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# COULOMB'S ELECTRICAL MEASUREMENTS

## Experiment No. 14

## Contents




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## I. BACKGROUND

The study of electricity in the latter half of the 18th century can be viewed in the light of two major influences. One was the refinement, with increasing mathematical sophistication, of the principles and methods of Newtonian mechanics, their general acceptance, and ever-widening attempts to apply them. The other was the great surge in experimental exploration of electricity and the development of electrical technique, which reached a climax in the mid-century with the development of electrical machines, the discovery of the Leyden-jar and the demonstration of atmospheric electricity and the electrical nature of lightning. For the most part the subject had been in the hands of the experimenters - many amateurs - and in particular physicians, and its continued exploration broadened the range of phenomena, and brought electricity into the domains of chemistry, physiology and medicine. From the few isolated and arcane phenomena known at the beginning of the century, the subject had in 50 years so grown that in 1767, Joseph Priestly could write:

> "The electric fluid is no local or occasional agent in the theater of the world. Late discoveries show its presence and effects are everywhere, and that it acts a principle part in the grandest and most interesting scenes in Nature." (1)

Inevitably the attempt would be made to bring an agency with such ubiquitous manifestations, albeit often exotic ones, within the domain of the apparently universal principles of mathematical philosophy. The challenge was a clear one; but who woulddare, or was able, to accept it?

Hardly the great mathematical philosophers of the day! Some may have been ignorant of, or disintersted in the subject; others may have shared Euler's feelings:

> "The subject almost terrifies me. The variety it presents is immense, and the enumeration of facts serves to confound rather than to instruct...What occult powers of nature are capable of producing effects so surprising...The greatest part of Natural Philosophers acknowledge their ignorance in this respect..." (2)

On the other hand, few of those who experimented with electricity were sufficiently well-versed in Newtonian philosophy or fluent in mathematics to attempt to bridge the gap that lay between the formal-theoretical and the practical-experimental. If they theorized at all, they limited themselves, perhaps wisely, to empirical, ad-hoc theories limited to the domain of electrical phenomena per se . Even the simpler manifestations of electricity, the forces between electrified bodies and the distribution of the electric fluid between conductors and insulators, presented, as we can now appreciate, problems requiring subtlety of concept and refinement in analytical technique far beyond that attained in 18th century electricity.

The scientific academies and societies must have frequently brought together the investigators of diverse talents, interests, and occupations: but this alone was not sufficient to bring about a unified understanding of electricity. This could only be done by genius of a high order, or some rare combination of insight and experience. Two attempts to combine the experimental-practical with the theoretical-analytical are outstanding: those of Henry Cavendish (1731-1810) and Charles Coulomb (1736-1806).

The significance of Coulomb's work, or the esteem in which it was held, is amply testified by the adoption (in 1881) of the coulomb as the unit of electric charge, and the virtually universal reference to the law-of-force in electrostatics as "Coulomb's Law". With such attributions Coulomb's name has become almost synonymous with electrostatics, and Coulomb's experiments with its empirical basis. But, as is so common in science, the lapse of time has idealised the achievement and its history -- what Coulomb actually did, as distinct from what Coulomb's name signifies. Cavendish's contributions to electricity are, today, almost forgotten. But it is hard to believe, despite their subtlety, their forbiddingly austere style of presentation, and their only partial publication (1771, 1777), that Cavendish's work did not provide inspiration to some of his contemporaries, Coulomb included.

Coulomb's own experimental work in electricity, and the experience, talents, and attitude he brought to this subject are the focus of discussion here. Cavendish's contributions are treated separately (Experiment No. 13).

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(1) Joseph Priestley, <u>The History and Present State of Electricity</u> (1st Edition 1767, 3rd Edition 1775, Johnson Reprint 1966). Vol. I. Page xiv.

(2) Leonhard Euler (1707-1783), <u>Letters to a German Princess</u>. Vol. II. Page 85 (1761).

## II. CHARLES AUGUSTINE COULOMB

### 1. C. A. Coulomb[1]

Charles Augustine Coulomb was born a few years earlier and died a few years later than Henry Cavendish. His scientific activity spanned precisely the same period - the last third of the 18th century. Both made outstanding contributions to the electrical science of their day, although for both electricity was but one of a much wider range of interests one which occupied their attention for but a few years. Formally their aims and achievements were quite similar: by a combination of theory and experiment to transform electricity from what must have seemed at best a qualitative science into a quantitative one; to replace the spectacular and theatrical by the intelligible and analytical. Yet there is no evidence that Coulomb and Cavendish ever met or communicated with each other; neither makes any direct reference in his writings to the work of the other.[2] They lived and worked in worlds apart, as different in character as their own personalities, and not simply the France of Louis XVI versus the England of George III. Cavendish's was the world of philosophical contemplation, of investigation and measurement. Nature in all its workings and manifestations wholly absorbed his interests and attention, to the exclusion of the affairs of men. Coulomb's world and life was one of public works and practical affairs, of planning, designing, executing and constructing, and of understanding in order to utilize. For Coulomb it is the artifact or the machine, more often than Nature itself that is studied. In man's handiwork - rather than God's - he sees Nature already civilized and orderly, and thus more amenable to analysis by his instruments and regulations and his laws. Coulomb was first and remained foremost an engineer, one of the greatest of his age, devoted to practice and dedicated to service: service to his colleagues, his profession, his country, and finally to science.

Born in 1736 in provincial France of good bourgeois stock - "une famille de magistrats" associated with law, finance and politics - Coulomb spent most of his early years in Paris where his father, a petty government official, was stationed. This allowed him to enjoy some schooling at the exclusive Collège Mazarin, an institution which boasted an observatory and a good reputation for mathematics, to attend lectures at the Collège Royal de France, and so to develop what would become his lifelong aptitude for mathematics. At the age of 20 Coulomb returned with his family to their traditional country home, at Montpellier, and there for a couple of years his interest in science and mathematics was further nurtured at that

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(1) Notes and references to this section follow, pp. 20-22.

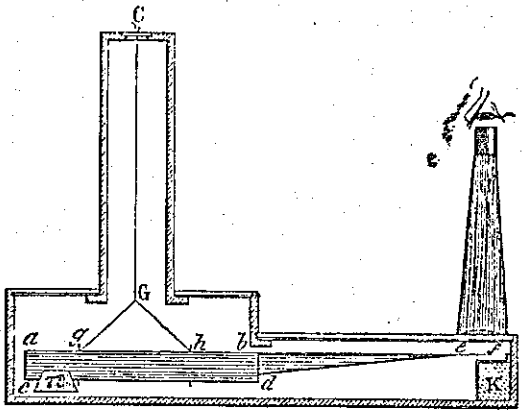
city's Societé Royale de Science (the second royal society of science to be founded in France - in 1706). The prospects of a quiet provincial life, one with modest opportunities for cultural and scientific activity, did not seem to satisfy Coulomb's energies and ambitions, or his need for some solid professional occupation. He decided on a career in military engineering - in the Corps du Génie, and so in 1758 he returned to Paris to prepare himself for the rigorous entrance examination to the School of Military Engineering at Mezieres, at that time the foremost technical school in Europe. Two years of disciplined training and wide-ranging instruction - which included much mathematics and some science, as well as engineering design, manual crafts, and hard practical construction work, and Coulomb, aged 25, graduated as lieutenant in the Corps, with a fair record and some evidence of a flair for mathematics, but with little to mark him out from others who enjoyed similar educational opportunities.

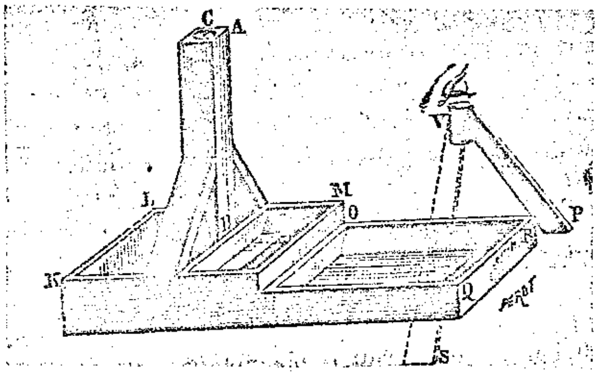
On entering the service, Coulomb is first posted to the naval base at Brest , but shortly thereafter, early in 1764, he is ordered to the French colony of Martinique, in the Carribean, to assist in the planning and rebuilding of the military fortifications of the island. Here for nine years, often in the most demanding circumstances and debilitating conditions, Coulomb acquired great practical experience in the management of both men and materials, and was afforded endless opportunities of putting to the stern and exacting test of practical accomplishment the theory of his earlier training. It was here at Martinique, despite the exigencies of the work, the inhospitable climate, and the increasing burden of responsibility, that Coulomb found opportunity to develop and test many of his ideas on the mechanics of materials and structural foundations; to begin to formulate what was, at that time, the barely nascent science of soil-mechanics. It was here that he develops and discloses his conviction that the academic laboratory experiment, oversimplified in form and unrealistic in scale, is no match for the direct full-scale test demanded by real practice. He acquires great skill and wide experience in a variety of mechanical problems, and his characteristic style takes shape. It is a direct, forceful attack - whether experimental or mathematical - on the problem at hand.

In 1773 Coulomb returns to France, as a military engineer of wide experience, solid accomplishment and high reputation; but also with a renewed interest in science. His aspiration is to enter the scientific community with its splendid center at Paris. In traditional manner he submits to the Académie his first substantial scientific memoir. The subject is <u>real</u> mechanics: the bending of beams, the strength of masonery arches and piers, and the structure of foundations; but the aim is to analyse the problems in terms of more theoretical principles and concepts: of statics, friction and cohesion. And the analytical procedures are, for practical engineering of his day, relatively novel: the calculus of maxima and minima. Already, in this first memoir, is stamped the hallmark of all Coulomb's later work: the urge, which later becomes a passion and a habit, to express his findings as mathematical formulae or laws. His procedure takes one of two forms, of which it is the first that is here in evidence. On the basis of some formulated concepts or premises - for example the basic nature of friction, or fracture - he calculates theoretically what is to be expected in practice, or what the best practice should be. The formula is then tested by practical experience; or by experiment - test and measurement of a specially constructed artifact which simulates practice. As Coulomb's interests turn increasingly from engineering to science, the confrontation of theory with experiment is more often reversed. First the phenomena are examined and measured, practically or experimentally; then the results of the measurements are cast in the form of a mathematical law, or shown to be in accord with one. Both these procedures - the test of theory by "experiment", and the empirical generalizations of systematic observations, (which for Coulomb, as for others, represent a distinction between how conclusions are presented as often as how they are reached) - are commonplace in all science. What so clearly distinguishes Coulomb's style is his irresistable passion to find and formulate the mathematical law or formula in every conceivable circumstance, whether it be some profound universality or some incidental complexity, whether in abstract science or in practical engineering - even when he is analysing the daily toil performed by working men. Coulomb's faith in mathematical order, and his ability to find it, seem limitless.

His scientific efforts are not unrewarded: in 1774 he is elected "correspondent" of the Académie. But he is no less an officer of the Corps, energetically devoted to public affairs. At the same time as he is preparing his memoir on the best method of making magnetic compass-needles in competition for an Académie prize (awarded in 1777)[3], he is also preparing for Minister Turgot a memoir on the reorganization of the Corps du Génie in accord with ideals - an army at peace engaged in public works - inspired by the Roman example! In the following years he writes memoirs on rolling and sliding friction (1781) and on the elastic torsion of metal wires (1784)[4], whilst at the same time serving on commissions dealing with canals in Brittany, hospital reform, public health planning, and relief maps. In 1781, still an officer of the Corps, he is elected Member of the Académie; and in 1784 he is appointed Commissioner ("Intendant") of the Waters and Fountains of the King.

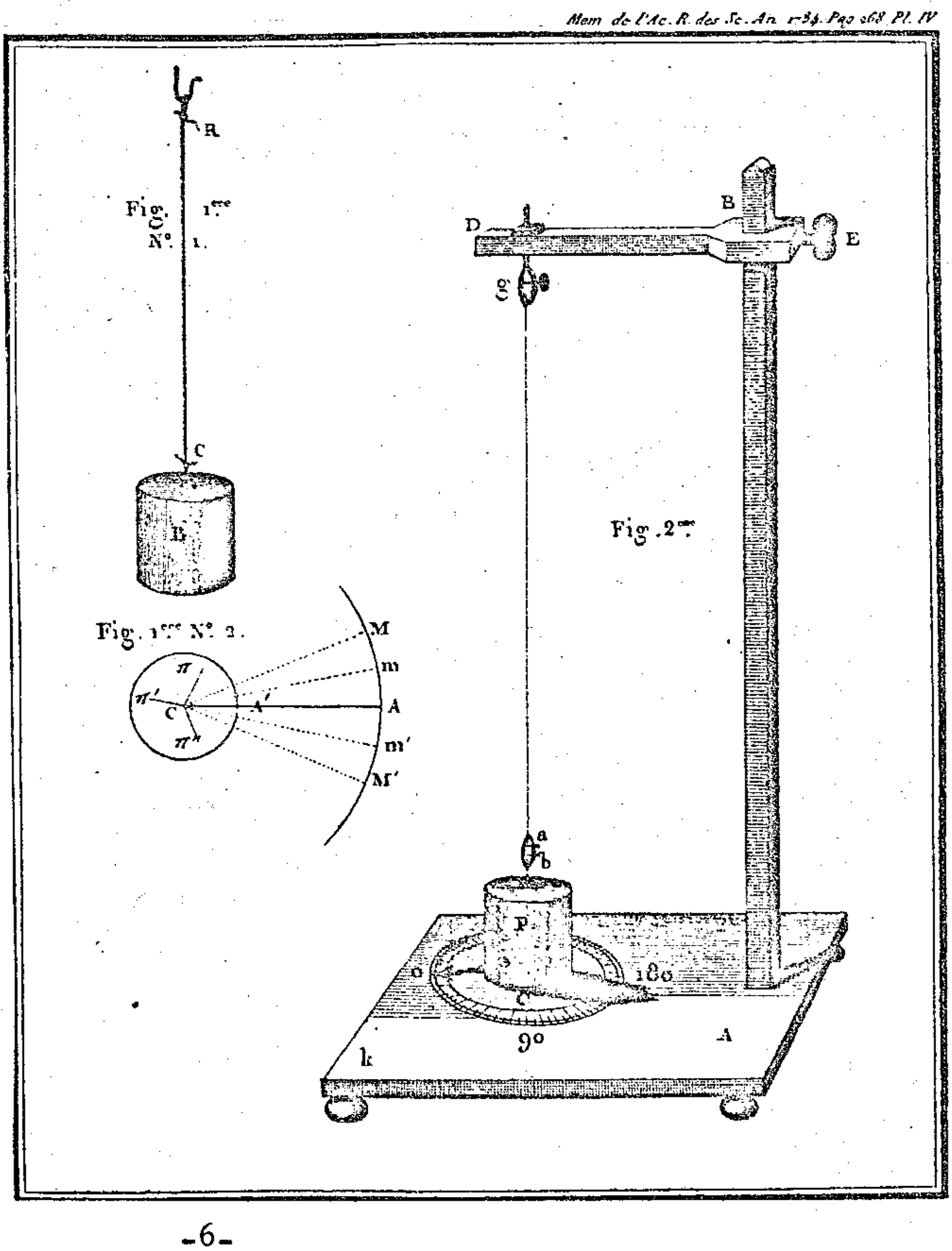
It is in this period and the few years following that Coulomb, though continuing his public duties and even adding to them innumerable administrative chores in the Académie, becomes mainly preoccupied with science, and makes his contributions to magnetism and electricity with which his name is so commonly linked.




