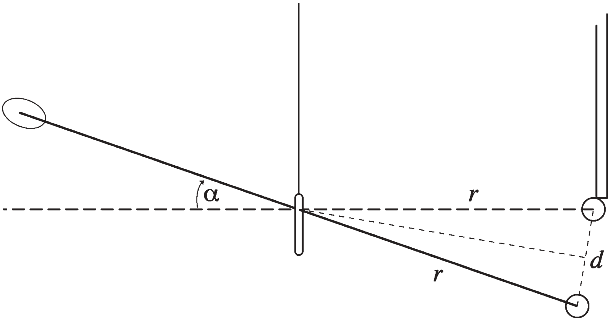

Fig. 2. Torsion balance under electrostatic repulsion

pith balls, as read on the lower scale. Accordingly, the torque exerted by the wire as it tends back to its initial position is

$$T_\alpha = \theta(\alpha_m + \alpha)$$

$$\theta \equiv w\frac{D^4}{l}.$$

For a first measurement, when the balls are separated by repulsion, and the micrometer still points to 0, the total torsion of the wire is $(0 + \alpha)$. Some writers however have claimed that at this point the total torsion depends also on the radii of the spheres.[14] They reason that, if there is no torsion on the wire at the very beginning (when the movable ball is at its initial position, adjacent to the stationary ball before repulsion), then the total separation after electrification includes this initial separation between their centers. This however incorrectly represents Coulomb's procedure, because he instead knowingly placed the stationary ball right at the initial position of the movable ball.[15] Before electrification there is hence a slight torsion on the wire although the micrometer points to 0. After electrification the entire distance of separation is then due *only* to the repulsion.

Now, considering the electrostatic repulsion between two equally charged spheres, we might assume that the pith balls have the same kind and quantity of charge, q (the calculations will not be affected by this simplifying assumption), we expect the *repulsion* R to be

$$R = k\frac{q^2}{d^2}.$$

Figure 2 illustrates the state of the system once the two pith balls have first separated by electrostatic repulsion.

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[14] E.g. Heilbron (1979), 472.

[15] Coulomb (1785), 571–572.

Since the distance between their centers is given by $d = 2r\sin\left(\frac{\alpha}{2}\right)$ the force of repulsion is

$$R = \frac{kq^2}{4r^2\left(\sin\frac{\alpha}{2}\right)^2}.$$

When the balls are in equilibrium this force must exert a torque that balances the reaction torque due to the wire's twist. Since the repulsion is not perpendicular to the waxed needle (moment-arm) of the movable ball, its component $R_\perp = R\cos\left(\frac{\alpha}{2}\right)$ must be taken. Substituting the value of R, and writing K for the constant $\frac{kq^2}{4r^2}$, we have:

$$R_\perp = \frac{K\cos\frac{\alpha}{2}}{\left(\sin\frac{\alpha}{2}\right)^2}.$$

This force of repulsion, multiplied by its moment arm r, will balance the opposite torque exerted by the wire, $R_\alpha = rR_\perp = T_\alpha$. Thus we have

$$\frac{rK\cos\frac{\alpha}{2}}{\left(\sin\frac{\alpha}{2}\right)^2} = w\frac{(\alpha_m + \alpha)D^4}{l}. \tag{1}$$

Now, by twisting the micrometer the movable ball can be forced to approach the stationary ball such that their separation will be reduced to an angle β. The total torsion on the wire will now be $\beta_m + \beta$, and a corresponding torque T_β can be formulated as before. The reaction torque now exerted by the wire and needle balances an electrostatic repulsion torque R_β:

$$R_\beta = \frac{rK\cos\frac{\beta}{2}}{\left(\sin\frac{\beta}{2}\right)^2}.$$

Here we retain the constant K used as above, under the assumption that the charge on the balls has remained the same over the interval of time between the two measurements in question. If so, the ratio of the initial to the subsequent electrostatic force is then:

$$\frac{R_\alpha}{R_\beta} = \frac{\left(\cos\frac{\alpha}{2}\right)\left(\sin\frac{\beta}{2}\right)^2}{\left(\cos\frac{\beta}{2}\right)\left(\sin\frac{\alpha}{2}\right)^2}.$$

At small angles each cosine may be set to unity, and each sine may be replaced by its argument, and thus the ratio of electric repulsions may be approximated simply by $\frac{R_\alpha}{R_\beta} = \frac{\beta^2}{\alpha^2}$. Either way, this ratio of repulsions must be the same as the ratio of the wire torques.

Let's next allow for the possibility that the elastic modulus θ might change from its value θ_α in the first position of the needle to a value θ_β in its second position (because extreme torsion can alter the resilience of a wire). Then we have:

$$\frac{R_\alpha}{R_\beta} = \frac{\left(\cos\frac{\alpha}{2}\right)\left(\sin\frac{\beta}{2}\right)^2}{\left(\cos\frac{\beta}{2}\right)\left(\sin\frac{\alpha}{2}\right)^2} = \frac{T_\alpha}{T_\beta} = \frac{\theta_\alpha(\alpha_m + \alpha)}{\theta_\beta(\beta_m + \beta)}.$$

This equation presupposes that the forces vary inversely at the second power of the distance, but the equation can instead be formulated without specifying the value n of the exponent in a force presumed to vary reciprocally at some power of the distance. Coulomb's paper does not include a value of the exponent, yet we may solve our ratio to obtain:

$$n = \frac{\ln\left[\frac{\theta_\alpha}{\theta_\beta}\frac{\left(\cos\frac{\beta}{2}\right)}{\left(\cos\frac{\alpha}{2}\right)}\cdot\frac{(\alpha_m+\alpha)}{(\beta_m+\beta)}\right]}{\ln\left[\frac{\left(\sin\frac{\beta}{2}\right)}{\left(\sin\frac{\alpha}{2}\right)}\right]}. \tag{2}$$

If we assume that the elastic modulus remains the same between the two positions (i.e., $\theta_\alpha = \theta_\beta$) and apply this equation with Coulomb's reported numbers (0, 36) and (126, 18), we obtain $n = 1.981$. And for (126, 18) and (567, 8.5) we find 1.842. Hence, Coulomb's average exponent is:

$$n = 1.911.$$

Or, by approximating for small angles, we have:

$$n = \frac{\ln\left[\frac{\alpha_m+\alpha}{\beta_m+\beta}\right]}{\ln\left[\frac{\beta}{\alpha}\right]},$$

and in this way the average exponent seems to be 1.923.

Now, consider equation (1) and simplify it by writing $h = \frac{wD^4}{rKl}$. Thus we have:

$$h = \frac{\cos\frac{\alpha}{2}}{(\alpha_m+\alpha)\left(\sin\frac{\alpha}{2}\right)^2}, \quad \text{and so,} \quad \alpha_m = \frac{\cos\frac{\alpha}{2}}{h\left(\sin\frac{\alpha}{2}\right)^2} - \alpha. \tag{3}$$

The latter equation serves to calculate the micrometer reading α_m corresponding theoretically to each observed angular separation α. Figure 3 plots an inverse-square curve from all such values. Likewise, the other curves are plotted by using exponents 1 and 3, instead of 2. For the three curves, the value of h is fixed by using only the *initial* data pair $(\alpha_m,\ \alpha) = (0,\ 36)$. Thus this kind of diagram exaggerates the importance of the first data pair. Still, it clearly illustrates the close proximity of Coulomb's data to the theoretically predicted inverse-square curve.

Next, rather than expecting, as with the earlier algebraic approximation, that to obtain an angular separation of 18 one would have to twist the micrometer to 126, and likewise, that to obtain 9 the twist would be 567, we may calculate more accurately what the micrometer should indicate (assuming an inverse-square law) in order to produce these particular angular separations, by using our more exact expressions:

$$\beta_m = (\alpha_m+\alpha)\frac{\left(\cos\frac{\beta}{2}\right)\left(\sin\frac{\alpha}{2}\right)^2}{\left(\cos\frac{\alpha}{2}\right)\left(\sin\frac{\beta}{2}\right)^2} - \beta.$$

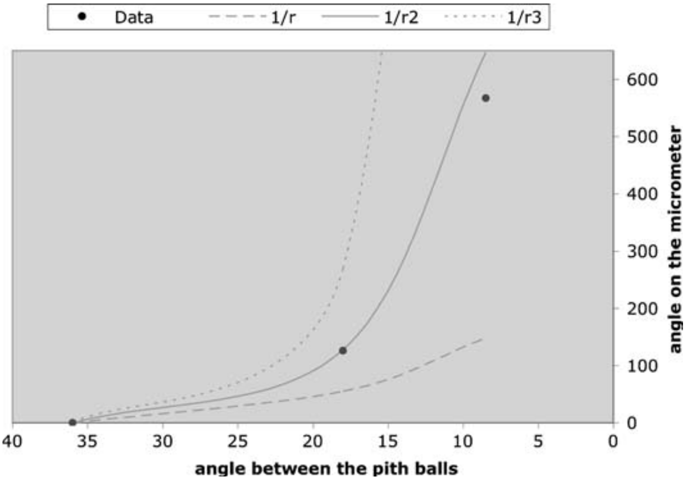

Fig. 3. The data from Coulomb's report of 1785

The result is that to obtain an angular separation of 18 the micrometer should be at 127.9, and for 8.5 the micrometer should be at 647.8. The latter seems to be much larger than the 567 which, using the small-angle approximation, corresponds to a separation of 9. This however just indicates the range within which the twist can vary to produce relatively small differences in separation. Conversely, we find that if the micrometer were set to 126, followed by 567, then the respective angular separations should be 18.11 and 9.06. The differences between separations of 18 and 18.11, and between 9 and 9.06, are quite inconsequential, and so the small-angle approximation is fair. Nevertheless, the analyses below, and the results in the Appendix, are carried out entirely in terms of the more exact equations, since some of the separations involved in our experiments are much greater than in Coulomb's reported trials.

Several additional physical considerations may be taken into account. To mention one, Michael Faraday experimented with a torsion balance and found that the inner surface of its glass cylinder tended to collect irregular amounts of electricity that exerted disturbing effects against the expected behavior of the movable pith ball.[16] To remedy this problem, Faraday attached two bands of tin foil, to the inner surface of the cylinder, above and below the plane of displacement of the movable pith ball. He connected the two bands and grounded them to ensure that any influence upon the pith balls would be uniform. James Clerk Maxwell, likewise, noted that adding charge onto the pith balls would induce electricity on the interior sides of the glass cylinder, which in turn could affect the behavior of the pith balls.[17] To reduce or control this effect he suggested that the interior of the cylindrical container should be metallic.

Finally, another noteworthy physical consideration is whether the electrical charges are evenly distributed over the surface of the pith balls. Coulomb expected that an inverse-square law could describe the repulsion between two charged spheres because

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[16] Faraday (1837), 7.

[17] Maxwell (1873), 44.

he tacitly applied a theorem from Newton's mechanics. A spherical material shell exerts gravitational attraction on outside bodies as if its entire mass were concentrated at its center. Likewise, Coulomb expected that a spherical distribution of charge would exert the same repulsive force as if all the charge were centrally located, and thus the repulsion between two spheres would be given by the line joining their centers. But in practice this expectation might not be satisfied because the surface charges are not fixed in place – they move. Accordingly, Maxwell objected that the repulsive action of each body would alter the distribution of electricity on the other, so that strictly speaking, the charge would not be distributed evenly on either surface, and therefore it should not be considered as concentrated at the spheres' centers.[18] This effect is greater at closer distances, whereby the charges would move towards the far sides of the spheres, producing a force apparently weaker than $1/r^2$ (that is, if we treat the charges as centrally located). Taking this effect into account, for Coulomb's separation of 8.5 degrees in his third trial, the apparent force of repulsion turns out to be about 5% less than $1/r^2$ in accord with the separation being smaller than 9 degrees.[19] The mathematical corrections necessary to account for the apparent decrease in force at close distances will not be taken into account in the following historical analysis, given that generally the effect is negligible. However, it is useful to bear in mind the qualitative relation: that at very small separations the force of repulsion should appear to be somewhat weaker than our approximations. In any case, such factors and others could disturb the operation of Coulomb's instrument, impairing its intended aim. Therefore, Maxwell expressed skepticism on the accuracy to which Coulomb's law actually might be confirmed by the torsion balance. Still, physics textbooks continued to assert the validity of Coulomb's experiment.

3. Historical Analyses of Coulomb's Experiment

Coulomb did not publish any results other than his three data pairs. Nor did he name or allude to any witnesses to his experiments. Yet he presented his apparatus to the Paris Academy and claimed that his results were "easy to repeat, and immediately disclose to the eyes the law of repulsion."[20] If we take these words literally, we might presume that 1.911 was not an unusual result, but that slightly higher or lower results readily were obtainable too.

However, not everyone obtained such telling results. John L. Heilbron noted that some scientists struggled to operate the torsion balance without much success.[21] For decades, it has become increasingly common to argue that Coulomb exaggerated the accuracy of his instrument. Given its high sensitivity, Samuel Devons commented that Coulomb "confuses that sensitivity with precision."[22] Other historians have agreed. Peter Heering has documented cases, especially in Germany, where experimenters failed to

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[18] Maxwell (1873), 43.

