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On Coulomb's inverse square law

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The famous inverse square law in electrostatics, first published in 1785 by C. A. Coulomb, was strongly contested during the next 40 years, especially in Germany. Therefore, at the Carl von Ossietzky University Oldenburg a replication of the apparatus described by Coulomb was made, and the reasons were investigated why none of his contemporaries succeeded in reproducing his results. In addition the respective theoretical concepts of Coulomb, his supporters, and his opponents were analyzed.

I. INTRODUCTION

In 1785 Charles Augustin Coulomb published the first and in 1787 the second of his seven memoirs on electricity and magnetism. In these two articles he formulated the main parts of the famous law, which today is known as the "Fundamental Law of Electrostatics." In the following years the inverse square law was strongly disputed, especially in Germany. This reaction is the more astonishing as Coulomb's relation seems to be very plausible because of its analogy to Newton's inverse square law of gravitation. It seems as if the results of Coulomb's experiments were not convincing and could hardly be reproduced; therefore, the apparatus described in Coulomb's memoirs was reconstructed at the University of Oldenburg and the experiments Coulomb claimed to have executed were repeated. The following article presents the results of the replicated experiments, an historical discussion about Coulomb's results, and the theoretical background of this discussion.

The experimental setups used by Coulomb will be described in Secs. II and III. In Sec. IV the results of the replication of Coulomb's apparatus will be discussed. During the experiments with the torsion balance several possible errors occurred from which Coulomb's results may have suffered. Their importance for and their influence on the data published by Coulomb will be discussed in Sec. V; and the conclusions that can be drawn from that discussion are given in Sec. VI. An historical discussion of Coulomb's relation will be reviewed in Sec. VII and the theoretical background of this discussion is examined in Sec. VIII.

II. COULOMB'S EXPERIMENTS: THE TORSION BALANCE EXPERIMENT

The first of the two apparatus described by Coulomb is the famous torsion balance [Fig. 1(a)] whose "diagram must be produced more often than any other diagram."[1] A glass plate placed on a glass cylinder (32 cm in diameter and 32 cm high) is pierced with two holes of 4.5 cm in diameter, one of them above which a glass tube of 65 cm height is placed is in the center of the plate. At the upper end of the tube a torsion micrometer [Fig 1(b)] is fixed. At the lower end of the micrometer a silver wire is clamped, 76 cm long and 0.04 mm in diameter (as calculated from the weight of the wire). At the end of the wire a cylinder of copper or iron with a diameter of not more than 0.2 cm is clamped [Fig. 1(c)]. A hole is bored through the cylinder so that a needle can be inserted. This needle is made either of a silk thread soaked in Spanish wax or of a straw likewise soaked in Spanish wax and finished off by a cylindrical rod of shellac. One end of the needle carries a pith ball 0.5 or 0.7 cm in diameter, the other end a little piece of paper fastened vertically that serves as a counterweight and at the same time damps the oscillations. In the second hole of the plate a small cylinder is inserted, the lower part of which is made of shellac. At the end of this cylinder a second pith ball is fixed having the same diameter as the first one. This ball is placed in the position the movable ball occupies when the wire is untwisted. A strip of paper divided into 360° is pasted around the glass cylinder at the height of the needle.[2]

In order to understand the theoretical background of the torsion balance a few words on its physical principles are necessary. During the experiment two forces cancel each other out: the electrostatic repulsion and the force caused by the torsion of the silver wire. The second force is, up to a definite degree of torsion, proportional to the angle of displacement. Because of this relation (which was formulated correctly for the first time in 1784 by Coulomb) it is possible to determine the relation between the electrostatic force and the distance separating the charges.

At the beginning of the experiment the fixed ball is charged through contact with a charged conductor which is immediately removed. As the balls come into contact, the movable ball is charged likewise and repelled. The position of the movable ball is read as soon as the oscillation stops. Then the torsion of the wire is increased by the micrometer in order to draw the balls nearer to each other and, after the end of the oscillations, the position of the movable ball is read again. In his memoir, Coulomb gave the following results:

> First Trial. Having electrified the two balls by means of the pin head while the index of the micrometer points to 0, the ball *a* of the needle is separated from the ball *t* by 36 degrees.
> Second Trial. By twisting the suspension wire through 126 degrees as shown by the pointer o of the micrometer, the two balls approached each other and stand 18 degrees apart.
> Third Trial. By twisting the suspension wire through 567 degrees the two balls approached to a distance of 8 degrees and a half.[3]

These data were the only ones Coulomb ever gave to prove the inverse square law in the repulsive case.