Magnetized Needles (Bussoles) 1777. (above)

Experiments on Torsion, 1784. (right)



## 2. From Engineer to Scientist

In Coulomb's two-prize memoirs - on magnetism in 1777 and torsional elasticity in 1784 - one sees both the change from Coulomb the engineer to Coulomb the scientest and the full development of the invariable and unmistakeable Coulomb style. The work on magnetism declares itself as utilitarian - even altruistic - "to serve humanity and one's country". In a loftily worded preface Coulomb accepts the appeal (by the Académie) "to physicists and geometers", to perfect that unique instrument, the compass needle, which guides the passage of ships by making out, even when placed at the center of a vast and undifferentiated horizon, the true direction. This propensity for the grand entrance is also part of Coulomb's style ( and, perhaps, of the time and place). When he turns from proclaiming magnetism's global importance, it is to the announcement of the two "fundamental principles" of the subject; an imposing and resounding formulation of what was hardly a sensational departure from the general view of magnetism. The analyses that follow are characteristically Coulomb's: magnets of different length, shape and weight, single and grouped together, are studied, and some curious and detailed empirical generalizations are made. A profound inquiry into the nature of magnetism is neither the primary aim nor the outcome. Better compass needles ("Bussoles") are the objective.

But there are other rewards, or by-products, of these investigations. An examination of the adverse effects of friction at the pivot supporting the magnetic needle, and of the possible ways in which these could be reduced, leads Coulomb to eliminate the pivot entirely, to replace it by a suspension of a very fine wire or a hair or strand of silk. His attention turns from the magnet needle itself to the thread which supports it; indeed the magnetic forces, whose "fundamental principles" are now assumed to be understood, may be exploited to study the mechanical properties of these fine threads. He is impressed by the extreme sensitivity of which a "balance" constructed on the torsional principle is capable. He returns repeatedly to the practical problems with which the inquiries began, his respect for practical detail - even trifles - is unfailing: he rarely fails to mention that the almost torsionless silk threads he uses are taken directly from the cocoon ("tel qu'il sort du cocon"), (at other times it is the hair of an Angora goat). But the interest is no longer so explicitly utilitarian: he is intrigued now by the instrumental possibilities - to serve science as much as "country and humanity".

Coulomb's carefully constructed suspended magnets, made as he proudly declares by his own hands - virtually without any assistance - seem to enjoy a fair success, at least locally. They are installed in the Paris observatory and used for some years to record the variations of the Earth's magnetism. However there are some instrumental irregularities, due to vibrations or draughts; or, as Coulomb is led to suspect, electricity (a magnet needle suspended by a silk thread is, incidentally, electrically insulated). This is, interestingly, the first mention by Coulomb (in 1782) of any concern with electricity, the subject which is soon to become his central interest. His reaction to this suspected electrical disturbance is revealingly characteristic: it is to take forthright steps to eliminate it, or its consequences, on the basis of his surmise, rather than to explore the phenomenon and establish its nature beyond doubt. He is as vigorous in his attack on detail as on principle; and he is not given to remaining in a state of uncertainty for long.

It is, then, to the mechanics and physics of elasticity and torsion, rather than the further exploration of magnetism or electricity that Coulomb's study of the compass needle first leads. Here Coulomb is fully at home. By timing the oscillations of well-defined objects suspended by wires and threads of different materials and of various lengths and thicknesses, he arrives empirically at the general law for torsional force exerted by a twisted fibre. He recognizes the great merits of the new instrument, based on the principles he has discovered, for other physical investigations, and he applies it to an examination of the frictional resistance of fluids to the motion of solid bodies. Mechanics of materials, friction, elasticity - these are the subjects at the core of Coulomb's interest and experience; and notwithstanding his earlier skepticism as to the value of laboratory experiments he now has in his hands the ideal instrument for them: his torsion balance. And as always it is application as much as understanding that attracts and interests him; much of his subsequent research are to be exploitations of the potentialities of this instrument he has devised and perfected.

## 3. The Fundamental Law of Electricity and Magnetism.

In 1785, one year after his prize memoir on torsion and the torsion balance, Coulomb presents to the Academy his first memoir on Electricity.[5] Apart from the accidental encounter already mentioned, there is little evidence from his published works or the course of his career, that Coulomb had, before 1785, devoted any efforts to the study of electricity or evinced any particular interest in these phenomena. Now, from the outset, he seems to regard electrical forces as essentially feeble (not surprisingly for one who has built military fortifications!), and consequently an ideal subject for investigation by his most sensitive torsion balance. This is, in any case, how he introduces to the Académie his first work on electricity. He has established the laws of torsion; he has so perfected the torsion balance that it is capable of measuring a force as small as 1/10,000 of a grain;[6] he has demonstrated its value in the study of fluid friction; and now he presents before the Académie "...an electric balance constructed according to the same principles: it measures with the greatest precision the electric state and force of a body, no matter how weak its degree of electricity." Now the study of "weak" electricity has some real advantages: bodies weakly electrified are more easily maintained in a constant state of electrification than those strongly electrified, and therefore more suited to precise and reliable measurement. Whether the study of weak electricity could provide a sufficiently broad basis from which to generalize about electricity at any level, whether there might be hidden subtleties associated particularly with weak electricity - as indeed there are,[7] these were matters that could only be revealed by a thorough investigation of electrical phenomena themselves. But it was not so much Coulomb's aim to explore electricity as to measure it; to measure the forces with the "greatest precision", even if, electrically, what was being measured could not be specified with precision at all. Not surprisingly it is with the general principle that he starts. The first application of the torsion balance is to demonstrate "The fundamental law of electricity", which is stated at the outset; thus:

> "The replusive force between two small metal spheres electrified similarly, is proportional to the inverse square of the distance between their centers."

It can hardly be conceived that the announcement of such a "fundamental law" of force was, in 1785, greeted either as a great surprise or a sensational discovery. Just 100 years earlier Newton, in his monumental Principia had laid down the pattern to be followed. The fundamental law of interaction was to be established from experiment or observation, and then the laws of mechanics could be applied. For Newton it was the universal gravitational force that dominated the phenomena to be explained, and the inverse-square law the universal law of force between any pair of particles. The immense success of the Newtonian method - as well as the particular form of the gravitational law, especially in the realm of celestial mechanics - established it as the unrivalled prototype. For those who followed Newtonian principles, as Coulomb certainly did, what could be more acceptable than Newton's own law? And indeed there were already many observations and measurements, of both magnetic and electric phenomena, although nothing as conclusive as the precise astronomical measurements, that had led Michell (1750), Lambert (1758), Priestley (1767) and Cavendish (1771), amongst others to propose that the same inverse-square law of gravitation applied also to magnetism and electricity. There are, of course, some very different problems, practical and conceptional, in eliciting the law of force for electricity or magnetism and that for gravitation. The "law" expresses the dependence of the force on the separation of two interacting "elements". For gravity any (small) piece of matter is the element, and the attractive force is proportional to the invariable "quantity of matter" (the mass) in each element - an intuitive if not too precisely formulated concept. But what were, and what was the measure of, the corresponding "elements" - the more ephemeral and elusive something - of electricity and magnetism? To formulate the verifiable, fundamental law of force implies not only that the elements can be identified and in some sense recognized, but also that this can be done in a unique and unambiguous way. Otherwise might not the law depend on how the elements were defined and identified? These were subtleties not easily or readily scrutinized in Coulomb's day. The appeal of Newton's inverse-square law was almost irresistable; and it seemed possible to conceive a distribution of elements consistent with the law and with the observed phenomena. It was perhaps as much a question of tailoring the facts to suit the law as vice versa.

Coulomb, his concern with "the fundamental law" notwithstanding, delves into these subtleties no more deeply than might be expected at that time. He tacitly accepts his "small electrical spheres" as equivalent to his "elements", and if he regards this only as an approximation, he does not examine its validity in relation to his aspiration for "the greatest possible precision". He does not, indeed, present his findings as a discovery of a new law: he calls it a "determination" of the law - the presentation of measurement which confirms a law which he, as others, more than suspected before the experiments were begun. Certainly his faith in its exactness far exceeds the evidence of his experiments. For he reports measurement of the force at only three distances, and the agreement with the inverse-square law is far from perfect, (a deviation of some 12% in one of the three!) What Coulomb stresses repeatedly is the sensitivity of his torsion balance - which is quite a different matter from its precision. And even if the mechanical precision were attained, this would be no guarantee that the electrical state of the objects he is examining could be specified and maintained with adequate certainty. Coulomb is not unaware that his measurements fall short of his ideal. Amongst other disturbing features he recognizes the gradual loss of electricity from his charged metal spheres during the course of his measurements - a phenomenon he subsequently examines in some detail. But he is content to observe that "...if [!] one wished for a greater precision...one should, by a preliminary observation, determine the law of diminution of the electric force between the two spheres, and use this result to correct the observation..."(8)

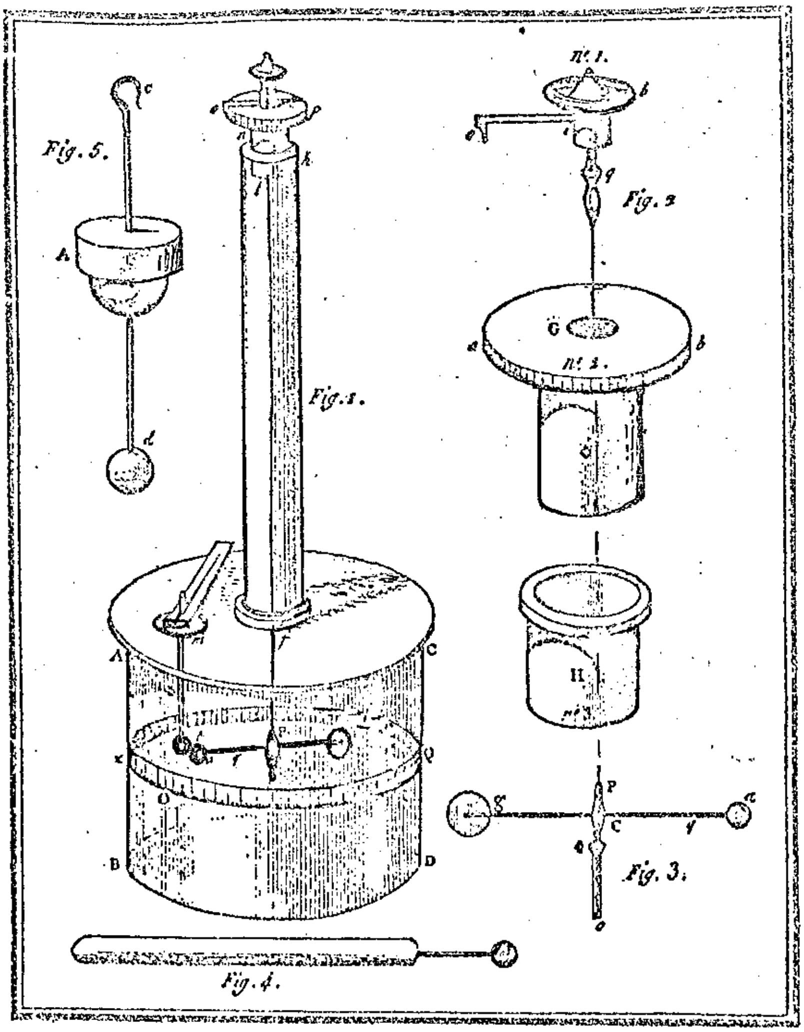
He also tells us that this diminution is small, because he "only needed two minutes to make the three measurements reported"!(9) How modest, then, would have been the time or effort required to repeat the measurements ten or even one-hundred times; to change the sequential order of the observation, so that, in one way or another, much of the error and uncertainty might have been eliminated?(10) Could it be that such thorough measurements were made, but that a frugality of style or a need for brevity compels him to omit such "details"? Hardly so, for elsewhere in his memoirs, where he is reporting detailed observations of complex situations, where the issues are immeasureably less significant than "the fundamental law of electricity", he regales us with dozens of observations and tables of data. In these complex situations the "law" cannot be forecast in advance: if a law is to be found, it has to emerge from the measurements. With the fundamental law it seems otherwise! Yet a few hours - or at most days - of careful thorough measurement with his excellently constructed instrument might have sufficed to show Coulomb that the law of force between two spheres was not precisely the simple inverse-square law; although perhaps consistent with such a law in the case of two ideal infinitely small spheres. He did not lack technique, nor ability, nor the aptitude for persevering effort - as his travails at Martinique amply testified - which such a thorough scrutiny demanded. But was this his purpose? Was it not rather - as he himself declared to the Académie, to display "immediately, before one's eyes, the law of electrical repulsion"?(11) And here, perhaps, lay the true merit of his "demonstration".

Despite its mechanical versatility and sensitivity, Coulomb's favorite instrument - his torsion balance - was not well suited to the examination of the force between unlike charges, which is attractive in contrast to the repulsive force between like charges.[12] For this purpose he turns to a method similar to the already familiar magnetic technique of observing the oscillations of a torsion-free pivoted compass needle as a measure of the magnetic forces acting on it. In place of the magnetic needle it is a suitably electrified body he uses, and instead of the pivot, a suspension - his own favorite silk thread straight from the cocoon. Remarkably his reported observations are limited, once again, to just three measurements; again the consistency with the fundamental law is far from striking, and again the irregularities are attributed to loss of electricity during the few minutes required to complete the measurements. This time Coulomb can quote some of his own measurements of this phenomenon of electrical loss, and use these to "correct" his observations. The discrepancy is explained. But as in the previous experiments, the circumstances are not as ideal as Coulomb assumes; and the critical reader of this memoir might well wonder whether faith in the inverse-square law truly rests on the evidence of the measurements.[13]

With his technical mastery and experience with his new instruments, Coulomb returns (1787) to the examination of magnetism[14] which had engaged his efforts ten years earlier. Now it is to demonstrate the same fundamental inverse-square law of force that he had demonstrated for electricity. The mechanical problems present no difficulty. Here the important issue is to recognize, and in a sense isolate, the "elements" of magnetism. Unlike electricity, magnetism - or the impalpable magnetic fluid, as it was conceived - could not be concentrated on small point-like objects: it did not come that way. Indeed if the magnetic fluid, like plus and minus electricity, was regarded as occuring in two forms: Boréal (North-like) and Austral (South-like), then it was well known, (Coulomb had himself categorically asserted it as a basic principle in his earlier magnetic work), that all magnetic objects comprise exactly equal amounts of the two fluids. The artifice which Coulomb - as others before him - used to surmount this difficulty was to use long needle-like magnets ( as were used for compass needles), where a concentration of the separate boréal and austral fluids occured at the opposite ends , or this at least was how matters were commonly interpreted. The two ends, or "poles" as they were traditionally designated, appeared then to act as centers-of-force, and were thus amenable to treatment as "elements" for the purpose of establishing a Newtonian-like law of force between them. Actually Coulomb, like Lambert before him, made a more elaborate assesment of the location of the "poles", replacing the notion of a simple pole with an extended distribution of magnetic fluid falling off in density from each extremity of the magnetic needle.

This conceptional separation of two sorts of magnetic fluid might well be regarded as an uncertain procedure - even of dubious validity in principle. William Gilbert, in 1600, would surely have castigated such attempts to separate in such two parts his essentially integral "verticity". But then he was not, as was Coulomb, determined to embrace the magnetism within the framework of the Newtonian system - forces acting between point-like pairs of particles. For Coulomb the validity and applicability of such a scheme was accepted, a priori - and justified a postiori. He could show that all his magnetic measurements were consistent with this scheme, and with the very same inverse-square law of force between the particles: or at least that with some postulated (and not unreasonable!) distribution of magnetic fluid in his magnetic needles consistency between this law of force and the observation was attainable. The Newtonian costume fitted - or at least magnetism could be squeezed into it. Needless to say Coulomb's was not the first demonstration of this feat; though it may well have been the clearest.