[19] Soules (1990).

[20] Coulomb (1785), 572. Also in Coulomb (1787), 578.

[21] Heilbron (1979), 476; e.g., P. L. Simon and G. F. Parrot as mentioned further below.

[22] Devons (1984).

confirm Coulomb's law.[23] Heering has claimed that "none of [Coulomb's] contemporaries succeeded in reproducing his results."[24]

Yet considering the prompt and wide acceptance of Coulomb's claims, especially in Paris, it is plausible that some of his contemporaries satisfactorily did employ (or witness) the torsion balance to evince the inverse-square relation, even if they did not proceed to publish results from such trials. Further, Coulomb's subsequent measurements of electric charge, carried out with his torsion balance, were of such high precision that they received striking confirmation in 1811 thanks to S. D. Poisson's mathematical analyses on the distribution of charge on the surfaces of conductors.[25] Poisson obtained deviations of less than 3.3% between his calculations and Coulomb's measurements, thus showing the accuracy of Coulomb's work in a domain in which he simply did not have the mathematical apparatus to have predicted these results independently of his experiments. This hardly speaks to the inherent difficulties and inaccuracies of Coulomb's device, and if we are to believe recent historical claims, then we might conclude that Coulomb alone could make his device work, and make it work so well that Poisson based an extraordinarily successful theoretical computation upon Coulomb's results.

Most of the objections that were later raised against the inverse-square law were actually based mainly on various other experimental devices and arrangements, not on Coulomb's torsion balance. Even the cases discussed by Heilbron and Heering do not rule out that results such as Coulomb's were obtained at least occasionally. In particular, Paul Louis Simon, at Berlin, disliked the torsion balance, but not explicitly because he could never obtain results comparable to Coulomb's, but because it was too complicated and unstable for use in demonstrations in his lectures.[26] Hence he designed a different experiment which he came to believe exhibited a direct inverse relation (but which can be shown to have demonstrated instead what no one could have computed at the time: the mutual induction between charged spheres placed close together[27]). In 1818, at Dorpat, Georg Friedrich Parrot reported results comparable to Coulomb's, but with deviations greater than 12%, which he attributed to defects in his apparatus and his lack of skill using it.[28] On the basis of other experiments, he too later argued for a direct inverse law. And in 1823, Ludwig Friedrich Kämtz used a torsion balance but concluded that the repulsion varies inversely with the separation to the power of 1.2; yet later he admitted that his small apparatus was inadequate to yield valid results.[29] As yet another example, consider Joseph Frick, at Freiburg, who in 1856 commented that "Experiments with the torsion balance before a large audience do not often succeed, for the air in a crowded room soon becomes too moist to admit of uniform results."[30] At any rate, it is certainly odd that in over two hundred years, and notwithstanding hundreds of discussions about

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[23] Heering (1995).

[24] Heering (1992a), 988.

[25] Poisson (1811).

[26] Simon (1807), 325–327.

[27] Jed Buchwald has carried out the computation using William Thomson's infinite series based on electric images.

[28] Parrot (1818), 22–32.

[29] Kämtz (1840), 150.

[30] Frick (1861), 264.

the torsion balance in textbooks and journal articles, that one apparently cannot find any published or unpublished records showing actual data similar to Coulomb's three data pairs.

In view of the lack of reports substantiating the accuracy of Coulomb's claims, Peter Heering attempted to reproduce Coulomb's torsion balance experiments. He carried out his work in the physics department of the University of Oldenburg, in the Research Group for Higher Education and History of Science, which is well-known for its many reproductions of historic experiments, especially in electricity and magnetism. A torsion balance was constructed following Coulomb's prescriptions, having in all its dimensions deviations of less than 10% from Coulomb's. However, Heering did not replicate all of the materials that Coulomb used. For example, the hanging wire was not pure silver but copper, it was not clamped but soldered, and the needle was made not of silk and Spanish wax but of PVC. Nevertheless, the properties of each part were quite similar to those prescribed by Coulomb, so that the apparatus should presumably yield comparable results. To give a sense of the difficulties involved, we may note that once Heering had all the components for the experiment in place it nevertheless took him six months of daily work to stabilize and calibrate the apparatus before being able to take meaningful measurements.

Even though the apparatus eventually behaved in a way that satisfied Heering, he observed effects that were not mentioned in Coulomb's memoir. When electric charge was imparted to the system by contact from a PVC wand with a metal tip, the movable ball did not simply retreat from the other one. Instead, because of the mechanical impact from the wand, it first swung away to an angle of around 95°, and then it returned to its initial position, where it stuck to the stationary ball for about 30 seconds, until finally, they repelled.[31] Coulomb described nothing of the sort. Moreover, Heering observed that the movable ball, under repulsion, did not remain stable, but slowly wandered irregularly by 2 or 3 degrees (every ten seconds or so). Nevertheless, Heering managed to obtain many series of measurements.

Heering reported that "in none of the experiments was it possible to obtain the results that Coulomb claimed to have measured."[32] Some series of his experiments could be characterized by exponents between 1 and 3, but only a few among them even yielded a definite exponent; in most cases the exponent seemed to vary during each experiment. As an example, Heering reported the following set of data, in the form of a diagram such as we provided above in Fig. 3 for Coulomb's values.[33]

micrometer	0	20	40	60	80	100	130	160	200	250	300	360
separation	32.5	26	22.5	19	16	14.5	13	10.5	8	6.5	5.5	4

Figure 4 plots these twelve data points against three theoretical curves, corresponding again to direct inverse, inverse-square, and inverse-cubed relations. While the first few data points lie in the vicinity of the inverse-square curve, the majority do not. Thus

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[31] Heering and Chevalier (1995), 67.

[32] Heering (1992a), 990.

[33] Heering (1992b), 14. See also Heering (1994), 55.

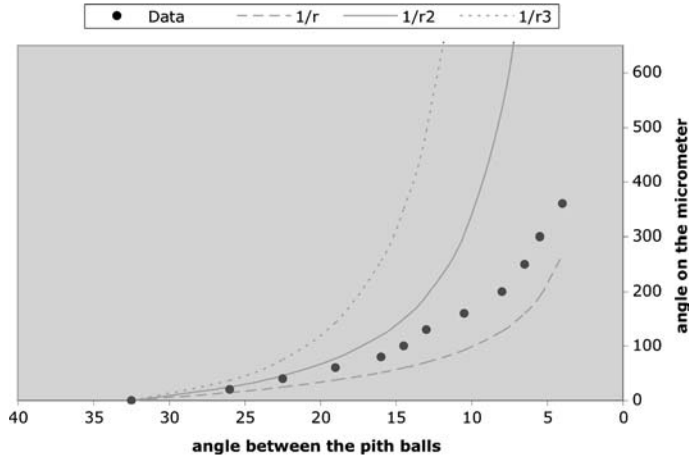

Fig. 4. The data from one of Peter Heering's experiments

the exponent seems to vary throughout the experiment. By using equation (2) to calculate an average exponent from these data points (though Heering did not do so) we obtain $n = 1.279$.

Consistent with this low exponent, Fig. 4 illustrates the divergence of the data from the inverse-square curve. (For a point-to-point comparison between each data item and the corresponding point on each curve, we need only trace a vertical line connecting any one data point to the curves above or below it.) As we noted above, this kind of diagram is plotted on the basis of equations that exaggerate the dependence on the first data point. Consider again equations (3):

$$h = \frac{\cos\frac{\alpha}{2}}{(\alpha_m + \alpha)\left(\sin\frac{\alpha}{2}\right)^2}, \quad \text{and} \quad \alpha_m = \frac{\cos\frac{\alpha}{2}}{h\left(\sin\frac{\alpha}{2}\right)^2} - \alpha.$$

We have used the latter equation to chart the micrometer reading α_m corresponding theoretically to each observed angular separation α. But, as we stated earlier, here h is permanently fixed by using only the *initial* data pair $(\alpha_m,\ \alpha) = (0,\ 32.5)$. To erase this over-dependence on the first data pair, Jed Buchwald suggests that we instead calculate an h for each data pair, average the set, and then use the average h to calculate the theoretical values of α_m. This would constitute a more stringent test on the degree to which the data fits any one curve. By so doing, we obtain Fig. 5.

This kind of diagram brings the three theoretical curves closer together. And here, the data sits even farther away from the inverse-square curve than in Fig. 4. By contrast, consider the same kind of diagram applied to Coulomb's data, Fig. 6. Here the data plainly select the inverse-square curve. Moreover, Coulomb's sparse data spreads across a greater range of total torsion of the wire.

Since Heering took more data than Coulomb reported, at least in the particular experiments in question, it might seem that the disparity with an inverse-square relation arises as the pith balls lose charge during the prolonged time for measurements. However, Heering ascertained the net effect of charge leakage at the end of the experiment (by bringing the micrometer back to 0 to see how much this last position of the movable ball

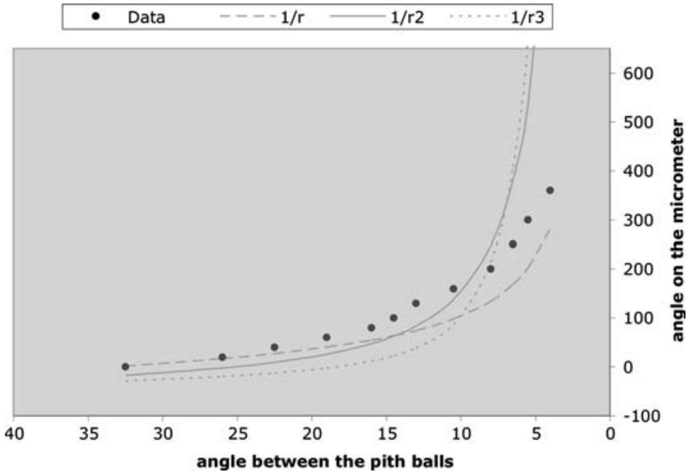

Fig. 5. The same data from Heering's experiment

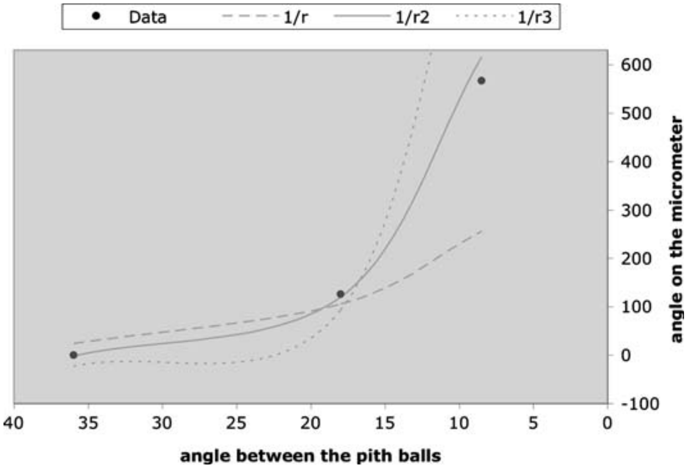

Fig. 6. The data from Coulomb's report of 1785

differed from the first). He found, in this and other experiments, that the effect was too small to account for the overall discrepancy.

At the time, the negative outcome was the sole instance in which the specialized group at Oldenburg, led by Falk Riess, had not reproduced the kind of results originally reported.[34] The main problem identified by Heering was that the movable pith ball and needle oscillated erratically. He found that these oscillations were caused mainly by electrostatic charge on the body of the experimenter, acting somehow upon the system. To eliminate that source of error, Heering surrounded the entire apparatus with a large

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[34] Dickman (1994), 500.

Faraday cage, a cylindrical wire mesh. He repeated his experiments and only then found approximate agreement with Coulomb's reported results.

In one of his experiments with the cage, Heering obtained the following series of data[35]:

micrometer	0	40	78	120	160	240
separation	22.5	14.5	11.25	9	7.75	5.75

This data, applied in pairs to our equations, yields an average exponent of 1.728. The deviation from 2.0 is attributable mainly to charge loss over time. So, thanks to the Faraday cage, the results show that the force of repulsion does approximate an inverse-square relation. One shortcoming of these results, however, is that they only spread over a range of total torsion of 245.75 degrees, compared to Coulomb's higher 575.5. Also, at their high end, Heering's data pairs (160, 7.75) and (240, 5.75) give a rather low exponent: 1.277. To illustrate the results over a higher range of total torsion, Heering reported another experiment that gave the following data:

micrometer	0	10	20	30	40	60	80	100	120	140	160
separation	35	31.25	29	26.75	24.75	21.5	19	17.5	15.75	15	14
continued:	180	210	240	280	320	360	420	480	540	600	
	13	12	11.25	10	9	8.25	7.5	6.75	6.25	5.25	

Here, the average exponent is 1.635. Again, since this series consists of twenty-one readings, it seems likely that prolonged charge leakage caused the exponent to be lower than Coulomb's. Also, at the very end, the elastic limit of the copper wire was exceeded. Therefore, while the exponent for the data pairs (480, 6.75) and (540, 6.25) is 1.499, the exponent for the pairs (540, 6.25) and (600, 5.25) is only 0.586.