III. COULOMB'S SECOND EXPERIMENT (ATTRACTIVE CASE)

In the case of unlike charges measurement with the torsion balance is nearly impossible because the torsion force is directly proportional to the angle of displacement, but

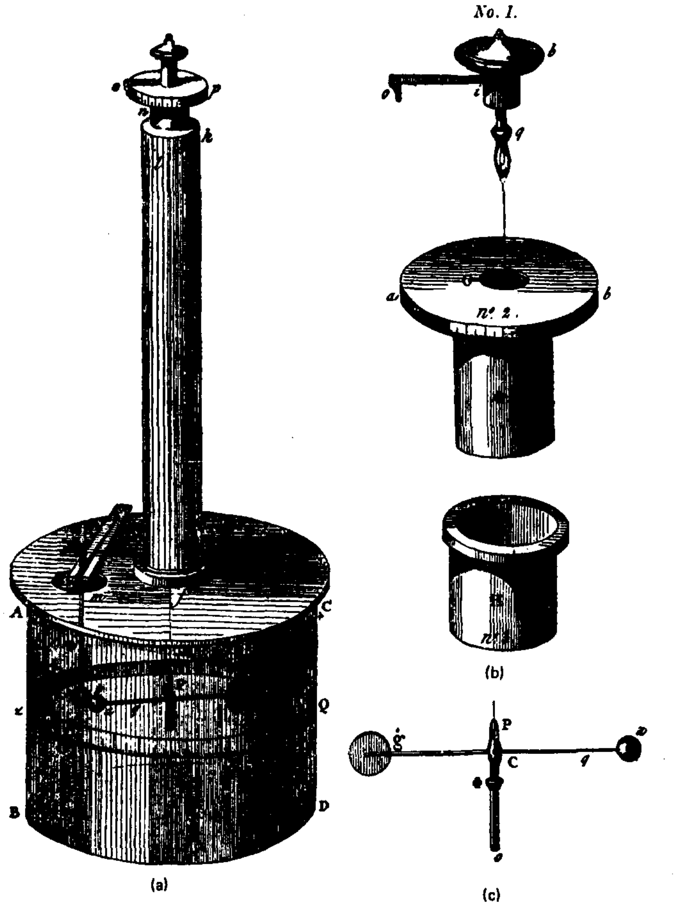

Fig. 1. (a)–(c) Diagram of the torsion balance.

the electrostatic attraction is (inversely) proportional to the square of the distance. This means that after passing over the equilibrium point, the attractive force increases far more than the repulsive (torsion) force. This leads to a contact of the balls and consequently to an equalization of the charges. Therefore, Coulomb designed another experimental setup to determine the relation in the case of unlike charges.

Coulomb describes his apparatus (Fig. 2) as follows. A needle of shellac 3.4 cm long was suspended by a silk thread 1.8 cm long which was taken from a single fiber of a cocoon. This fact seems to be very important, because it is mentioned several times by Coulomb in his memoir. The thread was attached to a little, dried rod coated with shellac or Spanish wax. This rod was part of a wooden rack, which made it possible to change the position of the needle vertically as well as horizontally. At one end of the needle a disc cut from a gilt sheet was fixed perpendicularly. It had a diameter of 1.6 cm. A definite distance away from the disc a globe with a diameter of 32.5 cm was situated, which was made of copper or of cardboard with tinfoil. It was carried by four uprights coated with Spanish wax and terminated, for better isolation, by four rods of Spanish wax. The lower ends of these uprights were set in a base placed on a little movable table so the table could be brought in the position that was most convenient for the experiment.[4]

It has to be mentioned that Coulomb gave in his memoir two sets of dimensions, one in the description of the apparatus, which does not include all the necessary information to rebuild the apparatus. The other set, which is complete, is given in the description of the experiment. This set was used to rebuild the apparatus and is therefore cited in this article.