Torsion Balance
for
Electrical Repulsion
(1785)



## 4. Exploration of Electricity[15]

With the basic general principles of electricity (and magnetism) now established in the Newtonian pattern, the task now was to apply them to particular, generally more complex, circumstances, both to illustrate their explanatory power and to provide further confirmation - if such be needed - of their essential validity. It was a program much as Newton himself had laid down in the Principia:

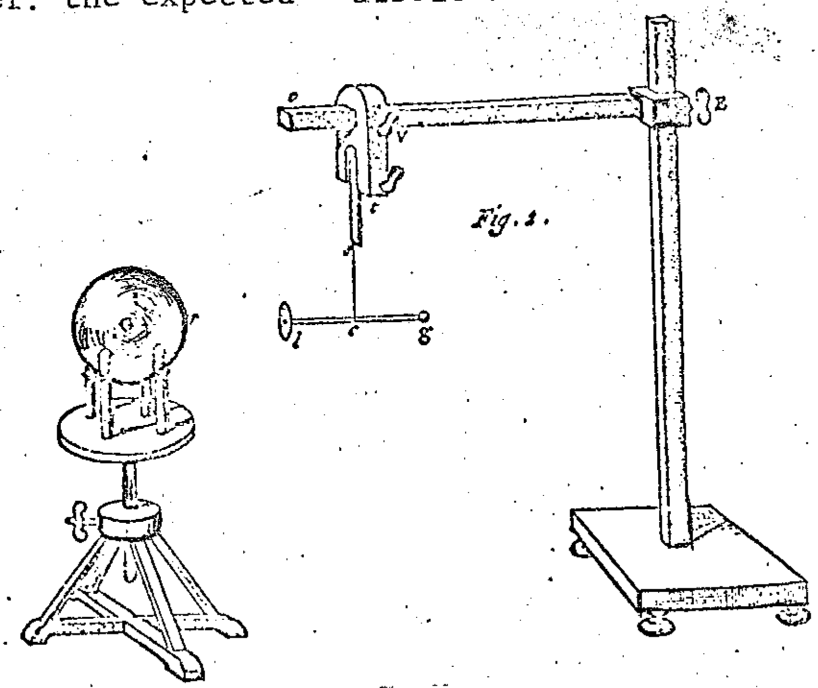
> "...from the phenomena of motions to investigate the forces of nature, and from these forces to demonstrate the other phenomena..."

For Coulomb this took the form of examining the distribution of electricity on bodies of variegated sizes and shapes - mainly spheres and cylinders, and in various combinations.[15] The task was two-fold: from the fundamental laws the distribution of electricity could - in principle - be calculated; with the well-developed instruments and technique, and again assuming the validity of the inverse-square law of force, these distributions could be measured. Experiment could check theory - or was it the converse? Coulomb's instrumental techniques are quite adequate for such an investigation; but his mathematical sophistication is hardly up to the tasks he sets himself, which are not always the most judicious or imaginative he might have chosen. For example having examined the distribution of electricity over two conducting spheres in contact (a), he continues with three (b), four (c), up to twenty-four (!). Needless to say there is some rather crude guess-work in his


mathematical analysis, but experiment and theory always agree, at least as well as is expected. Beyond these particulars some generalizations, not entirely novel, are presented: for example - that the (excess) electricity (or electric fluid) on a charged conductor always lies on the surface; that the distribution of electricity on a conductor, or its partition between different conductors in contact depends only on their geometry, and not on the particular materials (elder-wood or copper - it makes no difference!); and that the density of electricity on the surface of a large conductor can be ascertained by a sampling procedure in which a small flat conductor is brought into contact with the larger one, and then removed. These generalizations are all inferred from the fundamental laws; and subsequently confirmed by experiment. There is no discord - between experiment and theory, complete harmony. There are not even surprises. The closest he comes to the discovery of something new, or unpredictable is perhaps in his examination of the rate of loss of electricity in different atmospheric conditions. Inevitably it is a "law" he is seeking and a law that he finds: that the rate of loss of electricity is, other things being equal, proportional to the cube of the quantity of water in the atmosphere![16] (One more "law" to be added to dozens he has already found in mechanics, elasticity, fluid-friction as well as electricity and magnetism.) Otherwise in Coulomb's electricity everything appears if not predictable, at least not unexpected, and to fall obediently in place.

Could it really be that all the mystery and magic of electricity had now gone, that all could be explained - in principle at least - by the fundamental law? Remarkably in Coulomb's electrical writings there is only a single reference to the work of others - their experiments or their theories.[20] Coulomb could hardly have been unaware of the many manifestations of electricity that lay beyond the immediate range of his "fundamental" theory, even if his own experiments - or rather measurements - had steered clear of such mysteries or complexities. Coulomb was not looking for surprises; but could he, one might ask, have recognized and responded to the unexpected had it occured within his own realm of experimentation? This is certainly one hall-mark of the experimenter. We need not only conjecture. In one instance Coulomb should have been surprised! His analysis of his charge sampling procedure is faulty; his conclusion is wrong, by a factor of two.[17] Yet in his experimental test of this procedure - which was surely sensitive and precise enough to reveal such an error - (17)
he finds all is in order: the expected - albeit false - is confirmed!

Law of Force For
Electric Attraction
(1785)



## 5. Reputation and Influence

In the seventh and last memoir in the series on Electricity and Magnetism (1785-1789) Coulomb returns to magnetism - his long-abiding interest.[18] It is now the intrinsic nature of magnetism, and its distribution, rather than the formal "fundamental" law of force that is his concern. He is particularly worried by one puzzle posed by his two-fluid concept of magnetism (or indeed by any theory invoking magnetic fluids). Why if the fluids in a long thin magnet are concentrated towards the extremities, is it not possible, by breaking the magnet in the middle, to isolate one fluid from another? Experimentally this is never observed. All magnets exhibit identically equal amounts of the two sorts of fluid. Indeed this was the principle he had taken as his starting point in 1777 - an inference based on the observation that all magnets turn rather than move bodily under the (uniform) magnetic influence of the earth. To resolve this dilemna, Coulomb proposes what is perhaps his most original "philosophical" idea: that the two magnetic fluids are trapped in equal quantities in every molecule of matter. The apparent bulk separation of the magnetic fluids in a magnet is illusory - a consequence of the separation of the fluids within each molecule. Any ordinary division of matter does not split molecules, but only separates them, and since each molecule contains an exact balance of the two fluids, such a division cannot upset the overall balance of the fluids in each part. This concept, although eventually discarded with the whole notion of magnetic fluids, was destined to have a considerable influence for several decades. It illustrates Coulomb's deep feeling for a subject - magnetism - of which he had long practical and experimental familiarity. His involvement with electricity was more transitory; he seems to have been drawn into it almost inadvertently - as much to demonstrate his instrumental and analytical virtuosity as to explore the subject itself.

The end of Coulomb's scientific researches coincided more or less with the end of the ancient regime. With its collapse, Coulomb's numerous public offices and titles - associations with old order - became dubious assets. In 1790, after nearly 30 years in its service, he resigned from the Corps du Génie. In 1793, the Académie, in its old form, was abolished; and Coulomb prudently retreated from Paris to his property in the country, where for a couple of years he managed, more successfully than some of his eminent contemporaries, to survive the excesses of the Terror. Coulomb, who always appears as a most worthy, able and upright public servant rather than a passionately principled one, seems to have adjusted himself successfully to the changing political vicissitudes of the Revolution; never wholly divorcing himself or being wholly removed from public service. In 1795 he returns to Paris as a member of the Experimental Physics section in the Institute - the newly formed successor to the Académie. There are a few desultory scientific works of this period, but it is mainly as an elder statesman of science that his remaining years are spent. In 1802 he is appointed by First Consul Napoleon to his last major public office as Inspector General of public instruction - a member of a commission which included the astronomer Delambre, the naturalist Cuvier, and the chemist Fourcroy.

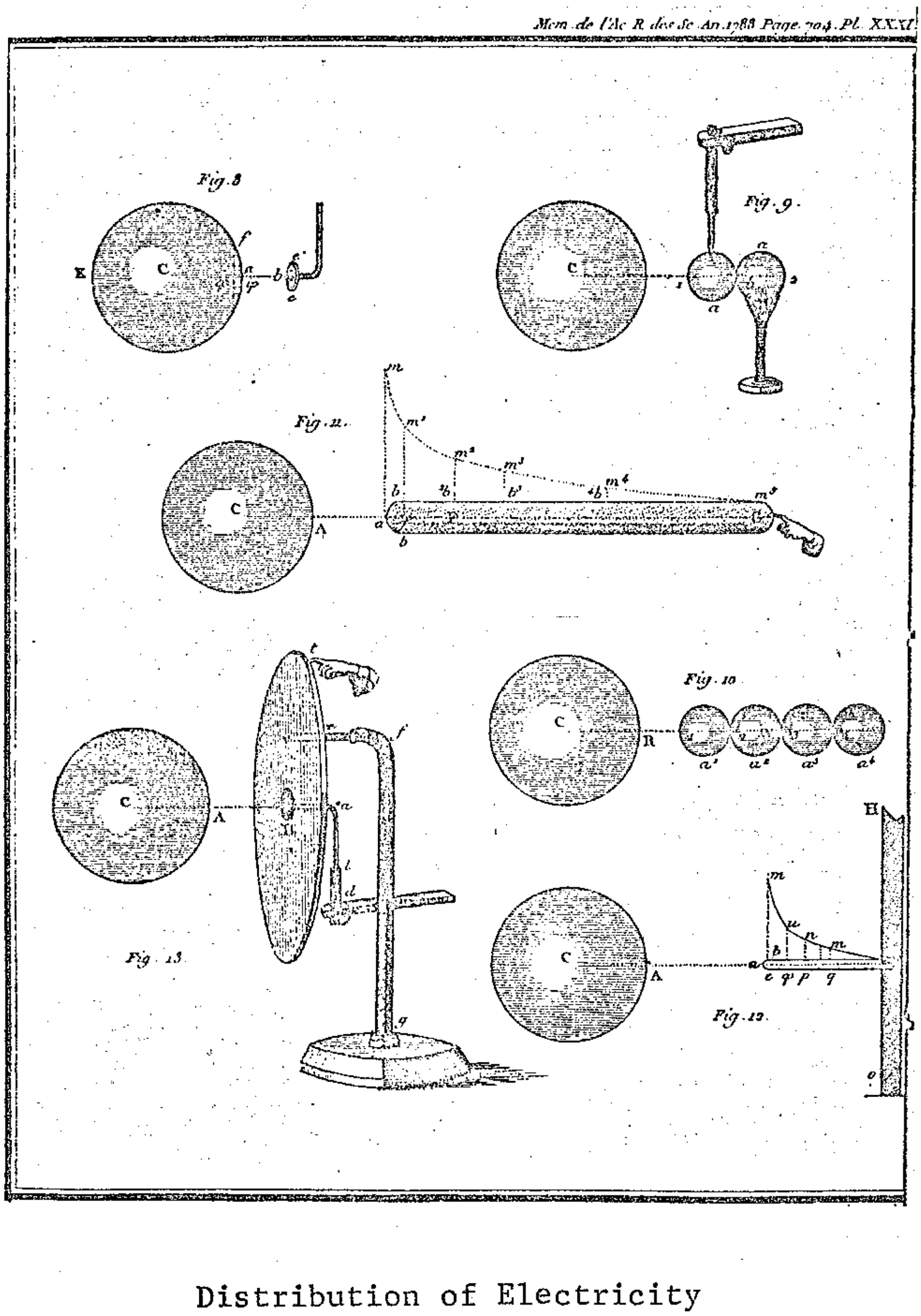
Whatever influence Coulomb may have had in this capacity on French education, and through it on the development of science, it seems more likely that the example of his life and career, his achievements and his style of scientific work were of greater impact. As an experimental scientist there seems little doubt that his example inspired many who followed him; and the association of his name with the basic law and the unit of electricity bears witness to the high esteem of his contributions to that science. But it is much easier to affirm Coulomb's influence, than to define precisely what Coulomb contributed to science of electricity or the art of experiment.

He is commonly credited with having played the major role in transforming electricity from a qualitative into a quantitative science, and thereby opened the path to its future development.(19) Yet when we turn to the major developments of electrical science in the decades following Coulomb's work - and there were some momentous ones - we find practically no reference to his work, no emulation of his methods and little exploitation of his technique. Some mathematical physicists, Coulomb's fellow countryman, Siméon-Denis Poisson (1781-1840) in particular, did use Coulomb's experimental results to test his more sophisticated mathematical formulation of the same electrical phenomena; but Coulomb's own theoretical principles were of no great originality, nor were his methods seminal. Despite their ostensible precision and sensitivity, Coulomb's electrical measurements did not lead to the discovery of any new - certainly no surprising - electrical phenomena. Sensitivity and precision of themselves did not suffice, unless assiduously directed, to ferret out what lay below the surface; what, in electricity, were subtle manifestations still awaiting exploration. This was not the thrust of Coulomb's work.

On the contrary it seems that Coulomb designed and deployed his instruments to measure the electrical phenomena as they were then known, or conceived to exist; and often enough what he measured was chosen to suit his instruments rather than the converse. But above all it was the mathematical formula that he sought. To quantify a physical relationship that was known to exist, rather than to seek new physical relationships, seemed to be his overriding passion. He was perhaps more faithful to his training as an engineer than his aspirations as a scientist. In his scientific outlook Coulomb was in many ways more a continuation of the 17th century than a prelude to the 19th . His ambition might well have been, what was later said of his countryman André-Marie Ampère (1775 - 1836 )- to do for electricity what Newton had done for mechanics. But the great triumph of the 17th century was in scientific explanation rather than exploration. The problems were age-old, the phenomena - of the celestial motions, of falling bodies, of light and color - were familiar ones. What the new principles, and more accurate observation, supplied was the rational coherent interpretation - and a pattern for the future. Coulomb followed this pattern, faithfully - perhaps too faithfully. For electricity was not simply to be ordered and explained ; most of it had yet to be discovered. And Coulomb could hardly have conceived that what he was laying out was not a comprehensive basis for the whole subject, but only a recapitulation of its first chapter.

To many of Coulomb's contemporaries, and especially those not immersed in its detailed study, much if not all of electricity appeared mysterious - as much a spectacle as a science. It was here perhaps that Coulomb provided a major service. He demonstrated how much could be made, or made to seem, orderly and quantitative. Electricity (and magnetism) need not be any less rational, or systematic or precise than the more familiar mechanics. Perhaps there were still mysteries in electricity. But the practical, orderly Coulomb was not looking for mysteries: he was more concerned to dispel than to reveal them. By using methods which were direct and rational, and readily appreciated as such (especially by those not privy to the arcane particulars of electricity), by exploiting his mechanical experience and his practical genius he was able to make electricity appear not only intelligible, but even orderly and respectable. He made electricity - as he himself saw it - intelligible to the non-electricians. Creating order and intelligibility in what is known can be a great step towards discovering the unknown; but it is no substitute for it.

For Coulomb, the great engineer, the master of mechanics, and the upright public servant, it is always order and regularity that is the aim. In his experiments and measurements it is always the "law" that he is striving to confirm or to formulate: but his "laws" represent not so much something new, as the replacement of what was vague and imprecise by what is exact, concise and mathematical. In a well-ordered universe, as in a well-laid plan or a well-designed machine, one does not seek the unknown - one's aim is to eliminate it.



Distribution of Electricity

1788

References in Text

1) For further particulars of Coulomb's life and work see Gilmore.

2) Cavendish 's two major published papers on electricity were one on general principles (1771), and the other on the electric torpedo (1776 ). Coulomb's first memoir on electricity appeared in 1785.