At any rate, only by using the wire cage did Heering obtain results similar to Coulomb's. Did Coulomb use a comparable device? As early as 1781 E. F. Gattey had argued that a copper container served to isolate a magnetic compass, or an electroscope, from its surroundings.[36] Thus emerged the paradoxical realization that a metallic conductor serves better than "insulating" glass to isolate an electrical system. Yet what was eventually termed the Faraday cage was well-known only decades later (studied by Faraday in 1843), and Coulomb did not describe any such fixture for shielding his apparatus from charge on the experimenter (except, perhaps, the small copper tube sealed to the interior of the top of the glass tube, under the micrometer). Therefore, Heering conjectured that Coulomb did not compensate in any way for this problem.

Heering argued that passages in Coulomb's paper of 1785 suggest that Coulomb's experiments suffered accordingly. Coulomb, he argued, actually described these very oscillations of the needle, fluctuations of 2° or 3°, which would prevent precisely

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[35] Heering (1992b), 17.

[36] Blondel (1994), 107.

locating even its zero position. And, Heering continued, if a wire twice as thick were used, following Coulomb's suggestion to do so at one point, then it would be impossible to twist it 567 degrees without hampering the results or breaking it, since this thicker wire would have a workable torsion threshold of only about 300 degrees, as Coulomb himself noted. Therefore, Heering wrote, such passages "show that he [Coulomb] could not have made his measurements with any of the wires he mentioned in his memoir. Because of these arguments it seems reasonable to assume that Coulomb did not get the data he published in his memoir by measurement."[37] He concluded that Coulomb likely settled on the inverse-square law owing more to theoretical considerations, rather than doubtful measurements; that Coulomb perhaps idealized the actual experiment and reported results that were rooted in a thought-experiment.

Heilbron commented that "It appears from Heering's careful and resourceful labor that Coulomb either faked his numbers altogether or obtained them under experimental conditions materially different from those he reported."[38] Heering's analysis has been accepted by some historians,[39] while others have voiced reservations.[40] Meanwhile, by studying the narrative structure of some of Coulomb's experimental memoirs, Christian Licoppe has argued that Coulomb's rhetoric of purported instrumental precision was tailored to his primary audience, mathematicians of the Paris Academy who were ready to believe that electricity obeyed a simple mathematical law, like Newton's law of gravity, and to accept the rather unique and private results of a fellow member.[41]

Still, Coulomb wrote as if he had actually obtained his numbers *from experiment*, for instance, by commenting about "the day when I made the preceding trials."[42] Also, it is clear that Coulomb was aware that highly sensitive instruments can be affected by the static charge on the body of the experimenter. In 1782 he advised Jean Dominique Cassini, director of the Paris Observatory, on ways to try to isolate Coulomb's magnetic torsion compass from experimenters' charge or to dissipate their charge onto the damp basement floor and surroundings.[43] Yet the needle on each compass continued to move when the experimenters approached, and it remains unclear how they finally solved the problem.

In any case, there seems to be consensus that tension exists between Coulomb's elegant and simple narrative of 1785, and complications that arise in actual practice. In particular, Coulomb's wonderfully accurate numbers seem unlikely in light of Heering's results as well as Coulomb's admission in his original memoir that his balance suffered from defects that he planned to correct later.[44] Accordingly, some historians have advanced conjectures about tacit procedures or practical knowledge that Coulomb might have exercised but left unreported. If the movable pith ball, once charged, oscillated for a while before settling, then how could Coulomb possibly make his three readings

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[37] Heering (1992a), 991.

[38] Heilbron (1994), 151.

[39] E.g., Falconer (2004).

[40] Pestre (1994), 25; Heilbron (2003), 133.

[41] Licoppe (1994).

[42] Coulomb (1785), 575.

[43] Gillmor (1971), 148–149.

[44] Coulomb (1785), 570.

in only two minutes? Heilbron conjectures that Coulomb may have had to guess where the moving ball would come to rest, and then reported that guess as an actual reading of its position. Heilbron further proposed that

> Probably Coulomb put some torsion on the wire by twisting the knob even for the first data pair, although in the narrative he says that in its first position the needle balanced between the electrical force of the balls and the torsion given the wire by their repulsion alone. Coulomb's convenient round number for his first measurement ($f = q = 36^\circ$) suggests that he had his hand on the micrometer knob for the first setting and that he had a special way of estimating the equilibrium position of the needle.[45]

Thus, the apparent discrepancy between the text and the practical subtleties have led to various speculations about the actual operational procedures involved as well as the contemporary practices of scientific reporting. They also have led to informal but perceptive questions about possible unidentified material factors in the laboratory environment. Christine Blondel has wondered whether perhaps Coulomb's apparatus was glazed with an insulating varnish of gum-lac. Jed Buchwald, relying on the accuracy with which Heering reproduced Coulomb's procedure as described in print, suggested that Coulomb might have used a short-range sighting instrument, a simple telescope, to make his readings while staying a distance away from the apparatus. Maria Trumpler, and Heilbron, have even commented that perhaps factors such as a wig and a silk shirt could affect the charge on the experimenter.

But perhaps the Oldenburg reproduction did not accommodate all of Coulomb's requirements. After all, there were differences between the Oldenburg apparatus and Coulomb's, among which are the following. The thickness or composition of the glass cylinders made at Oldenburg might be sufficiently different from Coulomb's to not insulate as well.[46] The extremities of Heering's copper wire might have suffered in the soldering process. In principle, if the thickness of the wire is diminished at a given point, without breaking it, then the wire at that point will be more easily twisted and thus may give rise to irregularities in its behavior, even facilitating drift. Furthermore, the pith balls used by Coulomb might have been smoother and less porous than Heering's, depending on what kind of pith was used and how it was prepared. If so, the more irregular material will not foster an even distribution of charge around its hardly spherical surface. Electricity might then move irregularly on the surface and even inside the pith balls, which therefore will not behave as expected.

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[45] Heilbron (1994), 156.

[46] In 1764, Jean Antoine Nollet tested the conductivity of different kinds of glass, and he concluded that the least conductive glass was the hardest and best vitrified, such as was produced at St. Gobain (Picardie) and at Cherbourg, and he preferred leaded glass produced in England and Bohemia; see Hackmann (1995), 36. Likewise, in 1856, describing the components of electrical instruments, Joseph Frick remarked: "The conducting power of glass varies very much. Common green glass (not white glass colored green by copper or chromium) generally insulates best. Some sorts of white glass also, the Bohemian among others, are good insulators. Its insulating properties may easily be tested, by attaching to it pith balls by fine linen threads, and imparting to them electricity; or by touching a well-insulated electrometer with a rod of the glass." Frick (1861), 253.

Meanwhile, perhaps in response to the multiplicity of possible variables, Peter Heering has suggested that "it is not possible to decide how Coulomb came to the data he published."[47] All in all, Coulomb's most famous contribution to physics, a paradigmatic example of the application of mathematics to the experimental science of electricity, has become riddled by historical uncertainties. To remove such ambiguities, we will show that, actually, Coulomb's results can be procured without material or procedural alterations or rhetorical arguments, by following just what he told his readers to do in print.

4. New Replication of Coulomb's Experiment

In the Spring of 2005, at the California Institute of Technology, Jed Buchwald and I co-taught a history course that aimed to analyze and reproduce a selection of influential experiments in physics. As part of that course, in May I prepared a workable torsion balance based on an apparatus that had been constructed previously at M.I.T., trying to closely follow Coulomb's prescriptions of 1785. I continued to refine its materials and operations over the following three months. My experimental research evolved through several stages characterized by changes in the material components of the apparatus together with variations in the operational techniques attempted. The evolving stages, procedures, difficulties, and all numerical results are reported in detail in the Appendix.

Over the four-month period, experiments were carried out with five kinds of wires of various metals and diameters (along with various other component materials, and thus yielding various results, as described in the Appendix). Coulomb used a silver wire, but only after months did I manage to obtain silver wire, 99.99% pure, and to use it for four weeks. Coulomb reported that his wire was so thin that a segment one (Paris) foot long (32.48 cm) weighed merely 1/16th of a Paris grain (3.32 milligrams). By considering the weight of pure silver, one estimates that his wire had an average diameter of 0.035 mm (American Wire Gauge 47), about as thin as a human hair.[48] Our wire had a similar average diameter of 0.051 mm (AWG 44), which suited the experiment, since Coulomb advised that a wire nearly twice as thick as the one he employed would give even better results.[49]

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[47] Heering (1994), 66.

[48] 1 Paris foot = 12 pouces $\approx$ 324.84 mm $\equiv l$, 1 Paris grain $\approx$ .053115 grams. The density of pure silver (at 20°C and standard atmospheric pressure) is 10.492 grams/cm^3. By treating Coulomb's silver wire as a cylinder, we have: *wire volume* $= l\pi r^2 = 3.32$ mg $\div$ 10492 mg/cm^3 = .316 mm^3; therefore, $r = .0176$ mm, and the wire's diameter is .0352 mm, which is approximately equal to .03556 mm (AWG 47). For a comprehensive review of the literature on the properties of silver, including density, tensile strength, and moduli of elasticity, see Smith and Fickett (1995).

[49] The composition and quality of Coulomb's silver wires is unknown, but metallurgical analyses have been carried out on fragments of musical wires of the period, showing that old iron wires, for example, compare very favorably in quality with modern commercially pure iron; having excellent surface finish, scarce nonmetallic inclusions, and carbon content of less than one hundredth of a percent. In his memoir of 1784 Coulomb analyzed the properties of fine harpsichord wires, iron and brass, and his data on strength and rigidity have been found to be dependable. See Goodway and Odell (1987).

After months of using various means of suspending the wires, a way was at last devised to effectively clamp the thin wire in a small metal clasp gripped by a pencil-holder under the micrometer assembly. A small cylinder-pivot was also devised, very much like the one described and illustrated by Coulomb, and suited to clamp tightly onto the bottom end of the wire and hang there while balancing the needle. As for the needle itself, Coulomb's specification was initially followed to produce several waxed threads that were sufficiently rigid to use in experimental trials, but which in the end were problematic. One needle sagged a bit too much, and another (made from a more rigid sealing wax) reacted to the charge on the pith balls, and at the moment there was no insulating varnish with which to coat it, *per* Coulomb's advice. This latter shortcoming could have been remedied but time grew short. Therefore, a very thin and light needle was used that had been made earlier from melted plastic and which had served well in many series of trials, prior to obtaining the silver wire.

As for the pith balls, experiments were performed with several variations, all made from dehydrated, polished pith. Most such experiments used a pair that was painted with metallic leaf. But in the end, for the series of results now to be discussed, only one pair was used, both having bare surfaces, as Coulomb's apparently did as well (since he did not specify gilding). Only towards the end of the trials was this approach attempted, for the following reason. In his account on the torsion balance, Faraday had cautioned that if the balls are made of bare pith they are rather useless: "If of pith alone they are bad; for when very dry, that substance is so imperfect a conductor that it neither receives nor gives charge freely, and so, after contact with a charged conductor, it is liable to be in an uncertain condition."[50] Instead, Faraday recommended smoothly gilded pith. Accordingly, Maxwell described Coulomb's setup as allegedly involving elder pith "smoothly gilt."[51] But since Coulomb did not specify gilding in these experiments, whereas he did for a different one,[52] we conclude that he used bare pith. Our torsion balance and its parts are pictured in Fig. 7.

Table 1 lists the dimensions of significant parts of our apparatus, comparing them to those given in Coulomb's memoir of 1785 (converted into metric units).

Coulomb did not report the thickness of the glass walls of the large cylinder. In the present case, the approximate thickness is 6 mm, so the interior diameter is 28.8 cm. (As for the glass tube, its interior diameter is 4.8 cm.) Several other components and conditions of the experiments warrant attention, but they are left to the Appendix. Consider now the main procedures employed.

First of all, consider the widespread claim that it is difficult to find the zero position of the movable ball because of unavoidable oscillations. In most trials there was simply no such problem, despite the uncovered hole on the glass lid (which might have let in air currents). That said, in a few quite rare instances the needle did drift erratically. When this happened, the needle was observed and its many extreme positions were recorded (even drifting as far as 17° to one side) over twenty minutes, but no pattern whatsoever was found. Among the few instances in which this problem arose, two were cases in which

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[50] Faraday (1837), 8.

[51] Maxwell (1873), 264.

[52] Coulomb (1785), 576.