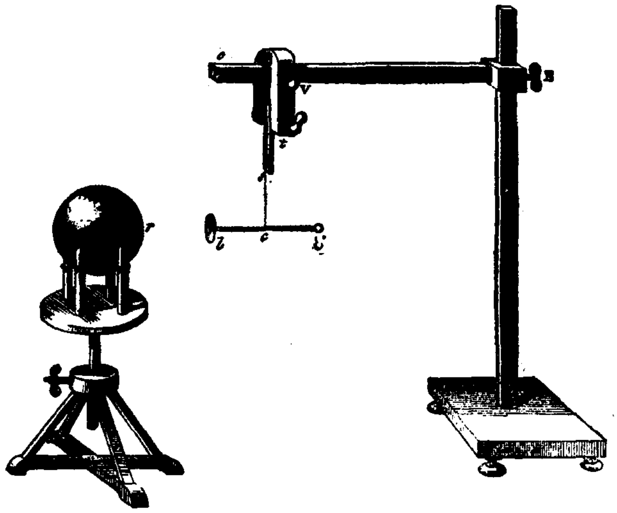

Fig. 2. Diagram of Coulomb's second experiment (attractive case).

At the beginning of the experiment the ball was charged by a spark from a Leyden jar. The disc was given a charge of opposite sign by a short contact with a small grounded conductor. The needle was then set in oscillation with an amplitude not greater than 30 deg. The time for a certain number of oscillations and the distance of the disc and the sphere were determined. The disc was taken farther away from the sphere. Then the distance between the sphere and the disc was increased and the measurement was repeated. Coulomb gave the following results:

> Trial 1—The plate 1 being at 3 inches from the surface of the sphere or 9 inches from its center gave 15 oscillations in 20″.
>
> Trial 2—The plate 1 distant by 18 inches from the center of the sphere gave 15 oscillations in 40″.
>
> Trial 3—The plate 1 distant by 24 inches from the center of the sphere gave 15 oscillations in 60″.[5]

These were the only data Coulomb gave as an experimental confirmation of the inverse square law in the case of unlike charges.

IV. RESULTS OF THE REPLICATION OF THE EXPERIMENTAL SETUPS

The replication of the setups and measurements are—contrary to appearances—not without difficulties. On the one hand, this is due to the high sensitivity of the apparatus, which probably was at some points the best Coulomb could realize (e.g., the diameter of the thread of the torsion balance). On the other hand, the forces to be measured are very small so that they could only be determined for the first time by the use of the torsion balance. Coulomb declares that "to twist this thread, by acting on the point *a* which is four inches away from the wire...through 360 degrees it is necessary to apply at point *a*, a force of 1/340 grain (437.5 grains = 1 ounce (av.))".[6]

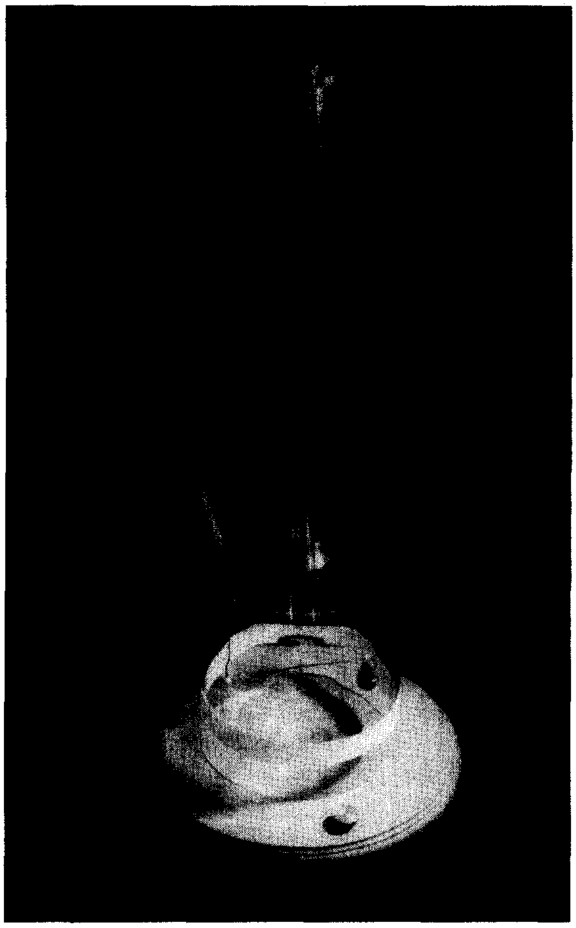

Fig. 3. Replication of the torsion balance.