3) "Recherches sur la meilleure manière de fabriquer les aiguilles animantèls", Vol. IX Mémoires des Savants Etrangers (1777). Reprinted in Mémoires Relatifs a' la Physiques. Vol. I. Paris, 1884, pp. 1-62.

4) "Recherches thèoriques et expèrimentales sur la force de torsion et sur l'èlasticitè des fils de mètal". Mém.de l' Acad. Royale des Sciences, Paris, 1784. Reprinted in Mèmoires, 1884. Vol. I. pp. 65-103.

5) Mèmoires sur l'èlectricitè et le magnetism. Memoire I. Mem. Acad. Royale Sciences (1785). Reprinted in Mèmoires, 1884, pp. 107-115. Extracts from this first (and the second mèmoire) are given in English translation in: Magie, Source Book in Physics. Harvard 1965. pp. 408-420.

6) One grain 52.5 (approx) dynes force, or 0.054 (approx) gm. mass.

7) For example, the whole range of contact phenomena, whose exploration Volta was about to embark upon.

8) Ref. 5) above. p. 114.

9) Ibid. p. 113.

10) See Experimental Notes. pp. 34-37.

11) Ref. 5) above. p. 111.

12) Mèmoire II. 1785. Reprinted in Mèmoires, 1884, pp. 116-125. See also Experimental Notes, p. 37.

13) There are other factors which introduce deviations from the simple inverse-square dependence. See p. 40.

14) Ref. 12). pp. 125-

15) Fifth Memoir (1787)"On the way in which the electric fluid is distributed between two conductors in contact, etc." Sixth Memoir (1788) "Continuation of the investigation of the distribution of the electric fluid between several conductors, etc..." Reprinted in Mémoires (1884) pp. 183-229 and pp. 230-272.

16) Third Memoir (1785) "On the quantity of Electricity which an insulated body loses in a given time; by contact with air of greater or lesser humidity or via "ideoelectric" supports. Reprinted in Mémoires (1884) pp. 147-172.

17) From measurements of the distribution of electricity between a sphere (8" dia.) and a plane circular disc (16" dia.) in contact, Coulomb concludes:

"...it appears from this experiment that the electric fluid distributes itself between the plane and the sphere."

He then goes on:

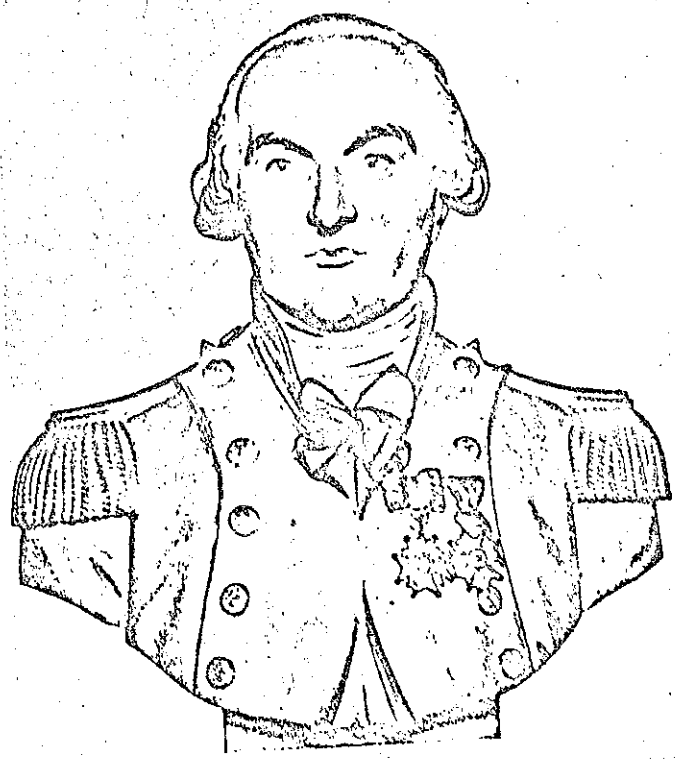
"I have found from a great number of experiments, made with much smaller planes than the preceeding ones, that this result is always true; that is to say that no matter what the diameter of the sphere and that of the plane, everytime the plane is placed in contact tangentially with the globe, it shares the electricity of the sphere in the ratio of the sum of the two surfaces of extension of the plane to that of the sphere. Experiment has given this result with high precision, especially when the plane placed in contact has a diameter very small in comparison with that of the sphere, so that when one touches, for example, the sphere of 8" diameter, with a small insulated plane of 6 lignes(about ½") diameter, it takes on each of its surfaces an electric density equal to that on the surface of the sphere. That is to say, this small plane of 6 lignes diameter becomes charged with a quantity of electricity double that of the portion of the globe it has touched." (Ref. 15), p. 253)

Coulomb proceeds to give a wholly invalid "proof" of this fake proposition (loc. cit. p. 254) and to assert that the result is "found to be exactly in conformity with experiment!" Later (loc.cit. pp. 269-270) he describes an example of experiments to measure the charge induced on a small grounded disc placed near a charged sphere. Using the incorrect theory of the "proof-plane" (above) he again finds that theory "very exactly conforms with experiment!"

18) Seventh Memoir (1789) "On magnetism". Mémoires (1884). pp. 273-318.

19) See for example W. J. King: The Quantification of the Concepts of Electric Charge and Electric Current. The Natural Philosopher, 2, p. 105 (1963).

20) This is a brief reference to the one-fluid theory of Aepinus (or "Oepinus", in Coulomb's reference). By contrast in his writings on magnetism, especially the earlier ones, there are numerous references to Musschenbroik's treatise on the subject, and scattered references to the work Aepinus, Le Monnier and Van Swinden.



Coulomb

## III. The Significance of Coulomb's Work in the Development of Electricity

Although the sustaining interests of Coulomb's work are mechanics, elasticity and magnetism, it is to the basic law of electrostatics, "Coulomb's Law", and to the unit of electric charge, "the Coulomb", that he has given his name. Our concern here is not with an assessment of Coulomb's own contributions to Electricity, or with any claims these may have to originality or priority. It is rather with the way in which Electricity, over a period of two centuries--from Newton to Maxwell--was transformed from the study of some bizarre phenomena on the fringe of physics to a subject occupying a central role in the subject. Coulomb stands roughly mid-way in this development; and it is of interest to examine what part the ideas he exploited and methods he developed played in this transformation of the science of electricity.

Coulomb's law is stated today typically as:

$$\vec{F}_{12} = k \cdot \frac{q_1 q_2}{|r_{12}|^2} \hat{r}_{12} ,$$

which reads as the force between two point charges ("at rest") is proportional to the product of the magnitude of the charges ($q_1 q_2$), and inversely as the square of their separation, and is directed along the line joining them ($\hat{r}_{12}$ is a unit vector in this direction). It is sometimes added that this law applies to electric charges "in vacuo" (although this is an unnecessary and misleading stipulation): and the constant k is written $1/4\pi\varepsilon_0$; $\varepsilon_0$ is the permittivity of free space, an empirical quantity.

In no real sense can one say that this law was "discovered" by Coulomb; nor can one take at face value his claim to have verified the law--as far as the inverse-square dependence is concerned--"très exactement." That the force acted along the line joining the "particles" was a tacit assumption of obvious inspiration, made by all who developed the theory of electricity along Newtonian lines. Likewise the assumption that the force was proportional to the charges was considered as self-obvious by Coulomb as well as those who preceded (and succeeded) him.* As for the qualification "in vacuo", whatever significance this has belongs to a much later development. Indeed for Coulomb, and for some of his contemporaries, the pressure of air played an essential role in preventing the charges from leaving the surface of the bodies which held them!

Coulomb, like other investigators in the two or three decades preceeding him, assumed the inverse-square law, and showed that the results of many of his measurements were roughly consistent with this law. Amongst these measurements his was the most direct, albeit not the most precise, demonstration of the law of inverse-squares.

Coulomb's measurements have not infrequently been assessed as the first quantitative examination of electricity, and indeed "Coulomb's Law" as providing, essentailly, the definition of electric charge; but this is an untenable position. Long before Coulomb's work, a fundamental law, and one so characteristic of electricity, had already been proposed - by Franklin - and generally accepted. It is the Law of Conservation of Charge: and it provided a basis - in principle and practice - for comparing charges. To be sure the techniques of gauging the absence , presence or amount of electrical charge appear crude - shocks and sparks, but in the hands of the skilled (Franklin and Cavendish, for example) could lead to quantitative inferences of remarkable accuracy and generality. The law-of-force, insofar as it involves the proportionality to the product of the electric charges, must be shown to be consistent at least with the Law of Conservation. This is neither logically nor experimentally trivial; but nor is it impracticable. Coulomb nowhere explicitly indicates that he had addressed himself to this problem. In the parallel case of magnetism, when he enunciates the same inverse-square law, and the same dependence of the force on the product of magnetic fluid densities, he dismisses this later feature summarily: this "partie de cette proposition n'a pas besoin d'être prouvée." !# But in fact, Coulomb's later work on the distribution of charge between conductors in contact would have led to inconsistencies if the two methods of quantifying charge did not identify the same entity - or if either law were invalid! But a thorough demonstration of consistency - or the exposure of inconsistency would have required carefully chosen situations, meticulous honesty in observation and an open-mindedness about the outcome which was not always the hallmark of Coulomb's work.

*Even if one assumes that Coulomb's law is the basic measure of electric charge, the question of experimental consistency arises. For example one could check whether the force between the two charges is in any way influenced by the presence of a third. If so, proportionality would not be possible.

# Second Memoire. 1785. (Mémoires, 1884, p.130).

Experimentally, his major invention (or independent development#) was the torsion-balance, an instrument capable of measuring forces with great precision and sensitivity, and one which has since been used widely in all sorts of experimentation. There is no question of its technical superiority over other instruments that were used in Coulomb's day - as far as its mechanical features were concerned. But, as we have seen, even if mechanical precision is attainable, this does not, ipso facto, guarantee significant precision in an electrical experiment - for it is just as essential for the electrical situation to be precisely defined as for the mechanical forces to be precisely determined. It is in their "electrical" features that Coulomb's experiments exhibit their serious limitations. There is abundant evidence of Coulomb's mechanical ingenuity, inventiveness and vituosity, compared with which his electric techniques and analysis are relatively unsophisticated. The most original of his electrical innovations is, perhaps, the "proof-plane" used to sample charge distributions; but this, although of considerable value in demonstrating, semi-quantitatively, the implications of electrical principles, is not the precursor of the methods of electrical measurement which were destined to play a major role in the development of physics.

Torsion-balances (and the method of oscillations),although capable of quantitative deployment, suffer the limitation that they are only capable of measuring the total charge, and that on conductors of simple shape (spherical) or small dimensions, and when these are far removed from other bodies. They are not particularly suited for use as "electrometers", i.e. instruments which can register the state of electrification of some arbitrary body, by means of a suitable electrical "connection". Indeed this type of measurement requires the development of the new concepts of an (ideal) conducting wire; of electrical potential; and of capacity. And it is only when these notions were developed - many decades later - that precise electrical measurement, as an integral part of some investigation, becomes a regular feature of physical experimentation. (It is of course the equivalent of these very concepts - a wire, potential, and capacity [in his terminology "a canal", "the degree of electrification", and "globular inches"] that Cavendish had earlier introduced into electricity; but his failure to publish his experimental work, which demonstrated the great power of these concepts, must have deprived his extraordinarily advanced ideas of much of their influence at the time.)

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#John Michell had earlier developed the same instruments for measuring the very small force between gravitating bodies. His ideas were communicated to Henry Cavendish, who later (1792) exploited them brilliantly in his famous measurement of the gravitational constant.

Despite these limitations, a refined version of Coulomb's torsion balance was used (in conjunction with an electrometer) as a means of providing an absolute standard of charge, as late as 1854, in a fundamental investigation by Weber and Kohlrausch. Coulomb's detailed measurements of charge distribution were also exploited a couple of decades later by Poisson - in his exemplification of the more powerful mathematical-analytical tools being developed to deal with electric and magnetic fields, and similar problems.

The contrast between Coulomb's methods and the earlier work of Cavendish has been remarked on by Maxwell:

> "It is remarkable that none of Coulomb's experiments coincide with any of Cavendish's. The method by which Coulomb made direct measurements of the electrical force at different distances, and that by which he compared the surface charges on different parts of conductors are entirely his own, and were not anticipated by Cavendish. On the other hand the very idea of capacity of conductor, as a subject of investigation is entirely due to Cavendish, and nothing equivalent to it is to be found in the Memoires of Coulomb." *

One might add that Coulomb never used the concept of a "wire" electrically (to Coulomb a wire was a torsional suspension, to Cavendish an electrical "canal"!), without which the concept of "capacity" can hardly be exploited. Thus we may contrast two problems, both concerning charge distribution - the first (a) two spheres connected by a long "wire", investigated by Cavendish; the second (b) two spheres in contact (Coulomb).


These examples typify the differences in the two approaches, and the seminal value of the concepts they embody.

The influence of Coulomb's work - and it was certainly considerable - may be attributed as much to the particular time it was done as to the originality of its discoveries or the fertility of its ideas. William Snow Harris - who himself made important contributions to the subject - writing in about 1840 - makes the fair comment that:

> "Cavendish had really anticipated all the great facts in common electricity which were subsequently made known to to scientific world through the investigations of the celebrated Coulomb and other (French) philosophers..."

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* The Electrical Researches of Henry Cavendish. Ed. J. C. Maxwell. pp. ix/x. (Cambridge, 1879).

Why, one may ask, then, was Cavendish's (published) work apparently far less influential than Coulomb's, published a decade or two later?

In Cavendish's day, Electricity was still largely in the hands of amateurs and experimentalists: the style of its investigators was almost exclusively descriptive and qualitative. Virtually in one stroke Cavendish (only in the less abstruse matters had he been anticipated by Aepinus) transformed the subject into a formal-mathematical system; which, had Cavendish's ideas been widely accepted, would have removed the subject far beyond the level of most of its practitioners! Also in Cavendish's day the mathematical-philosophers, who might have been capable of appreciating the formal subtleties of his work, displayed little interest in what they imagined to be unintelligible complexities of electricity. It was left, in a sense, for Coulomb to "popularize" the quantitative-analytical approach to electricity which Aepinus and Cavendish had founded.*

Cavendish's 1771 paper is written in a highly condensed, austere, logical style, patterned after parts of Newton's Principia (and about as difficult to read!). By contrast, Coulomb's formal arguments are interspersed amongst a great deal of less strenous matter - practical details of apparatus, tables of results, speculative remards and comments, which make his memoirs far less forbidding than Cavendish's paper. The problems of electricity are contained in a milieu of much more familiar mechanical ideas. Indeed one might say that Coulomb brought electricity into the familiar terrain of mechanics - rather than introducing mechanics, as and when necessary, into the domain of electricity. Coulomb's memoirs are liberally sprinkled with mathematical calculations and arguments, but these are mostly of a computational variety; nowhere does he seriously attempt, or attain, the subtlety and logical abstraction that characterizes the whole sequence of Cavendish's argument. And in any case the memoirs, containing as they do so much that is descriptive and empirical - could be read, and their main conclusions appreciated, even if the

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* Some 19th century writers, e.g. William Whewell, portray Cavendish-Aepinus and Coulomb as the developers of alternative, rival, theories (the one-fluid and two-fluid models). This distinction is not, however, the really significant one. Aepinus, to be sure, writes when the one- and two-fluid controversies are still being bruited around; but by the time of Cavendish, and later Coulomb, this issue has lost much of its force. Cavendish adopts the language of the one-fluid model, and Coulomb periodically avers his preference for the two-fluid, but in all the substantial work of the two authors - as Coulomb himself notices - the results can be "interpreted" within the framework of either model. This element in the difference between Cavendish's and Coulomb's approaches is only a superficial one.

mathematics were skipped! In the event, Coulomb seems to have convinced many of his contemporaries of the viability of an analytical-mathematical approach to electricity without requiring them to penetrate all its subtleties.