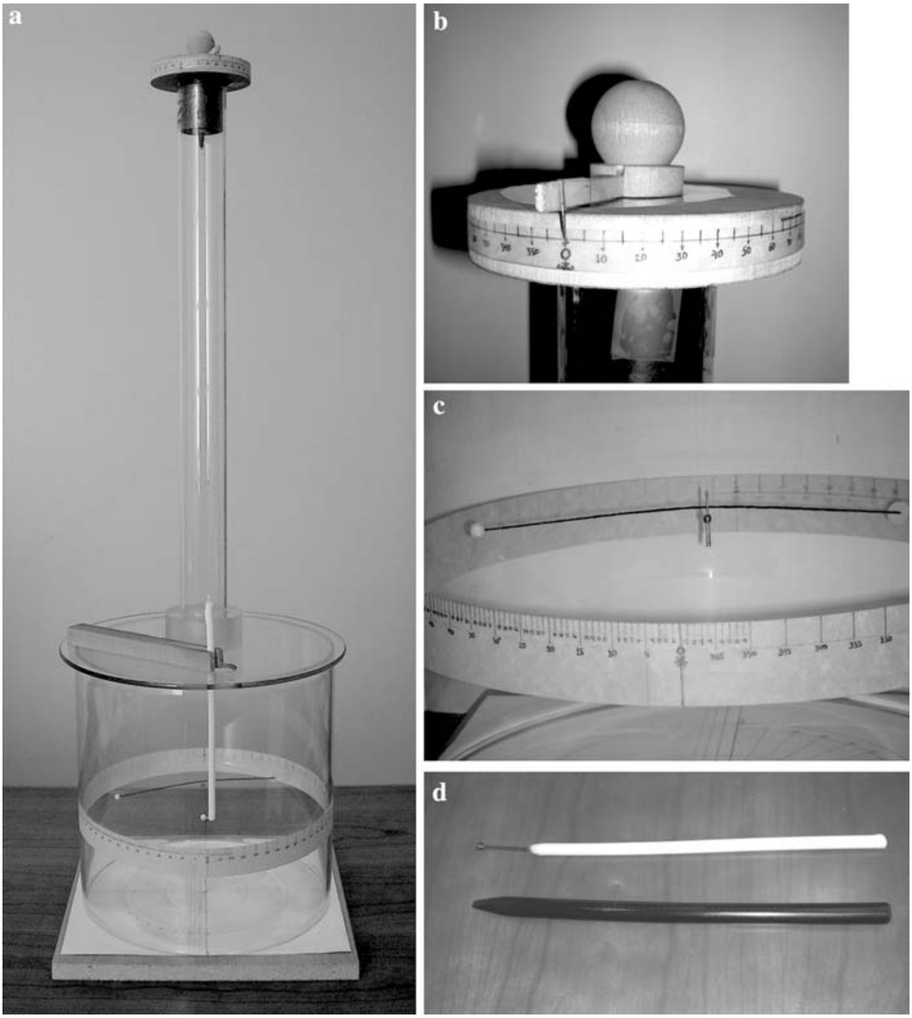

Fig. 7. Reproduction of Coulomb's torsion balance

Table 1. Comparison of the dimensions of the components of two torsion balances

Components	Reported in 1785	August 2005	Deviations
glass cylinder, diameter	32.48 cm	30.0 cm	−8%
glass cylinder, height	32.48 cm	30.0 cm	−8%
apertures on lid, diameters	4.51 cm	3.1 cm	−31%
glass tube, length	64.97 cm	61.5 cm	−5%
silver wire, length	75.79 cm	75.0 cm	−1%
silver wire, diameter	0.035 mm	0.051 mm	+31%
needle, length	21.66 cm	20.5 cm	−5%
pith balls, diameter	4.5 to 6.8 mm	6 mm	±18%

the wire had already been twisted repeatedly through large angles. On one occasion the movable ball and needle had been left suspended overnight, without support (which can deform the needle), and the next day it suffered from the irregular drift. It accordingly seems likely that problems of this sort were caused by damage suffered by the delicate wire, either in the process of experimenting, handling it, subjecting it to prolonged stress, or perhaps even from factory defects. At any rate, the solution was to discard that strand of wire, and to proceed with a new strand after which the movable ball usually maintained a constant position. Some slight tendency for it to drift was observed, in proportion to the thinness of the wire, but even with an extremely thin wire (48 AWG) the fluctuations rarely exceeded 1°. To confirm this, a stationary digital camera was used to record its position. Recording for 9 minutes (the memory limit on this small camera), repeatedly, and playing the videos in a large image format, confirmed that the ball remained practically fixed in place.

Returning to Coulomb's memoir, note that nowhere in it did he state that the zero position of the movable ball could not be identified because of oscillation. What he did precisely write was that *after* the initial experiment, in which the wire was twisted by a considerable amount, the position of the needle in a subsequent trial would not return to the initial zero position, by about 2 or 3 degrees:

> In repeating the preceding experiment, one will observe that when using a silver wire as thin as that which we have employed, which for the force of torsion of an angle of 5 degrees requires only one 24 thousandth of a grain, approximately, however still be the air, and whichever precautions one takes, one will not be able to match [répondre] the natural position of the needle, when the torsion is null, but to nearly 2 or 3 degrees.[53]

This does not assert that erratic oscillations accompany the needle's failure to return to zero. It merely specifies the approximate final distance from zero. For a series of experiments using the same wire, one would need to compensate for the problem, and, Coulomb accordingly proceeded to explain how to do so. He advised that

> Thus, to have a first trial to compare with the following, it is necessary, after having electrified the two balls, to twist the wire of suspension by 30 to 40 degrees, that which together with the separation of the two balls observed, will produce a force of torsion sufficiently considerable, so that the 2 or 3 degrees of uncertainty in the first position of the needle, when the torsion is null, do not produce in the results a sensible error.

Coulomb required – in a clever attempt to swamp the error – that the initial small divergence of the needle from its "natural" position at zero could be overwhelmed by increasing the total force of torsion on the wire. His words "a first trial," in the context

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[53] 1/24000 Paris grain ≈ .0022 mg. "En répétant l'expérience qui précède, l'on observera, qu'en se servant d'un fil d'argent, aussi fin que celui que nous avouns employé, qui ne donne pour la force de torsion d'un angle de 5 degrés, qu'un 24 millième de grain, à peu près, quelque calme que soit l'air, & quelques précautions que l'on prenne, l'on ne pourra répondre de la position naturelle de l'aiguille, lorsque la torsion est nulle, qu'à 2 ou 3 degrés près." (1785), p. 574. Here the word "répondre" is translated in the same sense that Coulomb used it in the two other instances in his memoir (both on p. 571; and likewise "répond" on p. 576), as meaning "to correspond"; though it may also be taken to mean "to answer."

of the previous sentence in which he establishes that he is talking about a repetition of the initial "experiment," suggest that by "a first trial" he was referring to the first of a new sequence within the *second* experiment. Accordingly, when he initially described the operations that led to the results 36, 18, 8.5, he called these three trials "*Essais*" and the overall procedure the "*Expérience*."

In his memoir of 1784 on the properties of wires, Coulomb had already discussed at some length the shift of the zero position of a wire following significant torsions. Depending on the thickness of the wire, and the angle to which it has been twisted, that wire will not return exactly to its initial position, but will exhibit a "displacement of the center of torsion" by returning only to some positive position away from zero.[54] For a fixed setup, as in his electrostatic torsion balance, a minor displacement will not be observable if the stationary ball is kept in place, because when the balls are in contact their centers are separated by about 5 degrees. The 2 or 3 degrees of uncertainty in the natural position of the needle would scarcely affect the separation when the total torsion is large. But if the total torsion were, say, only 33 degrees, then the initial uncertainty would have a considerable effect on the observed separation. Coulomb therefore advised an increase in the initial torsion for such a trial, by adding "30 to 40 degrees." Given this explanation of Coulomb's words, along with our observations of the stability of the needle, we conclude that Coulomb did not observe any significant problems or oscillations when, at the very beginning, he positioned the needle and movable pith ball at the zero mark.

To prepare the apparatus for experiments it was necessary to align the wire and the hanging metal pivot at the center of the large cylinder. Subsequently, the correspondence between the orientation of the micrometer and that of the movable ball (under no repulsion) was then examined. Having initially set both to zero, the micrometer was turned to arbitrary positions to (promptly) record the position to which the ball then moved. In one sequence the average positional differences were 2.8°, which seemed tolerable; in others it was less.

Nothing was used to dehumidify the air or experimental apparatus, nor to measure temperature or moisture. An ebonite rod was used to provide charge; it was rubbed harshly on a bundle of cat hair. Static charge was then picked up by a round metal pinhead (nearly 4 mm in diameter) affixed to the end of a plastic stick, which was subsequently inserted into the large cylinder to lightly and briefly touch the stationary pith ball. From prior experiments, it had become evident that to obtain numbers comparable to Coulomb's it does help to take only three or four measurements, in order to minimize any effects of charge loss, as he himself advised. This guideline was followed throughout the next series of experiments.

The first trials with this particular experimental setup proceeded as follows. The bundle of cat hair was rubbed as usual with the ebonite rod; it was then touched to the round pinhead which was then promptly inserted into the glass container and tapped lightly to the stationary ball. The movable ball immediately swung away and soon settled at a distance. It remained quite still, showing only faint oscillations of about 1 mm or less. The first reading gave an angular separation of 27 degrees, corresponding to zero torsion

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[54] Coulomb (1784), 255–261.

on the micrometer. The micrometer was then turned arbitrarily to 90 degrees, after which the movable ball settled, also stably, at 14.3. Next, the micrometer was turned again an additional quarter turn so that it now pointed to 180 degrees, at which point the movable ball settled (stably) at 10 degrees. After those three readings were obtained, the micrometer pointer was turned back to 0 to ascertain the charge loss. By then, 2.5 minutes had passed, yet the movable ball settled at 26, thus exhibiting a loss of merely 1° from the initial position of 27. Afterwards, the charge was removed from the pith balls, as usual, by inserting a wire into the container to gently touch them. Having removed the stationary ball, and after several minutes, the movable ball came to rest back at the zero position.

In the next experiment the initial separation was in the neighborhood of 37 degrees, but it soon decreased to 36, where the ball lingered. Given the coincidence with Coulomb's reported initial separation, the micrometer was promptly turned (*per* his own report) to 126. Again the ball settled nicely, this time at 19.5 degrees. Next, the micrometer was turned to 567 degrees, and the ball came to rest at 8.5 degrees – which is the *very number* reported by Coulomb. (The digital camera recorded these trials.) Finally, the micrometer was turned back to 0, and the ball settled at 35.5 degrees, after a total of about 4 minutes since charge had been imparted. Hence there was a remarkably low loss of charge, only half a degree decrease from the initial position of 36.

Next, charge was removed, and the ball gradually moved back towards the zero position. It did not however return to 0, remaining even after ten minutes at 7°. This was very likely a consequence of having subjected the 0.05 mm silver wire to a total torsion of 575.5 degrees, which may have stressed it past its elastic limit, so that it took a new set.[55] Coulomb too reported that his needle did not return to zero, but in his case the displacement was 2° or 3°. Having exceeded the elasticity limit for the present wire, it would therefore be reasonable to replace it with a new one. Since however the results obtained thus far seemed to be quite stable, several further measurements were made in this series, using the new set-point of the wire.[56] The apparatus was adjusted to the new zero position (by turning the micrometer until the ball settles at zero on the lower scale, and then rotating the micrometer's paper scale in the opposite direction until its zero line meets the pointer), and more measurements were carried out.

This time the initial separation was 44 degrees. One did not want to twist the wire too much (to avoid once again exceeding its elastic limits), and so the micrometer was turned arbitrarily to 120, which resulted in a position of 27; it was then turned to 400, which resulted in a position of 16.5. After this trial, once the balls had been discharged, the movable ball did return to the (new) zero position – thereby indicating that the wire had indeed taken a new set-point, though we shall see in a moment that the effects of its having previously exceeded the elastic limit did become apparent. So a fourth experiment

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[55] Coulomb's wire, approximately 0.035 mm in average diameter, was thinner than the one used here. We did not investigate the threshold at which our wire exceeds its elastic limits.

[56] These results also would reveal whether the wire had been stressed so far past its elastic limits that it was no longer behaving in accordance with Coulomb's proportionality between torsion and twist, that is, whether the wire had not taken a new set-point but had become permanently plastic.

was carried out. This time the initial separation stabilized at 31, so it seemed reasonable to follow Coulomb's procedure, multiplying the separation by 3.5 to calculate the next position of the micrometer, namely 108.5. This produced a separation of 17 degrees. The initial separation was then multiplied by 15.75 to set the third position of the micrometer, namely 488.25 (or thereabout), which resulted in a final separation of 8 degrees.

Throughout these four experiments, the movable ball remained so stable that the observational inaccuracy was judged to be no greater than 0.3 degrees. The inaccuracy in reading the micrometer's position was slightly greater, about 0.5 degrees, owing to its smaller scale. The measurements were used now to calculate the corresponding average exponents according to the equation given earlier. The results, along with the data, are summarized in the following tables. (Above each column is posted the time corresponding to the readings, with the time of the first reading set to 0 minutes with 0 seconds as a reference.) The maximal effects of the observational errors on each calculated exponent are negligible, being approximately only ±0.040, and are therefore not listed.

1) 11:51PM Aug. 22. *A1*	0:00	1:23	1:58	exponent	deviation	2:30 min.
micrometer	0	90	180	1.894	±0.222	0
separation	27	14.3	10			26
2) 12:30AM Aug. 23. *A2*	0:00	0:58	2:44	exponent	deviation	4:20 min.
micrometer	0	126	567	1.956	±0.307	0
separation	36	19.5	8.5			35.5
3) 1:06AM Aug. 23. *A3*	0:00	1:10	2:20	exponent	deviation	3:50 min.
micrometer	0	120	400	2.277	±0.173	0
separation	44	27	16.5			42
4) 1:40AM Aug. 23. *A4*	0:00	1:17	2:57	exponent	deviation	6:07 min.
micrometer	0	108.5	488.25	2.068	±0.249	0
separation	31	17	8			30

Average of these four exponents: 2.049

First, one must note the apparent effect of large torsion on the silver wire. After it was subjected to a total torsion of 575.5 degrees, both of the succeeding experiments produced exponents higher than 2. That said, it is nonetheless remarkable that these four experiments produced results so close to that very exponent. The second experiment is the most striking, having produced a mean value of 1.956. We chart its results in Figs. 8 and 9; both representing the same data, in the two formats we discussed earlier.