Every part of the torsion balance replicated at the University of Oldenburg (Fig. 3) had the same dimension as given by Coulomb with a divergence always less than 10%. There are some details that could not be replicated exactly as described in Coulomb's memoir. The wire was made of copper instead of silver because a silver wire would have been too expensive. The torsion micrometer could not be rebuilt as described. As a consequence, the wire was not clamped but soldered. And finally, the needle was not made of silk soaked in Spanish wax because it seemed to not have enough stability. Therefore, the needles were made of PVC. As to the second experiment, the materials were the same as described in Coulomb's memoir, the divergence of the dimensions always being less than 5%.

In the following, we will only discuss the torsion balance experiment in detail. This is justifiable from the historical as well as the modern point of view. For Coulomb and his contemporaries, the torsion balance experiment was the more important one. In the introduction of his second memoir Coulomb wrote that he was able to determine the inverse square law also in the case of unlike charges with the torsion balance. Nevertheless, he designed the second experiment although he was aware that it was not a direct proof. But in this experiment not that many precautions were necessary. As a matter of fact, subsequent historical discussions of Coulomb's work only referred to the torsion balance experiment. Even in modern references only this experiment is discussed.

V. DISCUSSION OF THE RESULTS OF THE TORSION BALANCE EXPERIMENT

During the reproduction of the experiment several errors could be observed which can be classified into two groups. The ones of the first kind can be avoided if the experiment is carried out carefully. These are, for example: (1) The counterbalance that slows down the oscillations should not be too large or the balance would not be as sensitive as possible. (2) The mass hanging below the wire should not be too large or the torsion of the wire will not be proportional to its displacement angle. (3) The same error will occur if the torsion of the wire exceeds its limit (which is not fixed). When this limit is exceeded it will decrease for the following experiments and the measurements are disturbed. These are some of the errors that occurred during the reproductions. Although they might have caused greater problems for Coulomb's contemporaries trying to reproduce his results (because the knowledge necessary to avoid them was not widely available at that time) they were not decisive either for Coulomb (who was a specialist in torsion experiments) or for modern reproductions.

The second kind of errors can be classified as the unavoidable ones. They are very important for the discussion of the validity of the data obtained from the replicated experiment as well as for the conclusions which can be drawn from Coulomb's data. During the experiment, three errors occurred that could not be avoided. The first problem was the redistribution of charge that caused a decrease in the distance separating the pith balls. This error was described correctly by Coulomb and he evaluated it correctly, too. He wrote that the measurement took 2 min and in 3 min the distance separating the balls diminished by only one degree. Therefore, this error had no remarkable influence on the results of the experiment. The second error was completely ignored by Coulomb. As in every other experiment the precision of the measured data is limited by reading accuracy. During the reproduction the angle of the torsion micrometer could be determined with a precision of one degree; the position of the movable ball could be determined up to half a degree. Coulomb never mentioned the exactness but stressed the sensitivity of the torsion balance several times. "I submit today to the Academy an electric balance ..., it measures very exactly the state and the electric force of a body however slightly it is charged."[7] Samuel Devons remarked critically: "In my reading, he confuses that sensitivity with precision."[8]

Coulomb described another error. In the first remark of his memoir,[9] he mentioned problems with the draught of air that caused oscillations of the movable pith ball which made it impossible to measure its zero position. In order to eliminate this error, Coulomb made two proposals which will be discussed later. During the reproduction these oscillations could be observed, too, hence it was absolutely impossible to measure the exact position of the movable pith ball because of these oscillations. This is exactly the phenomenon Coulomb described. But even when there was no draught of air the oscillations still occurred. In order to investigate the causes for the oscillations the experiment was repeated with an air draught, and it was impossible to distinguish the kind of oscillations it produced from the ones before.

Moreover, in none of the experiments was it possible to obtain the results Coulomb claimed to have measured. There were several series of experiments that could be ex-