There is not a little misunderstanding amongst historians of science about the use of mathematics for computation, and the formulation of a general quantitative principle as a basis of electrical theory. For example C. Singer (A Short History of Scientific Ideas, p. 354)gives the grossly exaggerated description of Coulomb as "the founder of the mathematical theory of these subjects" (electricity and magnetism); and William Whewell, a century earlier (1848) asserts that "the Coulombian theory is probably assented to by all who have examined it, at least in giving the laws of the phenomena" (History of the Inductive Sciences, Vol. 2. Bk. XI, Chap. II). What laws? The law of conservation was established earlier, the law of inverse-squares was essentially adopted, and the principle that no force acts on the mobile electric charges inside a conductor was already well known and exploited. Such general theoretical principles as Coulomb attempts to develop from these laws - for example that charge must reside on the surface of a conductor - bear a striking similarity to the earlier and much more profound analysis of Cavendish; except that in attempting to give some of the subtler theorems Coulomb shows that he appreciates the significance of the results without grasping the subtlety of the argument. Where he attempts to extend or apply some of Cavendish's theorems, for example in his "theory" of the proof-plane, Coulomb's errors are striking! Nor are his basic "philosophic" concepts on electricity particularly perceptive. Thus in support of his preference for "two-fluid" theory he writes:

> "The supposition of two fluids is moreover in accord with all those discoveries of modern chemists and physicists, which have made known to us various pairs of gases whose elasticity is destroyed by their admixture in certain proportions - an effect which could not take place without something equivalent to a repulsion between parts of the same gas, which is the cause of its elasticity, and an attraction between the parts of different gases, which accounts for the loss of elasticity or combination."*

This analogy is interesting, but hardly relevant to the issue of one- or two-fluids, where the question revolves around whether, in "ordinary" electrical phenomena, one or two "fluids" or "substances" are mobile, and not whether one or two such substances exist.

In sum Coulomb brought to electricity a direct, practical, quantitative (both mathematical and experimental) - rather than a "philosophical"

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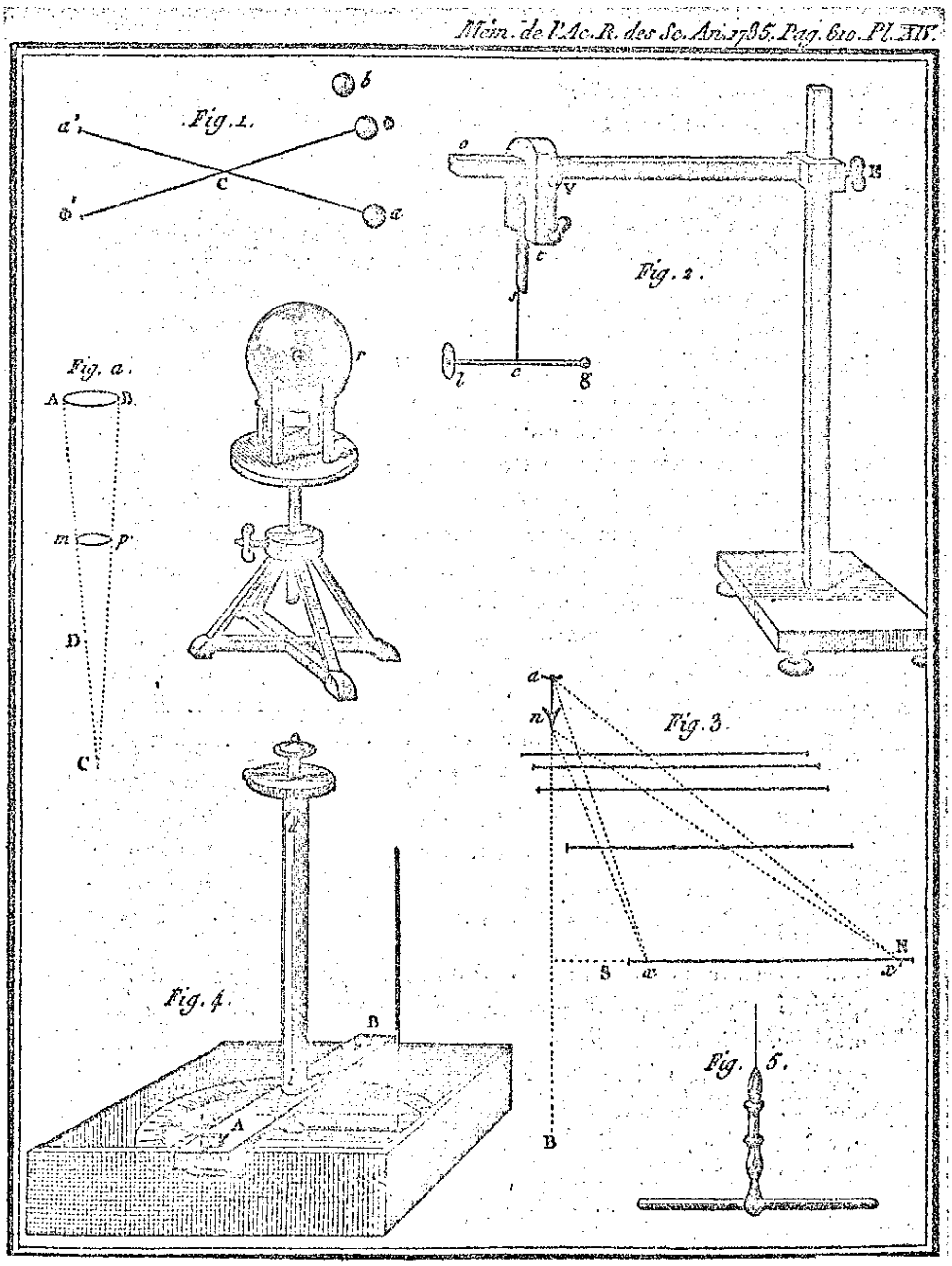
* Sixth Memoire. § XL. (1788). (Ref. 15 of p. 21; p.251.).

approach. One is tempted to say an "engineering" approach; after all Coulomb was primarily an engineer! He certainly played a very important part in establishing the rational, quantitative theory of electricity, but in the role of midwife rather than progenitor!



By the end of the eighteenth century one can discern two main lines of development of electrical science: the formal-mathematical and the exploratory-experimental. The simple phenomena resulting from the gross separation of electricity ("Macroscopic classical electrostatics" in today's terminology) were sufficiently well understood in principle--and with Coulomb's work the principles were well confirmed--to provide the basis for a formal theory. What was still lacking was the requisite mathematical apparatus to analyze any but the most simple situations. Thus a great deal of the experimental data accumulated by Coulomb could be interpreted by him only very crudely; it was subject to precise analysis by Poisson some 25 years later (1811). Even then the theoretical-mathematical concepts used were still quite rudimentary. But over the next few decades, deriving their inspiration from the analytical methods initiated by La Place in the treatment of planetary bodies, Poisson, Green, Gauss, W. Thomson (Kelvin) and others, refined the treatment of electrostatics (and other "field theories"--of gravitation, magnetism, etc.),until by the mid-nineteenth century there existed a quite sophisticated--both conceptually and mathematically--formal theory, capable of handling a great variety of problems. Especially important was the introduction of the concept of potential, and its relation to force or "field"; this in turn made possible the clear concept of electrical "capacity" and the relationship between quantity of charge, potential (difference), and electric field (force). Physically, all this is derived from no more than the basic inverse-square-law of force (with both attraction and repulsion), the principle of charge-conservation , and some simple idealized properties of conductors and insulators; but the realization of what even these few simple notions implied, was no trivial matter. Coulomb attempted to exploit and develop them in a direct unsophisticated way; in Cavendish's ideas and experiments we see in embryo most of the later and subtler concepts; but only after many decades and the exertions of many was the power and range of these concepts fully realized. This realization was important not only for the more skillful analysis of the elementary "electrostatic" phenomena, but equally, if less directly, for the elucidation of many phenomena undiscovered, and barely suspected by Franklin, Coulomb, Cavendish, and their contemporaries.

Far from being a complete, and completely formulated, subject epitomized in a few laws and principles, we now know that, before 1800, electricity was just beginning. The new phenomena to be discovered early in the nineteenth century: "galvanism," the "voltaic effect," electrochemistry, electromagnetism, etc. were all qualitatively so different from the older "frictional" or "ordinary" electricity, that one of the major, recurrent tasks was to establish the essentially "electrical" nature of each of these in turn; and since the actual phenomena were so different, the formal and analytical apparatus developed for ordinary electricity was only of very limited value in interpreting them. How for example does one exploit knowledge of the inverse-square-law beteween charges, to interpret the twitching of a frog's muscle, or the electricity set in circulation by the metals and "moist conductors" in a voltaic cell? In fact such electrical concepts as would be useful here were the more subtle ones of potential difference, electromotive force, current, resistance; rather than the obvious ones of charge and mechanical (ponderomotive) force. It was just because the Coulomb-Poisson approach was so direct, and appealing, for "ordinary" electricity, that it had little direct influence on--or value for--the unravelling of the new phenomena.

Let us look at one, characteristic example. Coulomb is very proud, justly so, of his ability to measure extremely small forces and thence extremely small electric charges, (small fractions of 1 e.s.u.; i.e. in the range $10^{-11}$ - $10^{-10}$ coulombs!). But to do so the charge had to be "isolated" and placed on a very small isolated object--one of the spheres in the torsion balance, where incidentally its typical "potential" would be hundreds (or thousands) of volts. But the subsequent development of electricity was not so much concerned with ideal, isolated charges--even small ones--as with the elucidation of the role of electric charge--and current--in circumstances where the quantity of charge involved could be immense, but its separation from charge of the opposite sign almost infinitesimal, or at least not "macroscopic." Forces were hidden and elusive, potential differences quite small (of the order of one volt or less), charges were not conveniently and arbitrarily isolated and located, but distributed in a subtle manner which was of the essence of the phenomena.

It is no accident then that Volta in his investigations of "contact" electricity--work which is roughly contemporaneous with Coulomb's work--derives his inspiration from Franklin, Canton, Aepinus, Cavendish; and in turn develops the new concepts of "condenser" (of electricity) and "electric tension." And at almost the precise time that Coulomb is establishing (experimentally!) his "law" that all conductors have equal "affinity" for electricity Volta is discovering the whole sequence of different "electrical affinities" of metals by studying contact electricity. Of course Coulomb, like his predecessors, is studying (with his torsion-balance), highly charged bodies (potentials of thousands of volts), where shape and size (capacity) are all important; Galvani and Volta, using the nervous-muscular system of dissected frogs, are examining minute potential differences of uncertain location and mysterious origin. It is the consequence of these latter researches that proves so momentous for the development of electricity!

With regard to the basic law of electrostatics itself--the inverse-square-law, it is certainly true that after Coulomb, or after Coulomb's time, the law seems to have been no longer questioned; indeed so confidently was it accepted that a whole superstructure is built on this foundation. But nearly 100 years later when Maxwell is editing Henry Cavendish's unpublished papers he finds that the direct experimental evidence for this law is no better than it was one hundred years earlier (see Ref. 2; 14). Maxwell himself subjects this law to a new experimental test; but it is Cavendish's not Coulomb's methods that he used. Coulomb's torsion-balance is certainly a sensitive instrument, mechanically; it can detect and measure small concentrated electric charges. But as a general electrical technique it is neither a particularly sensitive nor powerful one, simply because electricity does not usually come in such convenient neat packages. In the nineteenth century development of electricity, certainly before atomic phenomena are explored, methods of measuring sensitively and/or accurately small voltages were to prove more powerful than techniques relying on the isolation of macroscopic charges.

What Coulomb's work did possess was directness and unambiguousness; and it is in this context in connecting with an absolute comparison of the quantities of electricity, measured electrostatically and electromagnetically, that in 1850 Weber and Kohlrausch used a refined version of Coulomb's torsion-balance in a pioneer physical investigation. This is one of the few instances of such a direct electrostatic force measurement and even here its use marks a passing phase in the evolution of electricity. A decade or so later Weber and Kohlrausch's measurement is repeated by Maxwell, and already one sees more sophisticated electrical measurements, based on more sophisticated electrical concepts. The electrical concepts and methods Coulomb employed and the techniques he developed mark the end of a phase in electricity rather than the beginning of a new one. But his mechanical ideas, in particular the torsion-balance, had a lasting influence. Cavendish used a torsion-balance (developed independently by J. Michell), to measure the Universal Gravitational constant, in 1798; and throughout the whole of the nineteenth century the torsion-balance principle is used widely where small forces or torques are measured. Only in very recent times have electronic techniques tended to supplant it as a basic principle in instrumentation.

## IV. EXPERIMENTAL NOTES

Of Coulomb's electrical measurements the most celebrated are his "demonstrations of the fundamental law"; the most varied, perhaps more interesting and certainly most extensive are his measurements of the distribution of electricity on conductors. For the former he used two methods: the large torsion balance and the timing of "force" oscillations. Both of these experiments, and the apparatus used are detailed below. For the latter he used torsion balances of various sizes and shapes, and in particular very small ones. It is a relatively straightforward matter to construct such instruments, and it is instructive to reproduce some of Coulomb's techniques, - for example in the examination of the variations of charge density over the surface of a non-spherical conductor. It is equally instructive to examine to what extent it was possible with such techniques to verify the consistency of the Law of Conservation with "Coulomb's Laws", and specifically how the proportionality of the force to the product of the charges could have been demonstrated. No details of such experiments are given here, but they are readily obtained from Coulomb's own writings, once familiarity with the use of the electrical torsion balance has been acquired.

### A. Forces Between Repelling (Like) Charges: Large Torsion Balance.

The apparatus (see p.p 45-48 for details) is a reconstruction of the torsion balance described in Coulomb's 1st Mémoire, 1785 (see p. 13 ). It is used to compare the force of repulsion between two small electrified spheres, at various separations, with the torsional force exerted by the twisted wire. Coulomb had previously made extensive investigations of this latter force and had shown that it is proportional to the angle through which the wire is twisted (Hooke's Law); (and also to the fourth power of the diameter, and inversely to the length of the wire).

1. The wire-suspension should be perfectly straight: it should have been left with the torsion balance-arm suspended for several days before measurements are attempted. As preliminary tests one should verify (a) that the balance is properly levelled (see p. 45); (b) that the force (torque) is proportional to the angle of twist, e.g. by timing oscillations of the balance-arm of different amplitudes; (c) that the wire is truly elastic in another sense: the arm returns to the same original position after the torque is removed.

These tests can be made simply by turning the torsion-head without any electrical complications. From them one can also make an estimate of the torsional constant of the wire suspension.

2. With both fixed and moveable spheres uncharged (Devise some suitable procedure and test for this), the torsion head is adjusted so that the moveable sphere and torsion arm are at some convenient position - say 20°.

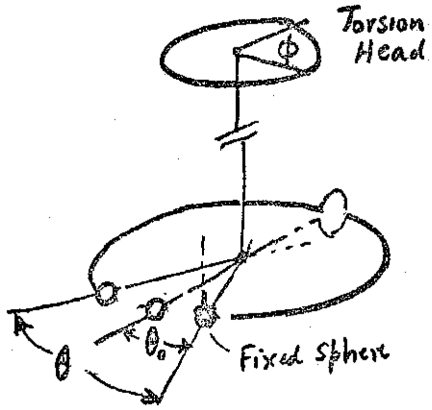
Now with the retractable charging rod in position, and with its knob in contact with the fixed sphere it is a simple matter to charge the latter. (A small Leyden jar is convenient for this purpose.). Remove the charging rod. By (gentle!) manipulation of the torsion head, the moveable and fixed spheres can now be brought into contact, so that both are - more or less equally - charged. This will be immediately evident.