This experiment shows a remarkable agreement in five out of the six numbers reported by Coulomb. Moreover, it yields a seemingly "better" exponent, 1.956 rather than the 1.911 that follows from his numbers:

1785, Coulomb's values				exponent	deviation	2 min.
micrometer	0	126	567	1.911	±0.070	0
separation	36	18	8.5			~ 35.5

But note that Coulomb's reported value of 18 is closer to the theoretical prediction of 18.11. In that sense, his results are on the whole better than those from experiment

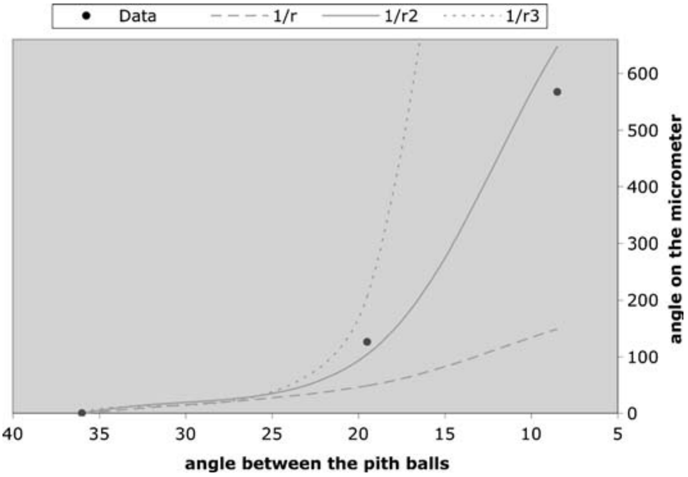

Fig. 8. The data from experiment *A2*, which resembles Coulomb's

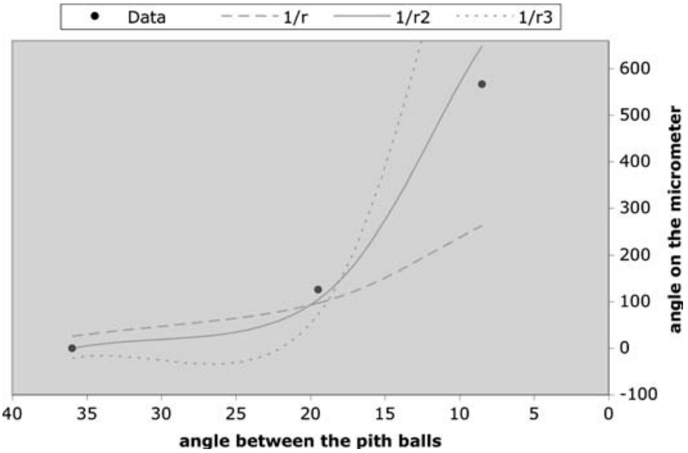

Fig. 9. The same data from experiment *A2*, which resembles Coulomb's

A2. The average exponent from *A2* seems better only because it stems from a mutual compensation of the exponent calculated from 36 and 19.5 (which is 2.263), and the exponent calculated from 19.5 and 8.5 (which is 1.650), the one high, the other low, averaging to 1.956. Thus, when assessing the stability of an exponent throughout an experiment, it is necessary to attend to its high and low bounds as conveyed by the standard deviations. These four experiments do not show the same constancy for the exponent as Coulomb's.

The results were nevertheless so remarkable that the next day more experiments were carried out with exactly the same setup, that is, without replacing the wire or touching anything other than to align the zero positions. Jed Buchwald provided a higher resolution digital camera for recording all the subsequent experiments. Trials were conducted as before, and the results are summarized in the following tables. As on the preceding night, the humidity was low enough that the charge loss over time was minimal.

5) 5:05PM Aug.23. *B1*	0:00				exponent	deviation	$\sim$ 6 min.
micrometer	0	100	200	300	2.216	±0.264	0
separation	41	26.5	20.5	17			40.5
6) 6:00PM Aug.23. *B2*	0:00	1:33	3:20	4:30	exponent	deviation	6:19 min.
micrometer	0	90	200	360	2.707	±0.058	0
separation	59	44	35.8	29.6			58.6
7) 6:39PM Aug.23. *B3*	0:00	1:18	2:15		exponent	deviation	3:29 min.
micrometer	0	45	120		1.678	±0.132	0
separation	19.6	11	6.5				19.3
8) 6:55PM Aug.23. *B4*	0:00	1:10	2:13		exponent	deviation	3:45 min.
micrometer	0	180	360		2.357	±0.076	0
separation	48	26.5	20.3				47.5
9) 7:12PM Aug.23. *B5*	0:00	1:15	2:28		exponent	deviation	3:46 min.
micrometer	0	200	400		2.266	±0.141	0
separation	48	25.4	19				47.5

Average of these five exponents: 2.245

Four of these five experiments resulted in unusually high exponents. Given the results from experiments *B1* and especially *B2*, the micrometer was twisted less in experiment *B3*, to see whether that would affect the exponent. Indeed, for total wire torsions much less than 200°, an exponent of almost 1.7 resulted. Next, for experiments *B4* and *B5*, which happened to give the same initial separation, similar measurements were taken to test how a slightly greater torsion affects the separation. The results of these two trials were quite close to one another. It may also be noted that throughout these experiments the movable ball had a well-behaved tendency to return to the zero position at the end (under no repulsion; having removed the stationary ball). For example, in *B1* the ball settled back to 1.5° when the micrometer pointed to zero (requiring only a slight adjustment for the following experiment). And after experiment *B5*, the movable ball returned precisely to its initial zero position, remaining there even 30 minutes later.

These five experiments exhibit an unusually high range of variation among the exponents compared to the previous ones. More tests were necessary. The likely cause of such unusually high exponents was that once the silver wire had been subjected to a large total torsion, the wire's elastic modulus decreased, because its elastic limit was exceeded. This generates a larger computed exponent, as we can see from equation (2) above, with $\theta_\alpha > \theta_\beta$.

To test this conjecture, the wire was replaced with a new strand, painstakingly handled with care and clamped onto the micrometer and pivot. The same pith balls and needle were used, but, in the process, the delicate plastic needle broke at one of its extremities (though losing only a 7 mm bit). Its length being still sufficiently long, 19.8 cm (compared to Coulomb's reported 21.66 cm), it was again used. The length of the new strand was also slightly different, 70 cm (it being difficult to ensure an identical length in all experiments due to complications in the clamping process). These two changes in length, both negligible, were the only material differences from the series *A* and series *B* experiments. As usual, once the apparatus had been reset with new parts and adjustments, it was left undisturbed overnight, so that any irregularities in moisture and charge distributions, caused by manually handling the glass cylinders, pith balls, and other components, would dissipate.

The next day, the trials proceeded as before. Given the new wire, the key question was now whether the next experiments would give results similar to Coulomb's. By chance the initial separation for experiment *C1* turned out to be 36 degrees – the very number reported by Coulomb. Accordingly, the micrometer was now turned to 126, to again compare the results. However, it was not turned next to 567, as it had been before, because that might alter the new silver wire's elastic modulus. It was instead turned to 300. The results, together with those from the subsequent trials, are summarized in the following tables.

10) 3:49PM Aug.24. *C1*	0:00	2:08	3:32	exponent	deviation	5:00 min.
micrometer	0	126	300	1.911	0.311	0
separation	36	19.3	12			34.5
11) 4:10PM Aug.24. *C2*	0:00	0:56	1:59	exponent	deviation	3:05 min.
micrometer	0	180	360	2.025	0.280	0
separation	40	20	14			39
12) 6:35PM Aug.24. *C3*	0:00	1:07	2:17	exponent	deviation	3:40 min.
micrometer	0	150	300	2.072	0.203	0
separation	49	28	20.5			47.7

Average of these three exponents: 2.003

Remarkably, the exponent calculated for the first experiment turns out to be the very same one that emerges from Coulomb's reported numbers. This experiment was filmed in a continuous, high-resolution digital video that clearly shows: all the indications on the micrometer, the stability of the movable ball in electrostatic balance, and, the alignment of this ball and needle with the markings on the cylinder scale: 36, 19.3, 12. The next two experiments resulted in exponents higher than 2, if only very slightly so, suggesting again that even at a total torsion of little over 300 degrees, the 99.99% pure silver wire of .05 mm in diameter became slightly less elastic.

The same wire was kept in place, all parts of the system were prepared again, and left overnight. The next day the following results were obtained:

13) 5:56PM Aug.25. *D1*	0:00	2:16	3:46	5:08	exponent	deviation	7:23 min.
micrometer	0	126	270	360	1.679	±0.277	0
separation	36	18.3	12.2	10			34
14) 6:25PM Aug.25. *D2*	0:00	1:15	2:15	3:03	exponent	deviation	4:26 min.
micrometer	0	100	200	300	1.956	±0.113	0
separation	52.5	34	26	21.5			50
15) 7:05PM Aug.25. *D3*	0:00	1:13	2:08	3:08	exponent	deviation	4:17 min.
micrometer	0	100	200	300	1.878	±0.022	0
separation	56	35.5	27	22.5			53
16) 10:25PM Aug.25. *D4*	0:00	1:18	3:03	4:47	exponent	deviation	6:10 min.
micrometer	0	100	200	300	1.919	±0.057	0
separation	59	39	30	25			55

17) 10:55PM Aug.25. *D5*	0:00	1:39	2:55	exponent	deviation	3:42 min.
micrometer	0	250	500	2.069	±0.066	0
separation	65	32	24			64
18) 11:15PM Aug.25. *D6*	0:00			exponent	deviation	∼4 min.
micrometer	0	200	400	2.072	±0.203	0
separation	48	25.4	19			47.5

Average of these six exponents: 1.929

Once again, in the first experiment, the initial separation happened to be 36. So, as before, the micrometer was turned to 126, obtaining this time an angular separation of 18.3, quite close to Coulomb's reported 18. But again, one chose not to subject this wire to a torsion as high as Coulomb used, to not damage it. (It takes several hours of work to carefully set a new wire and prepare the system anew, plus the wait overnight). All subsequent angles of torsion were chosen arbitrarily. The results of this set of experiments confirmed that the unusually high exponents in the experimental *B* series stemmed from the use of a wire whose elastic response had decreased because of high twists.

Experiments *D2*, *D3*, *D4*, constitute a set that shares the same sequence of indications on the micrometer. Significantly, the average exponents in this set are relatively close to one another. Moreover, the results of experiment *D4* are particularly noteworthy. Its data is synthesized in Figs. 10 and 11.

Not only does this experiment yield a slightly higher average exponent, 1.919, than Coulomb's, but it also exhibits a slightly lower standard deviation. Recall that Coulomb's data pairs yield the exponents 1.981 and 1.842. Similarly, the data pairs from experiment *D4* yield: 1.999, 1.883, 1.874. Therefore, it was possible indeed for Coulomb to obtain numbers that implied a considerable constancy of the exponent throughout the experiment.