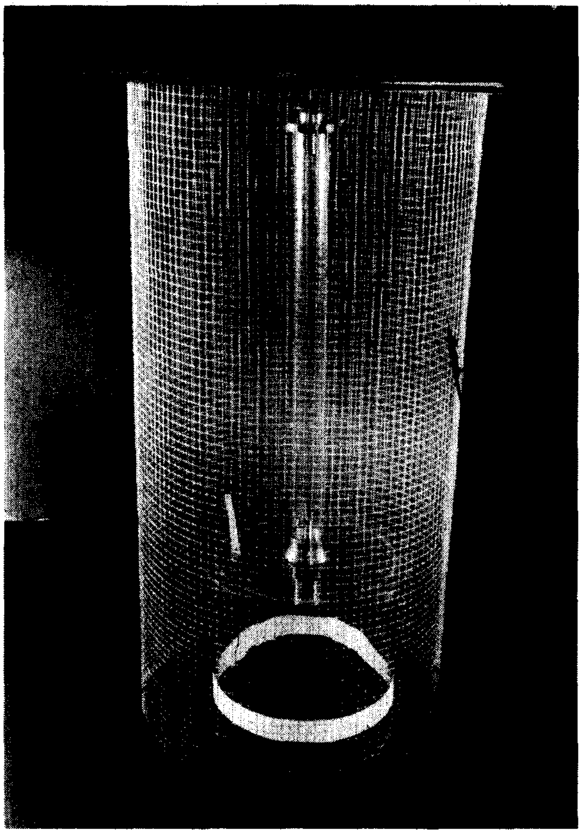

Fig. 4. Replication of the torsion balance surrounded by a Faraday cage.

plained by exponents of the separation distance between 1 and 3. And only in a small number of experiments was it possible to determine an exact exponent; in most cases it seemed as if the exponent varied during the experiment. During the experiments, it was striking that the experimenter always was charged himself. This charge certainly has an influence on the results of the experiment. In order to eliminate this error the torsion balance was surrounded by a Faraday cage (Fig. 4).With this alteration of the experimental setup no more oscillations could be observed; it can be taken as confirmed that the oscillations were caused by the charge of the experimenter. Besides, it was possible to reproduce Coulomb's inverse square law with this variation of the experiment.

VI. CONCLUSIONS FROM THE REPLICATION OF THE EXPERIMENT

The question to be answered now is whether Coulomb derived his relation mainly from measurement or from theoretical considerations. Our further investigations focus on whether Coulomb was charged himself during the measurements he made—or not.

There are three references showing that Coulomb did not take this important source of error into consideration appropriately. He had problems with the electrostatic charge of the observer during another experiment in 1782.[10] Coulomb came to the conclusion that the problems occurred due to electrostatic charges in the air around his apparatus. He finally solved these problems by changing the setup. Therefore, it does not seem improbable that he had the same problems in 1785 when he made his experiments with the torsion balance.

It has already been mentioned that the oscillations occurring during the replication of the experiment without the Faraday cage corresponded to the first remark in Coulomb's memoir.[11] The second argument for the hypothesis of the charged experimenter is that Coulomb described the oscillations very precisely. From our observations they only occur when the experimenter is charged during the experiment. Therefore, it seems very plausible to suppose that Coulomb was charged himself during his measurements. In the same text another argument can be found which supports the view that Coulomb derived his relation by theoretical considerations. Coulomb wrote that:

> the natural position of the needle, where the torsion is zero, can only be determined up to 2° or 3°, irrespective of how motionless the air might be and what precautions one might use. Therefore, to have a first experiment that is comparable to the following ones, one has to give a torsion to the wire of 30° or 40° after the electrification of the two balls. This will result in a sufficient torsion force together with the distance of the two observed balls whereby the 2° or 3° of insecurity in the normal position of the needle, when the torsion is zero, does not cause any perceptible error in the results.[12]

This means that it is not possible to get the result Coulomb claimed to have found in the first experiment (remember that the index of the micrometer was on 0°). Coulomb continues:

> Furthermore, it has to be mentioned that the silver wire I used in this experiment is that delicate that it tears with the slightest shock. I found out later that it is more convenient to use a wire with a diameter twice as big in the experiments, although its torsion capacity is fourteen or fifteen times less than the one of the first. ... it has to be remarked that when using this second wire the torsion should never be more than 300°... .[13]

The torsion limit of this wire Coulomb gives here contradicts the result of his third experiment (remember that the torsion was said to be 567°). All in all these two statements of Coulomb show that he 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. In order to understand why Coulomb published these data anyway, one has to look at the discussion about the correct force-distance relation.