3. Before proceeding to detailed measurements, a check should be made that the mode of charging used does deposit all the charge on the sphere, and none on the supporting stem (This can be done by showing that the fixed sphere can be readily discharged.).

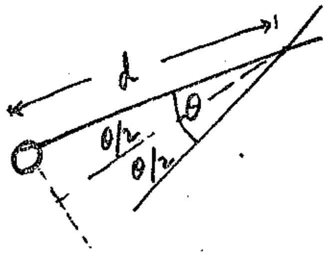
4. (a) The position of the moveable sphere (uncharged) and of the torsion head corresponding to zero torque is carefully noted, (§ 2 above). The spheres are charged (§ 3), and the torsion head is now adjusted, if necessary, to bring the sphere into a well defined and carefully measured position - of both sphere and torsion head.

(b) The torsion head is again adjusted to bring the spheres closer together, and the measurements repeated as in (a). Measurements should be made for several positions until the spheres are about one diameter or so apart. This set of measurements should be followed directly by a similar sequence with the separation of the spheres progressively larger, until the original separation is reached. As far as possible all these measurements should be made at regular time-intervals, and the times of the observations recorded. By this procedure of making the observations in one sequence, reversing the sequence, and then by appropriate averaging, the "errors" due to charge leakage can be largely eliminated. This is what Coulomb suggests should be done, but does not do! (For an alternative method see § 7 below.)

5. The torsional force is proportional to the overall angle of twist, i.e. to the angle $\phi-(\theta-\theta_0)$, where $\phi$ is angle through which the torsion head is turned, $\theta$ the angular position of the moveable arm, and $\theta_0$ its initial position - no strain in the wire. All angles are measured in the same sense. The origin for $\theta$ (and $\theta_0$) is at the position of the fixed sphere ( See Figure A).




(A)




(B)

Tabulate $\phi$, $\theta$, $\phi-(\theta-\theta_0)$, $\frac{1}{\theta^2}$ and the time of observation for all measurements. The distance apart of the spheres is approximately proportional to $\theta$, and the force between the spheres acts approximately perpendicularly to the torsion arm at fixed distance d. Hence if the inverse-square law is valid one would expect the torsion, $\propto \{\phi-(\theta-\theta_0)\}$ to be proportional to $1/\theta^2$, i.e. $\{\phi-(\theta-\theta_0)\}\times\theta^2$ to be a constant: this can be checked.

6. More precisely: (Figure B)
At position $\theta$, the torque is: F d Cos $\theta/2$. F, is force between the spheres and their separation is: 2d Sin $\theta/2$.
Then, if the inverse-square law is valid:

$$K\{\phi-(\theta-\theta_0)\} = \frac{q^2}{(2d\,\mathrm{Sin}\,\theta/2)^2}\cdot d\,\mathrm{Cos}\,\theta/2$$

( $q$ is the charge on each sphere)

or

$$\{\phi-(\theta-\theta_0)\} \propto \frac{\mathrm{Cos}\,\theta/2}{2\,\mathrm{Sin}^2\,\theta/2}$$

For small $\theta/2$

$$\frac{\mathrm{Cos}\,\theta/2}{\mathrm{Sin}^2\theta/2} \approx \frac{1-\frac{1}{2}(\theta/2)^2}{\theta^2(1-(\theta/2)^2/3!\cdots)^2} \approx \frac{1}{\theta^2}\left(1-\theta^2/24\cdots\right)$$

Thus the departure of Cos $\theta/2$ from 1 and of $(2 \text{ Sin } \theta/2)^2$ from $\theta^2$ approximately compensate each other. (Coulomb accepts this labor-saving (!) approximation as valid; rather surprisingly in view of the aspirations to "maximum precision"! Ref.(5), p. 114). For angles $\theta$ of up to say 36° (the maximum Coulomb quotes), this error, $\theta^2/24$, is less than 2%, which is certainly smaller than other approximations in the experiment.

7. Examination of the Leakage of Electricity

The spheres are charged and the torsion head adjusted to establish equilibrium with a small separation (about 10°). Now the spheres will slowly come together as the charge is lost. If the torsion head is adjusted, at regular time intervals, so as to reduce the torsion, the spheres can be held at a fixed separation.

The change in $\phi - (\theta - \theta_0)$, (only $\phi$ varies) is now a measure of the relative change in $q^2$. Assuming both charges leak away equally, then $\sqrt{\phi - (\theta - \theta_0)}$ displayed as a function of time, exhibits the rate of charge loss in each. One can now correct in any sequence of observation for charge loss by referring all measurements to a fixed (say, the initial) time. For any value of $\theta$ (and of $1/\theta^2$) one corrects the measured torque, $\propto [\phi - (\theta - \theta_0)]$, by a factor equal to the estimated (squared) charge loss. The relationship between $1/\theta^2$ (or more precisely $1/\theta^2 (1 - \theta^2/24)$) and this corrected torque can then be examined.

8. Some Questions

i) Is this torsion-balance suitable for checking that the force is proportional to one, or other, of the charges? Sketch the procedure in principle. Would a torsion balance of different dimensions be more suitable?

ii) How can you arrange to charge the spheres with electricity of opposite signs, and approximately the same quantity? Can you measure the forces with the torsion balance in this case? Try. Do you agree with Coulomb about the impracticability?

iii) As is well known a charged object can attract an uncharged one. Is this inconsistent with Coulomb's law? (As Coulomb propounded it?) Can you observe this force in the torsion balance? Can you measure it?

iv) Estimate the quantity of charge (in ESU or MKS) on the spheres in your experiments.

B. Forces Between Attracting Charges

The arrangement is essentially a replica of the apparatus described in Coulomb's 2nd Mémoire.

1. The position of the large sphere is adjusted, by eye is sufficient, so that the suspended "needle" moves along the extension of a horizontal diameter of the sphere.

2. With no charge on sphere or needle, the latter is set in oscillation, and its frequency of oscillation (if observable) measured. This is to ensure that the torsion in the suspension is negligible - or if not to provide the necessary data for making corrections for it.

3. The sphere can be conveniently charged from an electrophorus, or a small Leyden jar charged by an electrostatic machine. The needle is easily charged by bringing it fairly close to the sphere, and then momentarily touching the disc with a very fine wire mounted on the end of a stick. It is easily set in oscillation by bringing up a charged rod. (Blowing on it may impair the insulation.)

4. The period of oscillation is measured for a series of positions. The series may then be repeated in reverse sequence (as in Exp. A, § 4), and provided they are made at regular time-intervals, appropriate averaging may be made. Alternatively with the needle in a fixed position, the manner in which the oscillation frequency changes with elapsed time can be studied, and the corresponding charge-leakage correction factor determined. Notice that in this case, one certainly cannot assume that both sphere and needle lose charge (proportionately) at the same rate. The effective leakage rate should, then, be measured for a situation similar to that of the actual experiment.

5. Some Questions

i) Do you experience any difficulty in measuring the precise distance of the needle from the center (or surface) of the sphere? Why? Would it be feasible to place a scale immediately behind the needle so as to determine its position more precisely? How does Coulomb deal with this problem?

ii) Is this apparatus suitable for measuring the charge carried by a small object - say a Coulomb "proof-plane"? How does it compare with the torsion balance?

iii) Is there any measureable torque on the needle (a) when it is charged, but the sphere not; (b) when the sphere is charged and the needle not?

---

## How to Pull Single Strands of Silk from a Cocoon

Coulomb makes frequent use of single natural silk fibres taken straight from the cocoon ("jusqu'il sort du cocon"). There are modern alternatives which can be used, but a single silk strand does serve excellently for the experiment described.

1. Obtain cocoons (preferably Japanese) which can be purchased at 60¢ each from:

   Butterfly Art Jewelry Dept.
   291 East 98th Street
   Brooklyn, New York 11212 (DI2-1300)
   Mr. Glance, Proprietor

2. Boil in large pan for 2-3 hours to loosen glue that silkworms produce to attach silk to cocoon.

3. With a stiff bristled brush (a toothbrush works well) dabbed on the cocoon, a few strands will adhere to the bristles. (Do this while continuing to boil water.) Strands are exceedingly fine. Be careful not to damage while handling.

4. Gently take each strand and see if it unravels. Unfortunately, only 1 strand will do so and it may take many hours to find it!

5. When the appropriate strand is located, gently wind it around a pencil.

6. From the pencil, the correct length of silk can be cut to tie to the needle.

N.B. To keep cocoons (without hatching moths), store in refigerator. Boil in well ventilated area or under (chemistry) acid hood. Excessive inhalation of glue vapor will induce nausea and dizziness!

## Estimated Magnitudes, Corrections, etc.

A.1. From the information given by Coulomb about his (large) torsion balance - silver wire: 0.01 gm/meter, $r^2 = 3 \times 10^{-6}$ cm$^2$; length 75.8 cm; and the value for hard-drawn silver of $2.6 \times 10^{-11}$ dyne/cm$^2$ for the torsional rigidity; the couple for a twist of 360° is estimated to be 0.5 dyne cm. Coulomb gives the value, which he states is deduced from his earlier measurements of elasticity, of 1.66 dyne cm (0.153 dyne X 10.83 cm), which suggests a somewhat thicker wire; $r^2$ about $5 \times 10^{-6}$ cm$^2$. (It seems unlikely that the torsional rididity of his "silver" was three times larger!) In any event a wire of diameter 0.002 " or less is extremely fragile, and so a somewhat stronger one has been used, approximately 0.0025" diameter, with correspondingly smaller deflection angles for the same charges.

At 36°, corresponding to a separation of about 6.8 cm, the estimated force is some 0.015 dynes, giving a value of about 0.84 e.s.u. for the charge on each sphere. If the spheres were of radius 0.3 cm (Coulomb: "2 or 3 lignes de diameter"), their potential w.r.t. ground would be approximately 3 e.s.u. or 900 V, and the field at their surface 3 K.V./cm, which is far below the "breakdown level", provided the surfaces have no sharp projections!

These figures give some indication of the limitation of the torsion balance as a voltage-measuring instrument, quite apart from the problem of making proper electrical connections.

A.2. The force between two <u>spheres</u> is <u>not</u> simply proportional to the inverse-square of the distance between centers. A good next approximation is given by considering the dipole-moment induced by the charge on each sphere in the other sphere.

At distance (between centers) of r, the field $\mathcal{E} = q/r^2$ and the induced dipole-moment on a conducting sphere of radius <u>a</u> is $p = \mathcal{E} a^3 = q a^3/r^2$. <u>Attractive</u> Force on this dipole due to other charge $q$ is: $p \, \partial\mathcal{E}/\partial r = 2 \, q/r^3 \cdot q \, a^3/r^2 = 2 q^2/r^2 \cdot (a^3/r^3)$

Total (repulsive) force $= q^2/r^2 \, (1 - 4 a^3/r^3)$

At the closest distances Coulomb quotes $a/r \approx 1/5$; so the "correction" to the force is nearly 4%. This is not insignificant in relation to the precision attainable, if proper treatment is made of the charge loss. Coulomb's accuracy is much poorer. (Compare also Robison's measurements, pp. 54-57.)

B.1. In his study of attractive forces by timing oscillations, we cannot deduce from the dimensions and results given by Coulomb, the precise parameters of the experiment. From the quoted strength of the silk-fibre and the dimensions of the "needle" oscillator, we can deduce its moment-of-inertia (or at least an upper limit). If we assume this needle is charged by induction when it is in the closest position used in subsequent measurements, we can then estimate both the charge, on the sphere, $Q$ and on the needle, $q$. The result is $Q \sim 1000$ e.s.u. corresponding to a potential of about 70 e.s.u. or 20 K.V. This seems rather high, suggesting that Coulomb charged the needle when it was closer to the sphere.

In any event from the quoted oscillation frequencies one can estimate the product $qQ$ as about 2000 (e.s.u.)$^2$. From this, one can then estimate the deflection of the suspended needle, from the vertical, $\delta$ :

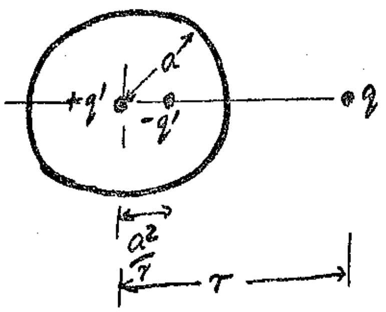
$$\delta \approx qQ/r^2 mg \qquad \text{(m is the mass of the needle, } \sim 0.2 \text{ gm)}$$

$$\sim 1/50 \qquad \text{(for r = 25 cm)}$$

Since the length of the suspension is $\sim 2$ cm, this can introduce an "error" of $\sim \frac{1}{2}$ mm in the position of the needle (if this is read directly from the upper scale), which corresponds to an "error" of $\sim \cdot 4\%$ in $1/r^2$. However the electrical distortion due to the scale at this proximity may be far from negligible!

[Incidentally, with a somewhat longer silk suspension (and a pith-ball), it would be a quite practicable matter to measure this deflection as a function of the distance from the sphere, and so verify the inverse-square law with a precision that compares favorably with the oscillation method! c.f. Robison: pp. 54-57 ]

B.2. The force between the charged sphere ($Q$) and the needle ($q$) is not simply $Qq/r^2$. The charge $q$ disturbs the charge distribution on the sphere. (There would be a force even if $Q$ were zero!) This effect is readily estimated, by calculating the electrical "image" of $q$ in the sphere. This comprises the pair of charges: $\pm q'$ at the center and at the inversion point $a^2/r$. This effect is clearly greatest when at closest separation, $r - a \ll r$. In this case the additional attractive force is approximately: $-q^2/4(r-a)^2$ ; or expressed as a ratio to the main force $qr^2/(Q \cdot 4(r-a)^2)$. Estimating that in Coulomb's experiments $q/Q$ was about 1/200, we have (for the closest



position ( $r = 9''$; $(r - a) = 3''$) , a correction of the order 1%.

The above considerations suggest the following general questions:

In both Coulomb's experiments, the force is measured not between two ideal point charges, but between two finite objects. Consider the two classes below and in each case estimate as quantitatively as you can the seriousness of the error made in treating the objects as point charges.

i) Two similar small spheres

a) both having same charge ,
b) one charge much larger than the other,
c) the limit of b: one charge zero.

ii) One large sphere, one very small object ("needle")

a) a much larger charge on the sphere than on the needle,
b) comparable charges on the two,
c) a charged needle.

## V. DETAILS OF APPARATUS

1. Drawings on the following pages (pp.45-50 ) show the apparatus built to replicate more-or-less, that described in Coulomb's first two memoirs on electricity. Success, and failure, in "electrostatic" experiments is notoriously dependent on the particular properties of the insulating materials used - not to mention the weather; but it would be foolish to attempt to follow 18th century practice in every detail - even if all their materials: gum-lac, spanish wax, silk - "tel qu'il sort du cocon", etc., could be resurrected. Instead we have used artifacts of our time, where Coulomb used those of his; without, it is believed, changing any essential features - or difficulties - of the experiments. The difference from Coulomb's materials should not however be concealed, especially where an assessment or criticism of Coulomb's work is in question.

2. Torsion Balance for Repelling Charges (Drawings A1, A2, A4, A5)

(a) The suspension used is a silver-nickel alloy wire, approximately 0.003 diameter (0.30 gms/meter). It is convenient to assemble the wire with end-fittings (See A 5), so that a replacement can be quickly made - when necessary! The wire is held (lightly) in the screw by a spot of wax or soft solder. When the screw is inserted lightly in its holder, the clamping action is completed.

(b) Fine lines are drawn, diametrically opposite each other, near the base of the glass column. These are of help in aligning the suspension so that it is central.