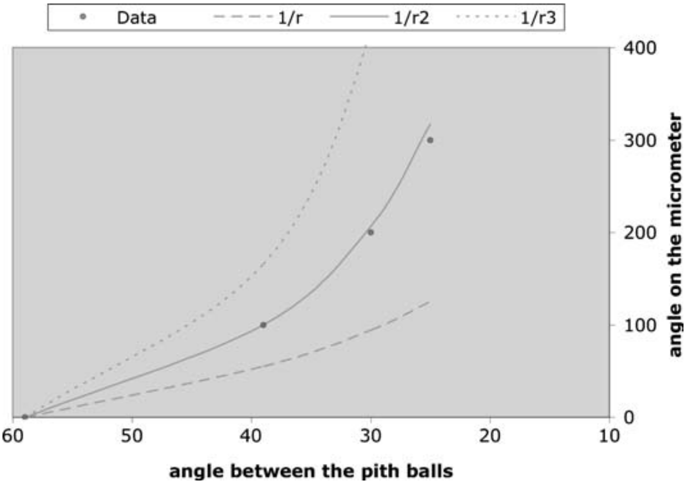

Fig. 10. The data from experiment *D4*

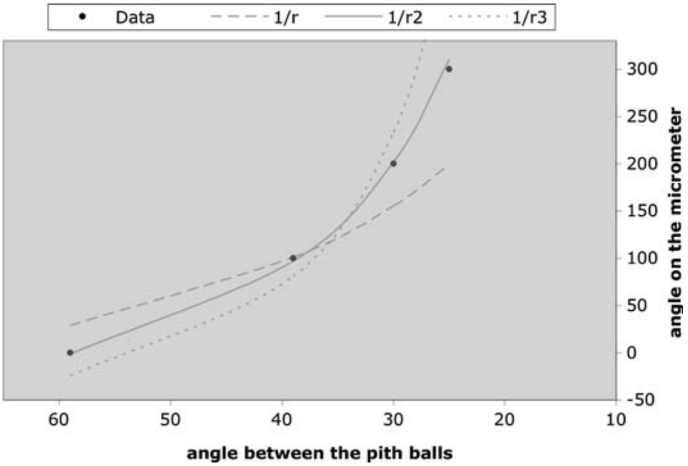

Fig. 11. The same data from experiment *D4*

The eighteen experiments described above include all that were conducted with the particular material setup in question: the stage characterized by the clamped silver wire and the highly polished bare pith balls. None have been omitted. Prior to this series of eighteen, hundreds of other trials were carried out using material components and alternative operational procedures that differed more from Coulomb's prescriptions than those employed here. (All of the numerical measurements that were taken during those prior experiments are reported in the Appendix.) It seems to be beyond the possibility of coincidence that of all the different kinds of experiments attempted, the one ensemble set that most closely resembles Coulomb's reported results corresponds to the particular stage in which the components and conditions best matched his prescriptions. Moreover, the behaviors of the system, in this series, matched Coulomb's description.[57]

The series of eighteen experiments over four days is sufficient for drawing definite conclusions. The average exponent obtained from among all of these experiments is 2.055. If, by contrast, we disregard all experiments from series *B*, along with *A3* and *A4* (which likewise followed the highest wire torsion), then the average from the remaining eleven experiments is 1.948. We see that the exponent entailed by Coulomb's reported numbers is entirely possible. It is also likely – and quite probable, given experimental practice at the time – that Coulomb, having carried out multiple measurements, just chose a set to report that was both typical and that nicely exhibited the quadrupling of the force with halving of the distance. Since he did not record or work with an expression for the exponent, he would naturally have chosen set-piece distances to exhibit the coherence between experimental values and an inverse-square. Also, there is good reason to con-

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[57] One would have carried out more experiments in this stage, but there were time constraints. Instead, the next experiments used gilded pith balls and another waxed-thread needle, but defective results ensued owing again to the lack of an insulating varnish for the waxed needle (a covering of synthetic shellac did not work). Still, the apparatus and all the components remain in place at the California Institute of Technology, and more experiments or variations can be carried out.

clude that Coulomb would easily have found an appropriate initial position from which to halve the distance, even if by letting a slightly greater initial separation decay to a numerically convenient position. Moreover, Coulomb's results exhibit an approximate constancy in the exponent which has also been evidenced, as we saw in experiment *D4*.

In contrast with Heering's experience, but not with Coulomb's report, no irregular behavior of the needle was observed. Moreover, the body of the experimenter did not disturb the needle, not even at distances of only 10 cm from the glass cylinder, reached while taking readings. Several witnesses helped make and confirm some of the readings and likewise did not visibly disturb the system. The video camera (battery powered) recorded such trials, at various distances, and neither did it affect the stable behavior of the needle.

We are led to the following conclusions. First, it is readily possible to locate the 0 position of the movable ball without any problematic oscillations. Second, the turning of the micrometer to positions such as 126 and 567 degrees is accomplished without difficulties or delays. As for the initial angular separation of 36 degrees, it is noteworthy that this very separation arose not once but in three out of eighteen experiments, suggesting that it would likewise be a separation that Coulomb could readily obtain. The fact that two out of Coulomb's three observations consist of integer numbers is not peculiar in light of the present trials. Third, Coulomb's halved distance of 18 degrees, corresponding to a micrometer position of 126, can be compared to three present instances in which likewise the micrometer was turned to 126 (following initial separations of 36). Our results – 19.5, 19.3, 18.3 – suggest that Coulomb's 18 is well within the range of obtainable values. Likewise, the identical match between Coulomb's data pair 567, 8.5, and a data pair from one of the present experiments, shows that there is nothing at all improbable in what he claimed to have measured. Finally, it was entirely possible to take readings in as little time as Coulomb reported.

In sum, these results all converge to one conclusion: Coulomb obtained his reported numbers from experiment. His results were not unusual, they were almost certainly typical. Therefore, he was justified in his claim that he had experimentally demonstrated what he confidently called the "Fundamental Law of Electricity".

Coulomb's report of June 1785, then, did not describe an idealized account of a plausible experiment, but instead it provides a literal and detailed description of an actual experiment with a representative sample of results that were easy to grasp. If Coulomb's memoir is typical of other experimental reports of the time, then it suggests that experimental investigations, carried out by specialists in the Paris Academy of Sciences, were reported under lucid standards of accuracy.

5. Appendix

My first attempts to assemble and operate a torsion balance built upon various parts that I had at my disposal, thanks to Jed Buchwald. A few years earlier his students at M.I.T. had attempted to replicate Coulomb's experiment for a previous version of his course on historic experiments. So I had the advantage (like Heering at Oldenburg) that the glass cylinders had been manufactured previously. There was also a workable wooden micrometer and paper bands marked with degrees, both prepared by the students at M.I.T. The dimensions of the main components are given in Sect. 4 of the present article. Still, I had to devise several components from scratch.

This Appendix, together with Sect. 4, report *all* the experiments from which numerical data were obtained. I will not detail my initial attempts, as they were grossly unsuccessful. Suffice to say that I first operated with a copper wire about .33 mm thick ($\approx$ 28 AWG) and also with an aluminum alloy wire of .20 mm (32 AWG). I found that both of these wires were just too thick, so that their restorative force was always too great and immediately overwhelmed any electrostatic repulsion between foam balls. Using these wires the foam balls would separate momentarily only while the source of charge remained in contact with the stationary ball. But the balls did not keep sufficient charge on their surfaces to alone maintain any separation. After such initial attempts, I followed Coulomb's prescriptions more closely.

Coulomb's needles consisted of a single filament of silk covered in Spanish wax, or instead a thin reed. Candle wax is not sufficiently rigid for the purpose described, and I did not find genuine Spanish wax in any local stores. However, in a stationery store I did find (synthetic) sealing wax. Still, expecting that this material, if it were to coat only a mere filament of silk, would be insufficiently rigid, I used instead an ordinary sewing thread. I tied small loops at both ends of the thread, to hold and handle it. Next I cut the bottom of an aluminum can and placed it, upside down, on a gas stovetop, and I then placed pieces of sealing wax on it and turned on the fire. Once the wax melted and was bubbling slightly, I then promptly inserted the center portion of the thread into the wax while holding both loops outside on one hand while tapping much of the thread into the wax pool with a thin glass straw with my other hand. I then slid the straw down between the strands of thread to its center to remove some of the wax as I lifted the thread outwards and then stretched it straight. I then hung it from one loop while weighing down the other end with pliers, and left it to dry.

A recurring problem, however, is that the melted sealing wax tends to clump up in spots, so that the resulting sticks were dotted with small globs of wax. Therefore I had to find a way to distribute the wax much more evenly along the thread. I tried several procedures and the following worked best, though I do not recommend it. The idea is to slide a bead smoothly down the string while the wax is still hot, thus removing any excess. So, before tying the loop knot at one end of the string, I there threaded a single metal bead, roughly spherical, having holes all around its surface, keeping it at the end of the string, before the knot. Once the string was covered in wax, I immediately straightened it over a sink (but not too quickly because droplets of hot wax can fly off) and then quickly slid the metal bead all the way down. Whenever I delayed for even a moment, the wax hardened, preventing completion. Extreme care should be exercised when performing any such procedure, given the temperature of the wax. I emphasize this because even though I originally protected my fingertips by covering them in tape, I forgot this step in one instance, in haste, and burned my fingertips badly. At any rate, I thus managed to make a few sufficiently thin and rigid waxed needles.

I purchased by mail two kinds of "pith balls": some covered in graphite, others in aluminum. But when they arrived I found that these balls were not made of pith at all, but of foam, and they had many imperfections. Finally I found a company (Fisher Scientific) that assured me that what they sell are actual pith balls, so again, I requested some covered in aluminum, some painted with colors, and some bare. Yet when the order arrived, they were not balls at all, but were in the shape of little cylinders, like plugs. I was then told over the phone that the problem is that this material is very difficult to shape into a spherical form, because it is fibrous. Still, I proceeded to try to do just that by using a cardstock nail-file. After an hour of careful work I found that, actually, one can make a relatively smooth surface, and thus began to make the small balls. One reason why Coulomb used pith was evident: it is a very light material. Yet Faraday cautioned that if the balls are made of bare pith they are rather useless, because they are poor conductors. I readily found that the bare pith surface, though polished, retains irregularities and tiny fibers that likely would hold charge unevenly, or let it leak to the air. Faraday recommended that the balls be gilded. I did not know how to obtain or apply actual gold leaf, nor much less apply it to a tiny curved pith surface, so instead, I prepared a pair of pith balls that I simply covered with two layers of silver leafing paint. Also, knowing that India ink is a pretty good conductor in simple electrostatic experiments, I prepared another pair or pith balls painted with India ink, noticing that this ink seeped quickly into the pith whereas the silver paint remained more on the surface. Furthermore, I prepared also a pair of bare pith balls, since Coulomb did not specify covering them with any substance.

Once I had these pith balls with these three surfaces (7.5 to 9 mm in diameter), plus the larger foam balls (12 mm diameter), I proceed to ascertain which ones would be most readily affected by an electrostatic charge. I successively placed on a wooden table one ball of one kind and a different ball some two inches apart. I then rubbed a thick bundle of cat hair on an ebonite rod, and then slowly moved the rod down towards the midway point between the two different balls in question, to see which one moved more quickly towards the rod. Fortunately, it was a very dry day in Pasadena, so not only did the balls roll towards the rod, some of them jumped up and down repeatedly against the rod, and were even stuck to its underside, or rolled along it. In the end, the silvered pith balls were most dramatically affected, followed by the balls painted with India ink, then the bare pith balls, next the graphite covered foam balls, and lastly the aluminum covered foam balls. To maximize the possible effect in using any such balls in my replication attempts, I decided to use mainly the silvered balls.

Next, not having a silver wire .035 mm in diameter as described by Coulomb, I used instead a thicker aluminum wire of about .13 mm ($\approx$ 36 AWG), obtained from a telephone cable. Unfortunately, this wire was not at all straight, so I had to devise a way to straighten it. A firm, quick yank will straighten some wires, but that did not work for this one. Still, I found that by hanging the wire vertically and inserting it into a small tight fracture in a flat bit of wood (about 4 mm thick), one can straighten it by then evenly running this wooden tablet repeatedly up and down along the wire's length, while holding down its bottom extremity.

To attach the wire to the underside of the micrometer, Coulomb recommended a sort of pencil holder so small that the thin wire would be inserted into its opening and then clamped there by sliding a metal ringlet to tighten it. Analogously, I used the gripping part of a modern-day mechanical pencil, but the wire was much too thin to be grasped by it, so after insertion through its center I bent the wire at the top and tied it in place (taking 4 cm of wire).

To attach the wire to the waxed needle, Coulomb used a copper cylinder, flattened on top, where he pierced its flat surface making a hole (for insertion of the needle). He added a collar to tighten the top of the piece to clamp the wire. This cylindrical pivot then hung vertically. I did not find appropriate pieces of metal to copy his arrangement, so I instead decided to use a metal clasp (purchased at a bead store), consisting of two cylindrical pieces that screw into one another. I removed little hooks at either end, and expanded the holes there to make them big enough for inserting the waxed needle. This cylindrical pivot (1 cm long) would hang horizontally, and would clamp the wire by placing the wire at its center, right between the two pieces that screw together tightly (thus leaving 74 cm of wire exposed).

Depending on the parts of the apparatus and minor details of their fitting, the hanging wire will not be centered on the bottom cylinder. To correct this problem, Heering used a base with three screws that he could adjust to slightly tilt the entire apparatus, in order to bring the wire into the center of the bottom cylinder. This approach was used by some physicists, such as Faraday, and seems to have been common in torsion balances sold as demonstration devices in the 1800s. But since Coulomb did not describe a base with screws, I chose not to employ one. Besides, tilting the apparatus creates a corresponding slight bend in the wire at its uppermost joint which can otherwise be avoided. So, to center the wire, I adjusted the position of the top plate (supporting the glass tube) while checking with a right-angled ruler that the wire aligned visibly with markings on opposite sides of the glass cylinder. To further aid in the process of aligning the wire and other parts of the assembly, I also used a sheet of thin cardstock paper, marked with reference lines, which I placed under the glass cylinder.

To sustain the stationary ball vertically, I used a plastic stem made from a coat hanger, and attached a thin sliver of wood near its bottom, horizontally, so that the pith ball would sit sideways and outwards from the bottom of the stem, preventing the ball from falling off. (Coulomb had not specified how to attach the stationary ball to the vertical stem; he mentioned only that the lower part of the stem was made of gum-lac.)

Through all the experiments, the apparatus sat on a flat wood board atop a wooden desk, which in turn rested on the carpeted floor of my office. I found that it was useful to keep the apparatus near the edge of the desk, so that whenever I needed to adjust something inside, I could carefully slide the glass cylinder beyond its base to overhang the desk by about a third of its diameter so that I could reach into the cylinder with minimal disturbance.