VII. REACTIONS TO COULOMB'S RELATION

Prior to the publication of Coulomb's memoir in 1785 some scientists had already formulated the inverse square law from theoretical considerations, especially Joseph Priestley, Henry Cavendish, and Lord Stanhope. (Cavendish and John Robison succeeded in giving the experimental proof of this relation, but both papers were only published in the 19th century and so they had no significance for the discussion at that time.) In the years after the publication of Coulomb's memoir several papers disputing this work were published. Probably the first one which referred to Coulomb came from Great Britain when, in 1790, Deluc published his so-called "cinquième lettre ... sur le fluide électrique." In this letter Deluc argued: "I have come to think that the law observed by M. Coulomb depends on the nature of his apparatus... ."[14] Deluc accepted the results of Coulomb's experiment, but he interpreted these results differently. He did not consider the distance of the two pith balls to be decisive but thought that one has to find a reference point in the air between the two balls where no electrical action takes place. This was not the only criticism that British scientists put forward. In 1836 William Snow Harris published a paper in the *Philosophical Transactions,* where he described experiments he had made to determine the force-distance relation. He came to the conclusion that "the law of the force, which at first was as $1/d^2$, became at a certain point irregular, until at last the repulsion vanished altogether, and was superseded by attraction."[15] The date of his critical publication is quite astonishing because meanwhile Poisson had published a mathematical theory that supported Coulomb's results. Nine years later this was one of the arguments in William Thomson's refutation of Snow Harris's results: "In the papers of Poisson on electricity we find the analytical solution of the problems that are combined with the most important parts of Coulomb's experimental researches; the correspondence of the results is very satisfactory, and the strength and beauty of the analysis are placing the theory of electricity next to the theory of gravitation, through mathematical correspondence at the first place of natural science."[16] But among British scientists Snow Harris and Deluc were exceptions; in general the inverse square law was accepted in Great Britain, although it was not generally accepted that Coulomb was the first to prove this relation. In France the reactions were similar; no opposition to Coulomb's law is recorded.

In Germany the situation was completely different. The first to formulate a different relation was Paul Louis Simon (1767–1815) who published a paper in Gilbert's *Annalen* in 1808. Prior to this paper a letter of Simon was published where he gave a comment on his article:

> Still I hesitated to make my experiments public because I hoped to discover any mistake in them and it was ever so embarrassing to me to displace such an acknowledged law. Having eliminated the possibility of such an error through frequency repetition and the variation of my work, I hold it to be my duty, however, to finally propose my paper .... And so I hurry all the more as I read in one of the latest volumes of your Annalen that Volta is preparing a paper, in which he hopes to overthrow Coulomb's law by electrophoric experiments and to prove that electrical attraction and repulsion stand in direct inverse relation to distance. This report raises all my doubts which I still had instead of my numerous experiments, because I couldn't persuade myself that Coulomb should have been wrong.[17]

The paper Simon referred to was a note by Maréchaux saying:

> Hr. Ritter paid a visit to Volta. This magnificent physicists is preparing a paper which should invalidate Coulomb's experiments and overthrow his law that electrical attraction and repulsion increase as the squares of the distance decrease. After Volta both stand in the simple inverse relation to distance ... .[18]

Obviously Simon did not dare to publish his results before he knew that Volta, one of the most famous researchers in electricity at that time, obtained similar results. In his paper[19] Simon first described some of the experiments he made with his electrometer. This apparatus (Fig. 5–7 in Ref. 19) is similar to a sort of beam scale. In his apparatus he used—like Coulomb—two pith balls, a fixed one and a movable one at the end of one of the arms which were made of glass. At the beginning of the experiment he charged the balls. The repulsion of the balls was compensated by pieces of thread placed on the other arm of the beam. The results of Simon's experiments led him to the conclusion that the electric force is inversely proportional to the distance between the charges.

In the following years this relation seemed to be accepted to a certain degree in Germany, especially because other scientists published papers which supported either Simon's theory or, at least, contradicted Coulomb as well as Simon. The most important examples are Schweigger in 1812, Parrot in 1817–18, von Yelin in 1820, Mayer in 1819–22 and Kämtz in 1823.[20] Finally the discussion came almost to an end, when in 1825 a paper of P.N.C. Egen was published.[21] At the beginning of his paper, Egen gave some theoretical considerations in favor of Coulomb's relation. Then he discussed the experiments of Simon and Mayer and characterized the ones of Parrot and of von Yelin as not being very important. Egen declared Mayer's experiments wrong, in contrast to Simon's experiments which he thought to be correct, but wrongly interpreted. Simon had made a mistake when determining the distance of the two balls he used in his experiment. In his calculations he used the distance between the surfaces of the two balls. By using the distance between the centers of the two balls in his calculations Egen was able to show that Coulomb's law agreed properly with Simon's data. Another paper of Egen[22] published in 1828 shows that the discussion had not completely been abandoned, but that Coulomb's relation had been accepted in general.