(c) The glass-cylinder - which has to ground off squarely, could be replaced by a lucite cylinder, which would be easier to fabricate. But care would be necessary to ensure that no charge settled on the lucite. Suitable painting of the lucite could obviate this. For the top-plate, one can - if charge is troublesome, line it with paper, which will conduct sufficiently.

(d) Measurement of the angular positions - on the external scale - is usually made by sighting along a line joining the pith-balls to the center boss of the torsion arm (A5). A fine white line or spot, marked on both pith balls and on the boss is helpful.

(e) The cylinder should be checked for reasonable circularity. The rim on the top-plate, which is machined concentric with the adaptor for the column, locates the torsion arm to the "average" center of the cylinder.

(f) Small styrafoam spheres can be used in place of pith. Painting with colloidal graphite ("aquadag") is a convenient alternative to gilding.

(g) Some drying agent may be usefully placed in the base, inside the cylinder to improve the "air-tightedness", the lower rim of the cylinder is fitted with rubber (split rubber-tubing serves well).

(h) Small constructional parts - e.g. in the torsion arm, the support for the fixed sphere (A4, A5) - may be made from dried balsa wood, which is then covered with shellac varnish (or modern counterpart). Such parts should be tested for insulation ("electrostatically") before being incorporated in the apparatus. Remember one is dealing with capacitors of $10^{-12}$ Farad and less (the small sphere), so even a resistance of $10^{12}$ Ohm results in a decay time of 1 second! Approximately $10^{15}$ Ohm is desirable, and easily attainable by proper procedures.

3. Oscillation Apparatus (Drawings B1, B2)

(a) For the large sphere a toy globe or a papier-mache sphere of reasonable sphericity can be used. The conducting surface can be either aluminum foil or a painted coating of "aquadag", rubbed down.

(b) A simple carraige mechanism enables one to move the "needle" laterally without accidentally touching the needle itself.

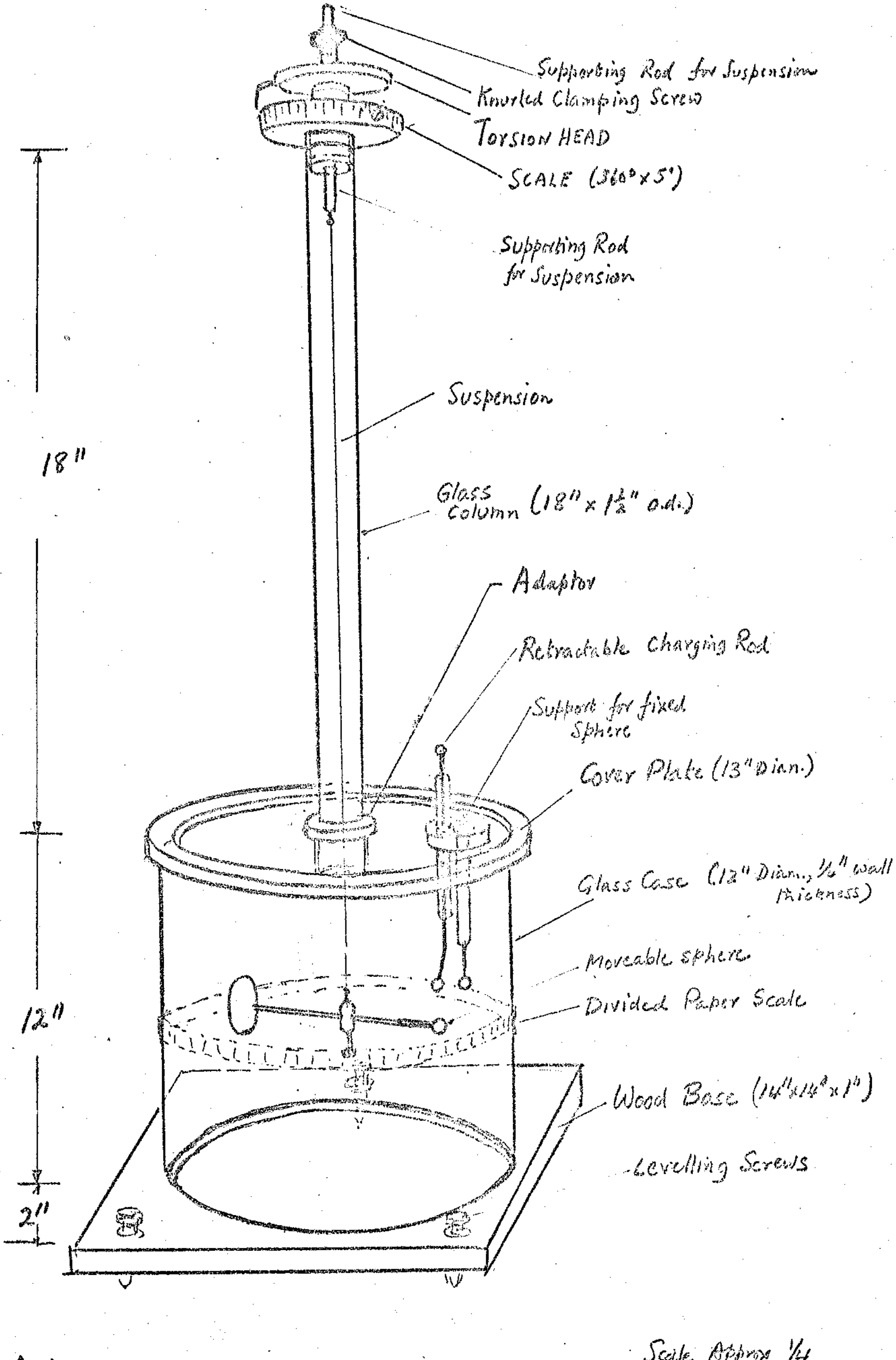
(c) The suspension for the needle is a fine (single strand) extracted from fine nylon hose. Silk threads can be used (see p. 39) but nylon is simpler!

(d) The stem of the needle must be of high-insulation material. It can be drawn from hard wax. Fine lucite rod is satisfactory.

(e) It may be helpful to shield the whole apparatus from draughts by means of a large (3'X4'X4') cloth-covered light wooden frame.

4. Miscellaneous

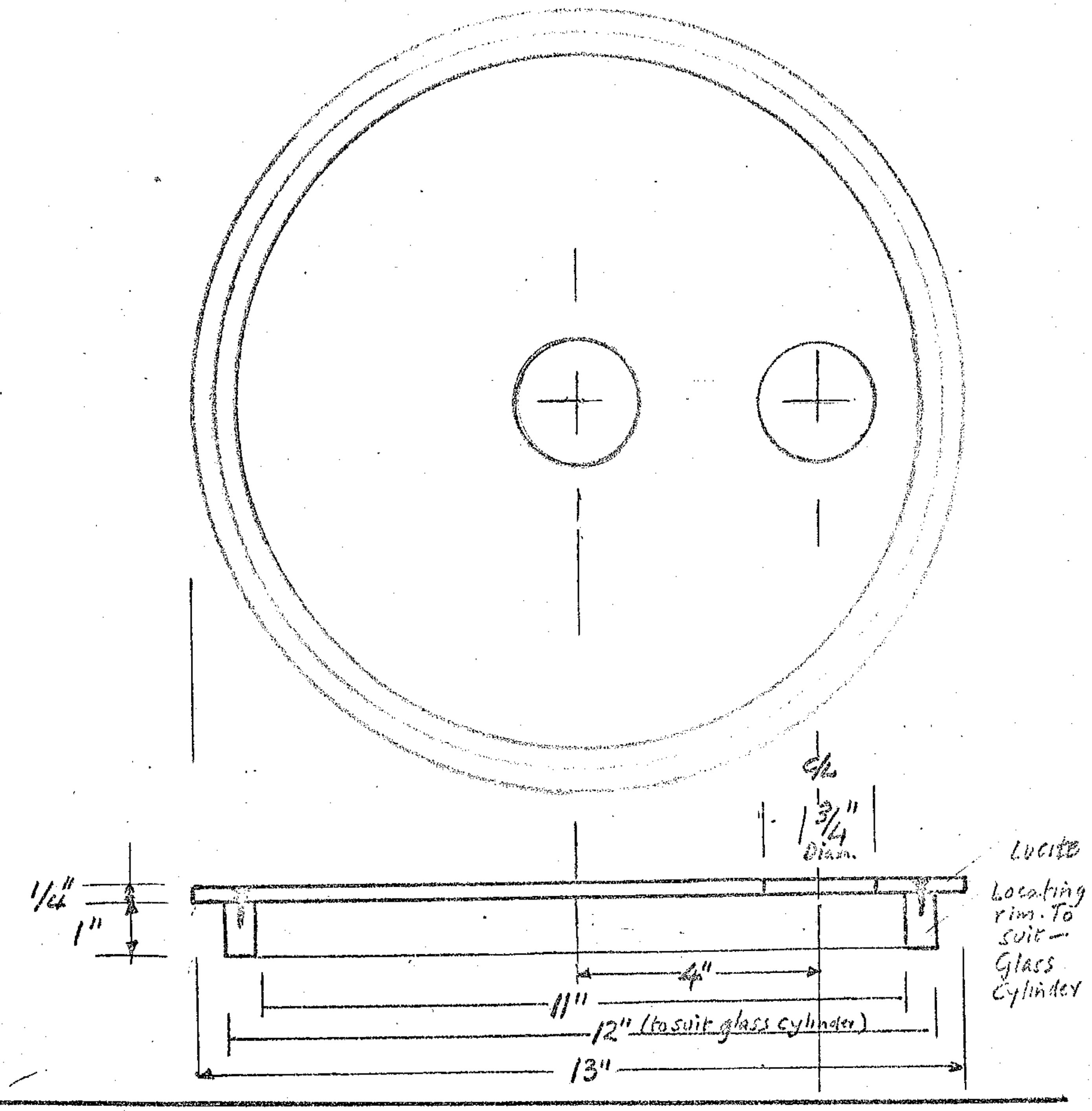
It is convenient to have available one or two small Leyden jars of different si?e. Small plastic bottles (such as used for medicines, etc.), foil-covered on the outside, coated with aquadag on the inside are suitable.




(A1.) TORSION BALANCE: Assembled

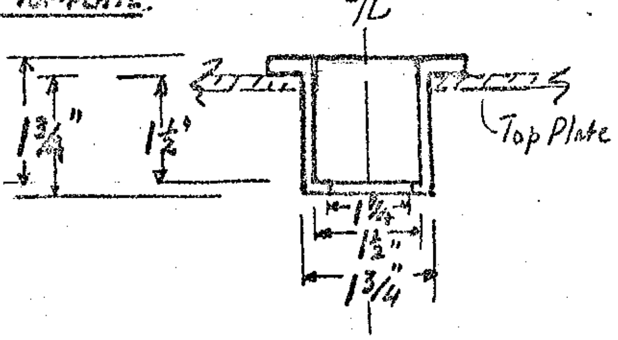
# Top Plate for Torsion BALANCE (A2)

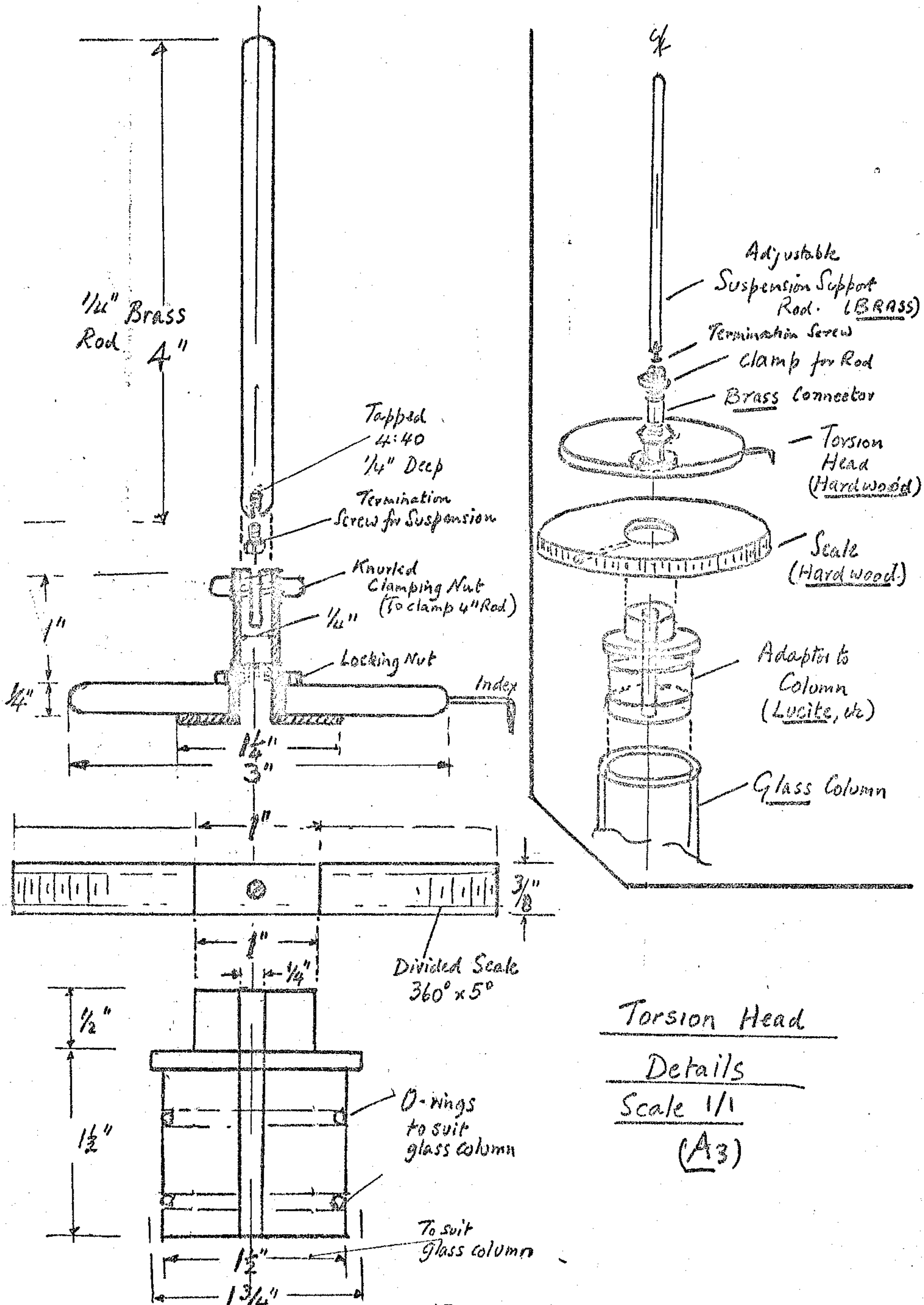
Scale 3/8




ADAPTOR for CENTRE of TOP PLATE.