Before operating the device by using and potentially damaging the silvered balls (9 mm diameters), I decided to first carry out a few trials with the India ink pair (7.5 mm diameters). I made one small hole on each ball with the tip of a thumbtack. I inserted into one ball the small sliver of wood, which in turn fit into the bottom of the vertical stem, such that the ball and sliver sat perpendicular to it. I inserted the waxed needle (20.5 cm) into the second ball, and on its other end I attached a piece of Scotch tape folded onto itself and cut into a circular shape of just the right size (1.3 cm) to serve as a counterweight against the pith ball. I then removed this ball, and proceeded to carefully insert the waxed needle through the hanging metal clasp, and then reinsert its free end into the ball.

Once the needle was centered with the movable ball at the 0° position, I next displaced the movable ball by placing the stationary ball at the 0° position. By rubbing an ebonite rod on a bundle of cat hair and inserting this very rod to touch the stationary ball, the two balls acquired charge. For each torsion of the micrometer, the resulting separation between the centers of the two balls is stated in the tables below.

May 30									exponent	deviation
micrometer	0	40	63						1.650	0.820
separation	18	10	8							
micrometer	0	10	20	30	40	50	60	70	1.591	0.860
separation	20	18	>15	12	11	>10	7.5	7		

Despite the positive results, the waxed needle was increasingly sagging under its own weight plus that of the objects at its extremities. I therefore tried to devise a more rigid alternative. First I tried to use another sealing wax that seemed slightly more rigid. Next I tried a blend, and next I tried to cook that wax long enough to make it harder, burnt. Since none of these attempts produced sufficiently good results, I then tried to melt sealing wax with plastic, and afterwards to just melt plastic alone. Yet the temperature required to melt the plastic was too high for the sewing thread, the latter just disintegrated every time when it came into contact with the plastic. Yet at one point, I was melting plastic (from the handle of a blue Gillete shaving razor) and it stuck to the thin glass straw with which I stirred it, such that as I pulled out the straw the plastic stretched upwards in many long strands that immediately dried in such shapes. By repeating this procedure several times, I made a few strands that were sufficiently thin and straight enough to serve as needles for the experiment. A resulting plastic blue needle was thinner, lighter, and yet more rigid (holding up its own weight when held at one end) than the green waxed string that I had previously used.

Using this plastic needle, with the pith balls that were painted silver (9 mm diameters), I carried out the following series of experiments. Again I used the telephone wire, the same metal clasp, and a circular counterweight made of tape. To dehydrate the air in the cylinder, I used a bright lamp shining for two hours on the semi-open top.

May 31 3:15PM									exponent	deviation		
micrometer	0	71.75							1.647	0.000		
separation	20.5	9										
May 31 3:30PM									exponent	deviation		
micrometer	0	10	20	30	40	50	60	70	1.542	0.983		
separation	19	16	14	12	11	9.8	8.75	8				
May 31 3:50PM												
micrometer	0	5	10	15	20	25	30	35	40	45	50	55
separation	17.4	15.8	15	13.5	12.7	11.7	11	10.2	9.7	9.3	8.7	7.8
									exponent	deviation		
continued:	60	65	70						1.697	0.777	0	
	7.6	7	6.6								17	
May 31 4:30PM									exponent	deviation		
micrometer	0	10	20	30	40	touch*	45		1.331	0.283	0	
separation	18.7	15.2	13	11.2	9.5		8.7				17.9	

*Note: wherever the pith balls touched (stuck together) the reading is not used to calculate the average exponent.

For the last experiment in this series the entire assembly had to be disturbed to adjust and rebalance the hanging pith ball, which had been accidentally moved. Moisture in my hands and the incidental effect of exchanging bodily charge with the glass and other parts clearly affected the results.

Experimental work was interrupted, until three weeks later, when I carried out more attempts. On June 22, I painstakingly aligned and calibrated the apparatus, and then used the lamp to dehydrate the air for three hours. I then tried ten times to impart charge to the system, but the largest separation was only about 10° so I did not carry out measurements. The next day the air and the materials were still humid, apparently, so it took many attempts to place a significant charge on the pith balls. Again, I used the lamp to try to dehydrate the air.

June 23 3:35PM										exponent	deviation	
micrometer	0	10	20	30	40					1.217	0.275	0
separation	17	13.6	11.3	8.9	7.3							16.3
June 23												
micrometer	0	10	20	30	40	50	60	70	80	90	100	110
separation	21.4	18.5	16.5	14	12.8	11.3	10.5	9.5	8.8	7.8	7	6.7
										exponent	deviation	8.38
										1.509	0.485	0
												20.5
June 23										exponent	deviation	
micrometer	0	10	20	30	40	50	60	70		1.324	0.603	0
separation	18.5	15.3	12.8	10.4	9.6	8.4	7	5.8				17.4

For the following experiments I did not use the lamp or anything to dehydrate the system.

June 24									exponent	deviation	
micrometer	0	10	20	30					1.561	0.337	0
separation	14.4	10.5	8.8	7.5							14
micrometer	0	5	10	15	20	25	30	35	1.482	0.321	0
separation	14.5	12.5	11.4	10.2	9.3	8.3	7.6	7			14.4

The next day it was even more difficult to add any charge into the system. In such cases I rubbed the ebonite rod many times with the bundle of cat hair. All the while I sought in vain to obtain a separation of more than 20 degrees. Coulomb had reported an initial separation of 36°, yet I could not obtain anything comparable.

June 25											exponent	deviation	8:38
micrometer	0	10	20	30	40	50	60	70	80	90	1.287	0.445	0
separation	18.3	14.8	13	11	9.8	8.3	7.5	6.7	5.7	5			17.7
June 25 this experiment began from the preceding charge											exponent	deviation	
micrometer	0	5	15	25	35	45	55				1.338	0.510	
separation	17	14.8	12.4	9.5	8.5	7.5	6						

For the following, it was again extremely difficult to place a significant charge on the pith balls, and in this particular case I touched the ebonite rod to the stationary ball twice.

June 28 11:40PM								exponent	deviation
micrometer	0	5	10	15	20	25	30	1.431	0.601
separation	13.5	11.3	9.9	9	8	7.5	6		

During all these experiments, I became increasingly aware that the likeliest way to obtain a significant separation between the pith balls would be to use a thinner wire.

Fortunately, I had obtained a spool of copper wire of .030 mm in diameter (48 AWG), visibly thinner than a human hair, and extremely delicate. After threading it (75 cm exposed length) into the system for the first time, I immediately carried out the following experiment.

June 29												exponent	deviation	8:38
micrometer	0	10	20	30	40	50	60	70	touch	80	touch	1.356	0.248	0
separation	24.5	21.1	18.3	16.3	14.5	13.2	11.5	10.5		9.5				17

Finally here was a separation of more than 20 degrees. Only afterwards, did I proceed to carefully align every part of the system, calibrate the correspondence of the movable ball, as moved by the micrometer, to the scale on the lower cylinder. For a sample of six sequential readings, the position of the ball on the lower scale approximated the indication on the micrometer by .52 degrees, which was good for carrying out experiments, such as the following.

June 29 11:05PM					exponent	deviation	9 min.
micrometer	0	89	89	50	1.135	0.590	0
separation	25.5	touch	7.5	11			22.5

At the start, once the balls separated, the movable ball returned and stuck to the stationary ball for 3 seconds before retreating again. I had not encountered this behavior before, and hence I next mark it in the few following instances in which it occurred. By contrast, Heering encountered this odd behavior regularly, and with the contact duration extending to about 30 seconds before the balls repelled.

For the following runs with a 48 gauge copper wire, I again used the lamp to dehydrate the system, by shining it into the large cylinder for an hour. Then the system was left to settle for three hours, although subsequently I manually re-aligned every part.

July 2 1:35AM at the start the balls came back together for 4 seconds							exponent	deviation	9 min.
micrometer	0	108	55				1.727	0.106	0
separation	31.5	13.8	18.2						29
July 2 1:45AM this experiment began from the preceding charge							exponent	deviation	10 min.
micrometer	0	70	150	210	230	250	1.325	0.323	0
separation	29	15.4	9.8	7	touch	5.7			26.5
July 2 1:56AM this experiment began from the preceding charge							exponent	deviation	5 min.
micrometer	0	50	90	140			1.429	0.179	0
separation	26	15	10.5	8					24.3

The next day was very sunny and the humidity seemed low. I did not use the lamp. Slight adjustments sufficed to realign the parts of the system. The initial separation between the pith balls was now immense, unprecedented in my experience. The thin wire made all the difference. Also, the separation at any given torsion was remarkably stable.

July 2 4:51PM								exponent	deviation	10 min.
micrometer	0	180	270	360	450	540		1.460	0.191	0
separation	76	36	29.5	25.6	22	19.5				62
July 2 5:10PM this experiment began from the preceding charge								exponent	deviation	6 min.
micrometer	0	45	90	135	180	225	270	1.723	0.150	0
separation	62	50.5	42.5	36.3	32	28.7	26.2			57
July 2 5:35PM this experiment began from the preceding charge								exponent	deviation	2:30 min.
micrometer	0	205	400	540				1.846	0.257	0
separation	57	29	21.7	18						56

For the latter of these experiments, I used a long plastic straw to bring together the pith balls to force them to share and redistribute their charge more evenly. The exponent then turned out to be higher than ever before.

It now seemed that perhaps by making minor improvements in the parts, one might be able to obtain higher exponents. Thus, for the following experiments, I glued the hanging pivot clasp to the needle, for the first time, by putting a small dab of Elmer's glue on the joint.

In several of the following experiments the angle of twist of the micrometer was obtained by multiplying the initial angle of separation times 3.5, as Coulomb seems to have done.

July 27 10:40PM					exponent	deviation	10 min.
micrometer	0	159.25	360...	626.75	1.556	0.130	0
separation	45.5	20.5	touch	8.5			45
July 27 10:52PM this experiment began from the preceding charge					exponent	deviation	7 min.
micrometer	0	154	606		1.451	0.259	0
separation	44	20	7				44

Following the latter experiment, I removed the stationary ball to check the position to which the movable ball freely returned. It settled not at 0 but at 4.8, owing evidently to the high total torsion to which this wire had been subjected.

Next, I used another new strand of copper wire. Considering that the uppermost part of each wire always had been *tied* to the top assembly, I proceeded to devise a way to clamp it instead. Since the lower pivot clamped the wire well, I attached a similar metallic clasp directly onto the bottom of the micrometer assembly, and thus I clamped the wire onto it. Given this alteration, plus all the prior ones, I obtained the following results.

July 30 6:00PM at the start the balls stuck together for 12 seconds					exponent	deviation	8:30 min.
micrometer	0	199.5	897.75		1.758	0.365	
separation	57	30	11.3				
July 30 6:50PM					exponent	deviation	
micrometer	0	138.25	614.25	569	1.791	0.000	
separation	39.5	18.5	5.5 touch				
July 30					exponent	deviation	
micrometer	0	136.5			1.899	0.000	
separation	39	19					

July 30						exponent	deviation	
micrometer	0	140	630	540	360	1.295	0.055	0
separation	40	15	5.5 touch		7.5			34

Meanwhile, for months I had found no company that would sell or custom-manufacture round silver wire of .035 mm in average diameter. One manufacturer said that it would be nearly impossible to make silver wire that thin in useful lengths. But finally I found a company, SurePure Chemetals, that sold thin silver wire. Rather than request a special order, which would have been too expensive, I gladly settled for an available list product: 100 ft of round wire, 99.99% pure silver, of 44 AWG, that is, having an average diameter of .05 mm.

I expected that my wire would be better than Coulomb's, perhaps in purity or in keeping a constant average diameter throughout its length. It is slightly thicker, but Coulomb recommended that the wire should preferably be even twice as thick as his original wire, so the slight difference in thickness is in the sense that he advised. Once set, the exposed length of my silver wire was 75 cm. Aside from the silver wire, all other parts of the system were still as before. Once all parts had been aligned and calibrated, I tried to impart charge onto the pith balls, but four attempts all produced initial separations of less than 10 degrees. Then I obtained the following:

July 31 5:45PM				exponent	deviation
micrometer	0	91	409.5	1.188	0.000
separation	26	8.5	5.5 touch		

Six attempts followed, but I did not obtain separations of more than 15 degrees. Yet the next day I obtained the following results.