But there were not only German scientists contradicting Coulomb's relation. It has already been mentioned that Volta was trying to find another relation. Another scientist—today as famous as Volta—who opposed the inverse square law was Oersted. He formulated a relation comparable to Snow Harris's. But it is not known that any other relation had been accepted in any other country but Germany.

VIII. THEORETICAL BACKGROUND OF THE DISCUSSION

As already pointed out, it is very astonishing that the inverse square law was not generally adopted because the analogy of this law and the fundamental law in gravitation (formulated by Newton) must have sounded very plausible. But wherein lies the reason for the acceptance of Simon's law in Germany?

The success of this relation is understandable if there was another relation that is analogous to Simon's law and generally accepted, too. And in fact the law of Boyle–Mariotte could serve as an analogy. This can be seen in a paper published by Schweigger in 1812: "...the law of Mariotte for elastic fluids [i.e., gases] corresponds to the one found by Volta and Simon for electrical repulsion."[23] Since Mariotte's law is valid for gases we can conclude that the supporters of Simon's law regarded electrified bodies as being surrounded by a sort of atmosphere which is formed of a certain electric substance. Consequently, the mechanism of propagation of the electrostatic force is based on a proximity theory. (It has to be remarked that this is probably not true in the case of Simon who was surprised to have found empirical data contradicting to Coulomb's.) The model used by Coulomb and his supporters was one of an action-at-a-distance according to the analogy to Newton's fundamental law of gravitation. At the end of the 18th century it was considered to be connected with that model (although Newton himself did not believe in action-at-a-distance). As Heilbron has remarked:

> Newton's first readers, who understood correctly that he believed in his universal centripedal accelerations, did not interpret his procedures in the later standard instrumentalist sense. Moreover, the intensity of his belief predisposed them to disregard his occasional disclaimers and to conclude—wrongly this time—that the Principia advanced a particular view of the cause of gravity. Its frequent references to mutual and equal attractions, to bodies drawing one another across resistanceless spaces, to powers exercised in proportion to mass, to accelerative forces diminishing as the square of the distance, made natural the interference that Newton held gravity to be an innate property of bodies, and to act immediately at a distance.[24]

That Coulomb, too, tried to achieve this analogy can be seen in the way he introduced the charges in his law. As this involves the definition of the charge it can be done in different ways. According to Hammon:

> Coulomb had not described any experiment to establish the dependence of the force on the quantities of charge on the spheres. His statement of the charge-dependence of the force was clearly by analogy with Newton's law of gravitation. This points out, perhaps more strongly than any other aspect of the history, the character of Coulomb's law as a *definition* of the quantity of charge. His choice of the simple product of the quantities of charge is certainly the simplest choice, but it is *not* the only possible choice.[25]

Thus:

> He could, for example, have chosen $D=(qq')^2$. This choice still retains the symmetry of the force law, but we would find that two small, identically charged objects exert *four times* the force, when placed next to each other, than either one of them alone.[26]

Coulomb's way of defining charge strengthened the analogy to Newton's gravitational law once more.

There is another fact that is worth mentioning in this respect. Coulomb used the product of the charges only in his second paper to explain why the torsion balance was too difficult to be used in the case of attraction. But it seems as if this part of the law was not that important either to Coulomb or to any other scientist; no one seems to have disputed this relation. Obviously, the force-distance relation was much more meaningful for the discussion, which is understandable because of its importance for the different theoretical concepts of electricity.

The different concepts of propagation of forces corresponded to another difference: The idea of an electrical substance distinguished Coulomb and his supporters from his critics. Kuhn describes the situation as follows:

> During the first half of the eighteenth century—when electrical forces were explained as the result of effluvia emitted by the entire charged body—almost every experimental investigation of the force law involved placing a charged body a measured distance below one pan of a balance and then measuring the weight that had to be placed in the other pan to just overcome the attraction. With this arrangement of apparatus, the attraction varies in no simple way with distance. Furthermore, the complex way in which it does vary depends critically upon the size and material of the attracted pan. Many of the men who tried this technique therefore concluded by throwing up their hands; others suggested a variety of laws including both the inverse square and the inverse first power; measurement had proved totally equivocal. Yet it did not have to be so. What was needed and what was gradually acquired from more qualitative investigations during the middle decades of the century was a more "Newtonian" approach to the analysis of electrical and magnetic phenomena.[27]