August 1 7:48PM							exponent	deviation	8:30 min.
micrometer	0	133	360	460	598.5		1.483	0.281	0
separation	38	17.5	10.5	8.5	7				38
August 1 7:55PM this experiment began from the preceding charge							exponent	deviation	8 min.
micrometer	0	60	120	180	240	300	1.503	0.323	
separation	38	21	15	12.6	11	8.5			

August 1 8:05PM this experiment began from the preceding charge						exponent	deviation	5:45 min.
micrometer	0	101.5	456.75	300	200	1.426	0.000	0
separation	29	11.3	balls touch (5.5)					27

In this last experiment I tried again Coulomb's implicit procedure, by multiplying the initial separation of 29 by 3.5 to obtain the following 101.5, and multiplying the initial separation by 15.75 to obtain 456.75. But at that high torsion, and less, the balls came together. After this last experiment, I removed the stationary ball and found that the movable ball returned not to its original zero position but to 8 degrees at 8:15PM; remaining there at 8:30; by 8:37 it was at 7. Thus, the extreme torsion to which the silver wire was subjected (more than 600 degrees of total torsion) had affected it. Again, I used the same silver wire to test the effects of this damage, and obtained:

August 1 9:00PM at the start, the movable ball swung back, touched the other					exponent	deviation	7:30 min.
micrometer	0	78.5	78.5	150	1.134	0.000	0
separation	22.5	7	balls touch (5.5)				18.5

The next day, despite the likelihood that this first silver wire had been damaged by the high torsion, it had returned at least to the zero position. I proceeded to perform more trials with it:

August 2 7:03PM								exponent	deviation	8 min.
micrometer	0	60	120	180	240	300	360	1.525	0.186	
separation	55	37.5	29	24	21	18	16			
August 2 7:20PM this experiment began from the preceding charge								exponent	deviation	8:30 min.
micrometer	0	90	180	270	360	460		1.483	0.399	0
separation	42	24	17.5	14.5	11	9				38.3
August 2 7:35PM this experiment began from the preceding charge								exponent	deviation	5:30 min.
micrometer	0	122.5	360					1.503	0.146	0
separation	35	15.4	7.5							32
August 2 8:08PM								exponent	deviation	9 min.
micrometer	0	60	120	180	240	300		1.341	0.190	0
separation	60	39	29	23.8	20.3	17.4				48
August 2 8:30PM this experiment began from the preceding charge								exponent	deviation	< 7 min.
micrometer	0	90	180	270				1.472	0.038	0
separation	39	20	13.5	10.5						35.8

For the last experiment in this set, the balls were initially forced together by using a plastic straw to push the needle. The redistribution of charge had no evident effect on the result.

A few days later, I used the same setup and wire. I obtained the following results:

August 5 1:00AM							exponent	deviation	7 min.
micrometer	0	122.5	270	360			1.408	0.438	0
separation	35	16.8	10.7	7.7					31
August 8 ∼7:35PM							exponent	deviation	9 min.
micrometer	0	90	180	270	360	450	1.243	0.315	0
separation	95	62	47	39	33	28.5			68
August 8 7:55PM this experiment began from the preceding charge							exponent	deviation	
micrometer	0	180	360				1.769	0.151	
separation	57	29.5	20.5						

Again, for the last of these experiments, I used a plastic straw to force the balls to share charge, and this time it seemed to produce a high exponent. (Coulomb described no such procedure.) But perhaps more importantly, I only made three readings in that attempt, lowering the effect of charge loss on the average exponent. I continued to use the same wire and setup with minor adjustments to realign the parts. Following the initial separation, the balls now tended to stick together. The movable ball also drifted. I obtained:

August 9 3:40PM at the start the balls stuck together for 12 seconds				exponent	deviation	8 min.
micrometer	0	50	100	1.616	0.134	0
separation	25.5	15	10.5			26.5
August 9 4:10PM				exponent	deviation	8 min.
micrometer	0	180	360	1.648	0.005	0
separation	82	47	34			74
August 9 4:26PM this experiment began from the preceding charge				exponent	deviation	6 min.
micrometer	0	180	360	1.738	0.089	0
separation	64	34	24			58
August 9 5:00PM this experiment began from the preceding charge				exponent	deviation	6 min.
micrometer	0	144	400	1.794	0.103	0
separation	36	16.5	9.5			33.5
August 9 8:40PM at the start the movable ball returned and touched the other				exponent	deviation	6 min.
micrometer	0	180	360	1.842	0.104	0
separation	54	27	19.5			52.5
August 9 9:00PM at the start the ball again again returned and touched the other				exponent	deviation	7 min.
micrometer	0	180	360	1.555	0.143	0
separation	53	24.5	16			49

In view of the relatively high results, I next used a Faraday cage placed around the lower cylinder to see whether that would produce a higher exponent. I obtained the following result.

August 9				exponent	deviation	7 min.
micrometer	0	180	360	1.631	0.180	0
separation	57	28.5	19			51

The system behaved as usual, and the average exponent was not significantly higher than in the prior attempts. I had obtained similar negative effects with the Faraday cage on qualitative attempts at other occasions, hence I continued to operate without it. Subsequently (that same night), I obtained the following results, which also fit the series.

August 9 inverted order				exponent	deviation	7 min.
micrometer	60	0	270	1.692	0.345	0
separation	31	44	13.5			42
August 9 10:19PM				exponent	deviation	7:40 min.
micrometer	0	180	360	1.711	0.139	0
separation	73	41.5	29.3			67

After all these nine experiments on the same day, I finished by removing the stationary ball to see where the movable ball would settle. Remarkably, it returned to 0, where I then recorded it to rest for 9 minutes. Half an hour later it was at 1 degree.

Since I had obtained data that yielded exponents as high as 1.84, I figured that by improving all parts of the experimental arrangement one might be able to obtain results as high as Coulomb's 1.911. Perhaps the main defects in my apparatus, I suspected, were the pith balls, being not sufficiently spherical and given the particular silver paint covering them. Since Faraday reported best results by gilding the balls, I now tried to do so. After becoming superficially acquainted with various forms of the process, and given my limited resources and time, I managed to produce at least a seemingly satisfactory result as follows. I first shaped the pith balls as before, and smoothed (compressed) their surface with my fingernail. Having mounted each ball on the tips of sewing needles standing on cork, I then applied two thin layers of synthetic shellac, left to dry, followed by more careful smoothing, followed by a thin layer of gesso, followed by further smoothing, and then I applied standard adhesive "size" followed after a while by attaching the (imitation) gold leaf. After carefully burnishing off the extra bits of leaf, I then added another coat of size followed by another layer of the metal leaf. After burnishing again, these golden pith balls looked better than the prior painted ones.

At any rate, in his memoir of 1785, Coulomb specified that he used a gilded pith ball, but *only* in a separate kind of experiment. Otherwise, the two pith balls that he used to measure repulsions were apparently neither gilded nor covered with any substance. Therefore, I proceeded to prepare also a pair of bare pith balls. Coulomb described them as being 2 to 3 lines in diameter, that is, 4.5 to 6.8 mm; I therefore decided to make them 6 mm in diameter. By this point I understood that pith is actually a very good material to work with. It can be gently filed down, like balsa wood, but moreover, it can be shaped by pressure, like clay. Having now only three pieces of bare pith left, and knowing that some of the other parts of my setup would still not be exactly like Coulomb's, I invested extra effort to make the pair of pith balls as spherical as I could, spending two days of work on that alone. I again used the nail file, but afterwards I also smoothed them down by pressing all around their surface with the topside of my fingernail, until the surfaces were smooth and the diameters were 6 mm. Still, it seemed unlikely, as Faraday warned, that bare pith would be an effective bearer and conductor of charge.

To attach the stationary pith ball to its supporting stem (23.3 cm long) I changed my earlier setup. Instead of using a small wooden sliver to connect the ball to the stem, I now carved a small plastic point extending sideward (6 mm) from the bottom tip of the plastic stem. Also, in all previous experiments, I had attached with Scotch tape the vertical stem to a wooden block sitting atop the glass lid. But now, I replaced the fixed taped approach with Coulomb's illustrated approach: I made a suitably narrow wedge in a slender block of wood (19.3 cm long), such that the vertical stem would be inserted there and held tightly, without any adhesive. This approach has the advantage of facilitating adjustments in the height of the stationary pith ball to match that of the movable ball.

Next, to attach the silver wire to the underside of the micrometer, I repeat, Coulomb had recommended a sort of *porte-crayon* so small that the wire would be inserted into its opening and then clamped there by sliding a metal ringlet to tighten it. Accordingly, I now tried to devise a similar way of clamping the wire in place. This time I worked by laying the stretched wire flat on a table, taped in place, which helped to prevent the accidental twisting and bending that were harder to avoid when I had worked on wire hanging vertically. Although the wire was too thin to be grasped by the mechanical pencil holder, I used a small metal cylinder, cut down to an appropriate size, and then clamped it onto one freed end of the wire by pressing it with pliers until it flattened, and then I carefully inserted this rectangular sliver into the slit on the gripping tip of the mechanical pencil. Thus I firmly attached the wire to the micrometer assembly, aligned carefully at its center.

Next, I obtained a new bar of crystalline sealing wax (more rigid than the kind I had earlier used) and made another "waxed" thread needle. To attach the silver wire to this waxed needle, I now imitated Coulomb's design: to make a metallic cylinder that would hang vertically (rather than horizontally) and be flattened on top to clamp the wire, while being pierced by a hole for the waxed needle to be inserted. I used a small "expansion cylinder," manufactured for use in necklaces of beads, and flattened it on top by pliers, which exposed the seam which I then cracked open with a knife, for subsequent insertion of the wire. Plus, I pierced it to make a hole of about 2 mm in diameter, and I added a collar, as in Coulomb's, and inserted a compressed cylinder at its bottom to add weight. The pivot's length (height) was 2.1 cm. After inserting about 3 mm of wire on the top, I clamped it tightly. Then I lifted carefully the micrometer knob, wire, and hanging pivot and inserted them into the hole on the micrometer and down into the glass cylinders. Finally, by sliding the entire system to overhang from the tabletop, I then put my forearms inside to thread the waxed needle through the hanging pivot, and then capped the needle with the pith ball. Finding next its balance point, I added a dab of Elmer's glue onto the junction to affix the pivot to the needle.

Furthermore, I decided to replace the paper scales on the micrometer and the large cylinder. I knew that these old scales had various imperfections since they were made by students at M.I.T. without utmost care. I considered making a standard length segment that I could repeatedly transpose along a long strip of paper to make the markings. But this would introduce slight cumulative errors in the joints between successive intervals. Instead, I found that the best way to manually make these markings was to *calculate* the exact distance from the zero point to *each* marking, and then use a ruler to pinpoint each location independently and mark it. The results were clearly better than the scales I had used in all earlier experiments, which now exhibited deviations as high as 2 degrees in spots. I finished the paper bands with a covering of shellac. I attached the large one to the glass cylinder, and encircled the micrometer with the small strip snugly but without any adhesive, so that one may reorient it (for ease in changing the zero position of the micrometer without having to move the glass tube or the micrometer assembly).

Finally, once again, I carefully centered the wire and aligned every part of the system. Since the components closely matched Coulomb's prescriptions, I expected that the next series of experiments would test well the likelihood of Coulomb's reported results. But I obtained the following results.

August 22 3:45PM					exponent	deviation	
micrometer	0	45	90		1.113	0.165	0
separation	31	18	11				31
August 22 4:05PM					exponent	deviation	
micrometer	0	45	90		0.891	0.050	
separation	44	26	17				
August 22					exponent	deviation	
micrometer	0	180			1.032	0.000	0
separation	36	7.5					29.5
August 22					exponent	deviation	
micrometer	0	90	180	180	1.051	0.373	0
separation	36	17	touch	7.5			28
August 22					exponent	deviation	
micrometer	0	45	90		0.932	0.157	0
separation	32.5	18	10				28
August 22					exponent	deviation	4 min.
micrometer	0	90	180	270	0.937	0.177	
separation	42	18.5	9	touch			

In all of these experiments, and a few more, the movable ball behaved in an erratic and relatively unprecedented way. These results were incomparably worse than anything I had obtained previously. Paradoxically, now the entire setup seemed to most closely resemble Coulomb's.

At first it seemed plausible that a combination of factors, maybe including humidity, but especially the surface of the bare pith balls, might account for why these experiments gave such peculiar results. However, I tried to find how the movable ball, under repulsion, behaved when I moved the stationary ball. By so doing, I found that the waxed needle itself accelerated towards the stationary ball when the two were close. Thus its particular sealing wax was not electrically neutral, and was interfering in the repulsion of the pith balls. There was, after all, one significant difference between my setup and Coulomb's prescriptions: Coulomb had instructed that the extremity of the waxed needle be covered by insulating gum-lac, and I had put no such covering on my needle. Lacking a proper varnish, I next substituted the waxed needle with the earlier plastic needle that I had used in most previous experiments. The results of the consequent series of experiments are reported in Sect. 4 of the present article.

Lastly, I had a very brief opportunity to experiment with the gilded pith balls that I had prepared. To that end, I now covered the problematic waxed needle with two layers of synthetic shellac, not knowing whether it would serve as proper insulation. The results were the following.

August 27 1:15AM						exponent	deviation	
micrometer	0	54.25				n/a	n/a	
separation	15.5	touch						
August 27 3:05AM						exponent	deviation	
micrometer	0	90	90	0	30	n/a	n/a	0
separation	32	touch	13	20.5	11.5			19
August 27 3:30AM						exponent	deviation	
micrometer	0	120	240			1.184	0.223	0
separation	37	15	8					~ 30
August 27 3:50AM						exponent	deviation	
micrometer	0	30	60	90		1.528	0.067	0
separation	29	20	15.5	12.7				28

Again the needle behaved in a highly erratic way, as prior to the coating of shellac. Evidently the coating did not serve to insulate the needle from interacting with the pith balls. The gilded pith balls fared no better in this series than the bare pith balls. Regardless, the revealing feature of these unfortunate results is how greatly they differ from the intervening set of experiments, considering that the mere decisive factor was just the composition (or electrical responsiveness) of the needle. Thus we see how just a single, slight, and easily invisible difference between Coulomb's apt prescriptions and a replication may lead to entirely different results.

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