It can be clearly seen in some of the terms used by the supporters as well as the opponents of Coulomb's relation that the idea of the electric charge was an important detail of the theoretical background of the discussion that took place. In other words, Kuhn is correct but it seems as if the older ideas of charged bodies emitting electric effluvia had not disappeared at the end of the eighteenth century. For example, Deluc assumed electrical atmospheres and wrote that:

> There is a high probability that steam is very similar to the electric fluid: So that when these two fluids manifest themselves, the one in the hygrometer, the other in the electrometer, their principal function in this state is to spread their components in the atmosphere ... .[28]

This opinion is completely different from Coulomb's and his supporters', who were using the term "electrical masses" associated with Newtonian physics. A scientist's conception of the substance of the electric matter also influenced the relations that seemed plausible to him.

IX. CONCLUSIONS

From our work in replicating Coulomb's experiments it seems quite plausible that Coulomb did not find the inverse square law by the doubtful measurements from his torsion balance experiments but by theoretical considerations. Thus we cannot agree with Kuhn when he states that Coulomb's "measurements were necessary to produce a firm consensus about electrical and magnetic attractions—they had to be done; science cannot survive on guesses... ."[29] It was not mainly the ingenuity of Coulomb's experiments that convinced major parts of the scientific community but the mere fact that a renowned scientist like Coulomb claimed to have found empirical data that fit into the leading conceptional framework. The decision between the possible relations concerning force and distance was not made by striking and valid empirical data but by confirmation of the analogy between electrical and mechanical concepts. The dispute on the correct relationship between force and distance was not only about differing empirical data but was one about competing theoretical concepts.

ACKNOWLEDGMENTS

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[2] After W. F. Magie, *A Source Book in Physics* (McGraw-Hill, New York, 1935), pp. 408–413.

[3] See Ref. 2, p. 412.

[4] See Ref. 2, pp. 413–416.

[5] See Ref. 2, p. 418.

[6] M. H. Shamos, *Great Experiments in Physics* (Henry Holt and Company, New York, 1959), p. 64.

[7] See Ref. 2, p. 409.

[8] See Ref. 1.

[9] C. A. Coulomb, *Vier Abhandlungen über die Elektricität und den Magnetismus*, Ostwald's Klassiker No. 13, edited by W. König (Akademische Verlagsgesellschaft, Leipzig, 1921), p. 9, author's translation.

[10] See C. S. Gillmor, *Coulomb and the Evolution of Physics and Engineering in Eighteenth-Century France* (Princeton U.P., Princeton, NJ, 1971), p. 147.

[11] Unfortunately, this remark is contained neither in Magie's nor in Shamos' translation.

[12] See Ref. 9.

[13] See Ref. 9, p. 9, author's translation.

[14] Deluc, "Cinquième lettre ... sur le fluide électrique," J. Physique **36** 450–469 (1790), see p. 453, author's translation.

[15] W. Snow Harris, "Inquiries concerning the Elementary Laws of Electricity, Second Series," Phil. Trans. **126**(1), 417–452 (1836), see p. 433.

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[17] P. L. Simon, "Auszug aus einem Schreiben ... an den Professor Gilbert in Halle," Ann. Physik **27**, 325–327 (1807), see pp. 326/7, author's translation.

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Research as a guide for curriculum development: An example from introductory electricity. Part I: Investigation of student understanding

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This is the first of two closely related articles that together describe how results from research can be used as a guide for curriculum development. This first article shows how the investigation of student understanding of electric circuits by the Physics Education Group has contributed to the building of a research base. The second article describes how the group has drawn on this resource both in developing a curriculum for laboratory-based instruction and in adapting this curriculum to fit the constraints of a traditional introductory course. Also discussed is how, in turn, development and implementation of the curriculum have enriched the research base.

I. INTRODUCTION

The Physics Education Group at the University of Washington has for many years been engaged in a coordinated program of research, curriculum development, and instruction. We have examined student difficulties in various domains of physics and have used the results from this research to design instructional strategies that address these difficulties. This is the first of two closely related articles that together demonstrate how research and curriculum development are conducted by our group as interactive components of a single iterative process.[1] In this article, we show how our investigation of student understanding has contributed to the building of a research base that can be used to guide the development of curriculum