Scale 1/2



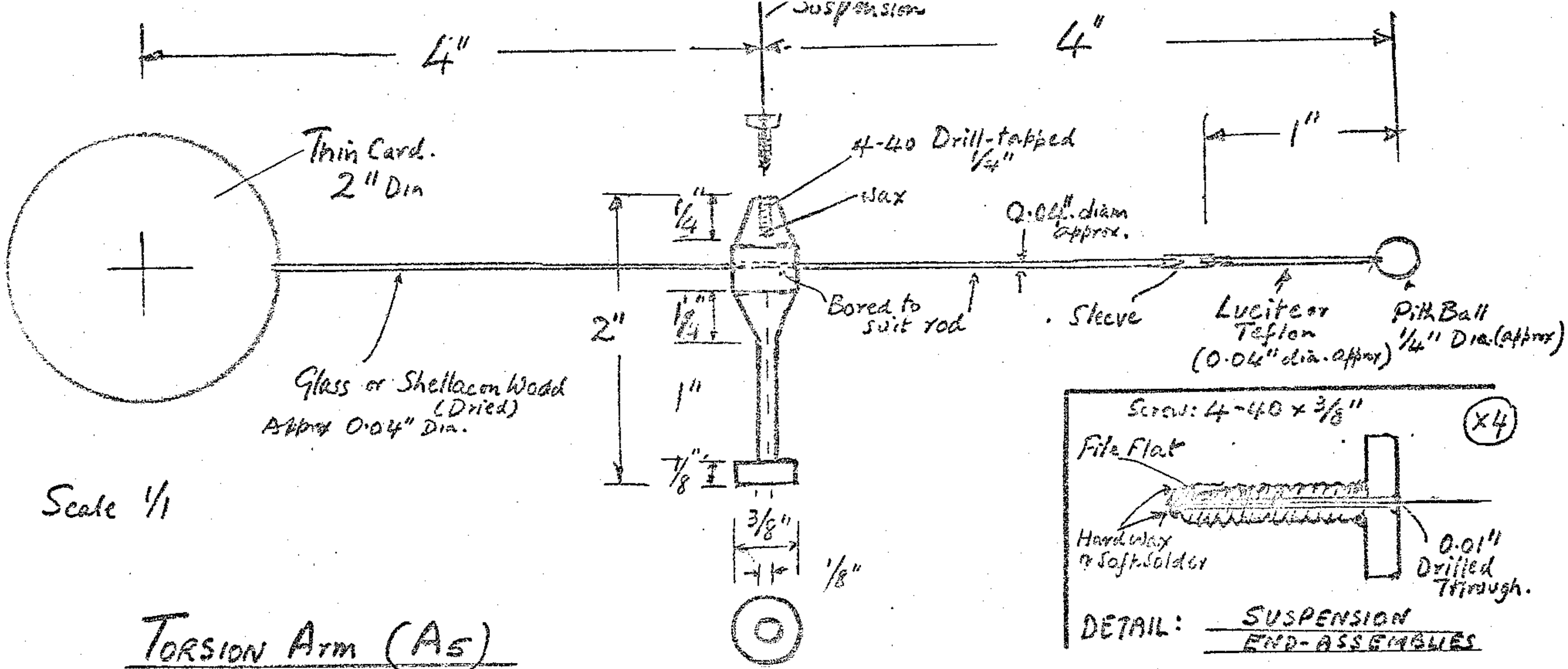



Torsion Head

Details

Scale 1/1

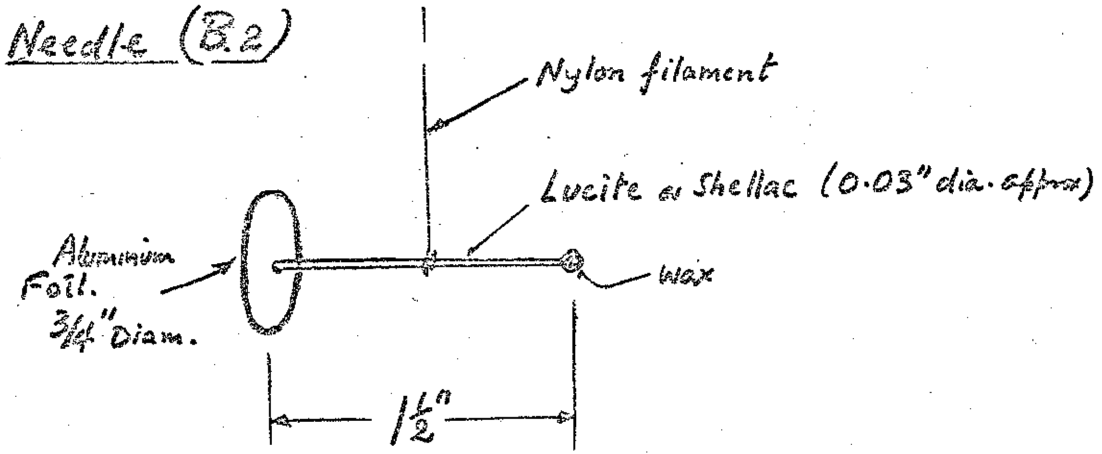
(A3)



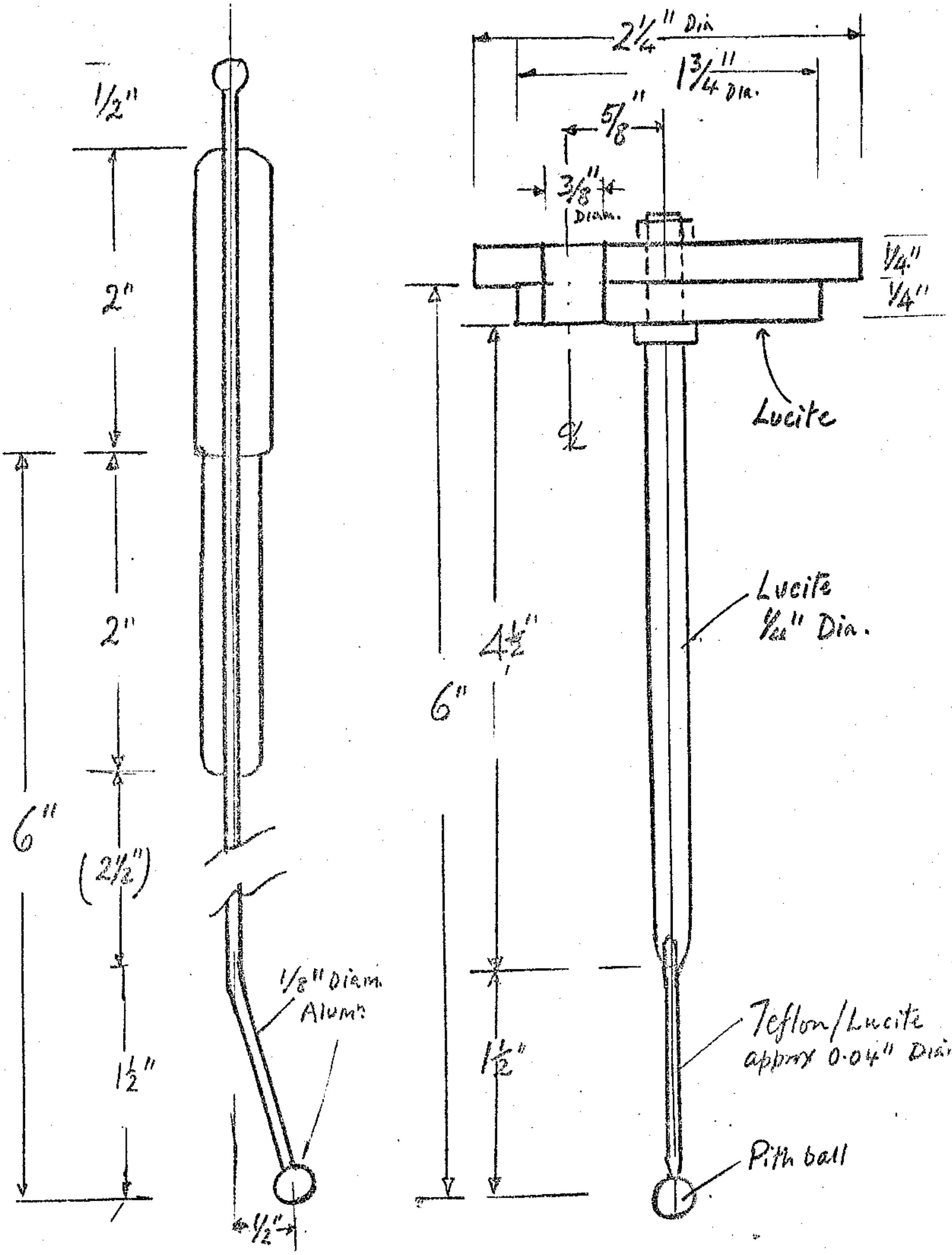

Scale 1/1

## Torsion Arm (A5)

## Oscillating Needle (B2)

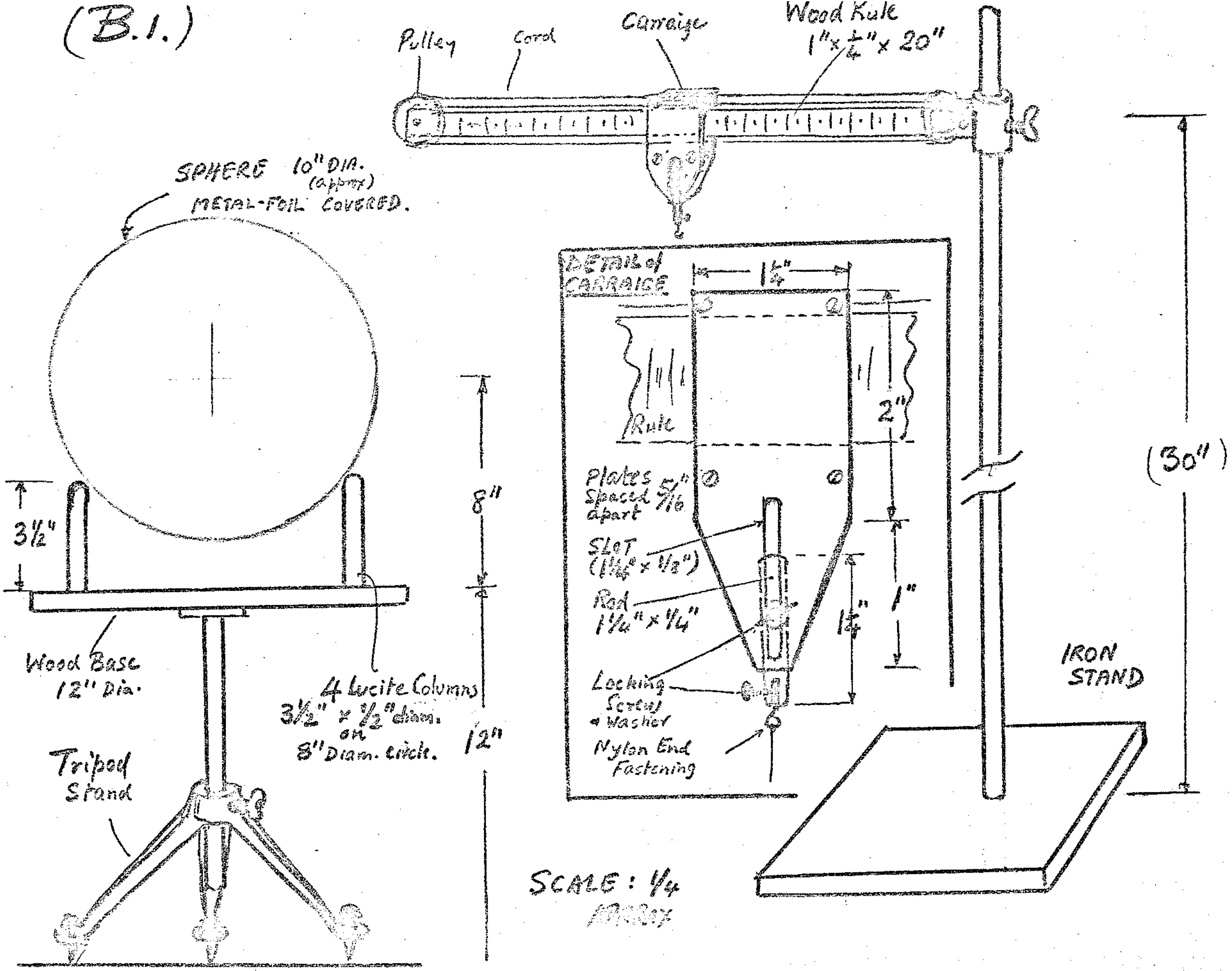



Scale 1/1




Charging Rod    Scale 1/1    Fixed Sphere

TORSION BALANCE (A.4)


(B.1.)
Pulley
Cord
Carraige
Wood Rule
1" x 1/4" x 20"
SPHERE 10" DIA. (approx)
METAL-FOIL COVERED.
DETAIL of CARRAIGE
1 1/4"
Rule
2"
Plates Spaced 5/16" apart
SLOT (1 1/4" x 1/8")
Rod 1 1/4" x 1/4"
Locking Screw & Washer
Nylon End Fastening
1 1/4"
1"
(30")
IRON STAND
3 1/2"
8"
12"
Wood Base 12" Dia.
4 Lucite Columns 3 1/2" x 1/2" diam. on 8" Diam. circle.
Tripod Stand
SCALE: 1/4

Appendix (i)

From J. Michell, A Treatise on Artificial Magnets, Cambridge, 1750.

One of the earliest statements of the inverse square law for magnetism.

Though it is not altogether neceſſary to the preſent deſign, yet it may not perhaps be amiſs, juſt to mention a few properties of Magnetical Bodies; ſome of which are very neceſſary to be known by thoſe, who have a mind to try Experiments; and for want of the knowledge of which, many experiments on this ſubject have fail'd, or wrong concluſions have been drawn from them. It will however be inconſiſtent with the brevity, I here propoſe, to give the proofs of them; which therefore I muſt defer till ſome farther opportunity offers.

*Firſt* then, Wherever any Magnetiſm is found, whether in the Magnet itſelf, or any piece of Iron, *&c.* excited by the Magnet, there



there are always found two Poles, which are generally called North and South; and the North Pole of one Magnet always attracts the South Pole, and repels the North Pole of another; and *vice verſâ.*

*Secondly*, This Attraction and Repulſion of Magnets is not at all hindered, or increaſed by the interpoſition of any Body whatſoever; though ſometimes in *appearance* it may be either, by the interpoſition of ſuch Bodies, as become Magnetical when in contact with, or upon their approach towards the Magnets, between which they are placed.

† *Thirdly*, Each Pole attracts or repels exactly equally, at equal diſtances, in every direction.

† This is a Property, which perhaps thoſe, who imagine Magnetiſm to depend upon a ſubtle fluid, may not be very willing to admit, as being utterly inconſiſtent with ſuch an Hypotheſis; but it is capable of being proved by a great variety of experiments.

The want of knowing this property of the Magnet has led ſeveral very accurate, and diligent enquirers into conſiderable miſtakes; amongſt whom was Dr. *Gilbert*, who wrote a very ingenious book, entitled *De Magnete*, about the end of Queen *Elizabeth*'s Reign. Not being aware of this property, he concluded from ſome experiments he had made, not very irrationally, that the Needle was not attracted by the Magnet, but turned into its poſition by, what he calls, a diſponent virtue; which he ſuppoſed to ſurround the Stone, ſomewhat in form of an Atmoſphere.

C *Fourthly*,

‡ *Fourthly*, The Magnetical Attraction and Repulſion are exactly equal to each other.

*Fifthly*, The Poles of Magnets are not at their Extremities, but at a little diſtance from thence; that is, Magnets are not ſo Magnetical at the Ends, as in the Middle; and in ſpring-tempered and ſoft Steel Magnets, the Poles are generally ſomewhat farther from the Extremities than in hard ones.

‡ Moſt people, who have mentioned any thing relating to this property of the Magnet, have agreed, not only that the Attraction and Repulſion of Magnets are not equal to each other, but that alſo they do not obſerve the ſame rule of increaſe and decreaſe. Their miſtake in this matter aroſe from their not attending to the different degrees of Strength, that Magnets have, in different circumſtances: for two Magnets, that are placed with their attracting Poles towards each other, will have their power by that means increaſed; and on the contrary, if their repelling Poles be placed towards each other, their power will thereby be diminiſhed: and this increaſe or diminution of power will be in a greater or leſs degree, according as the Magnets are nearer to, or farther from each other; whence in all the experiments made on this ſubject, the Attraction and Repulſion come perpetually nearer to an equality, the greater the diſtance of the two Magnets is, with which the experiments are made; and *vice verſâ*. And ſo great is the effect of Magnets on each other, that, when the repellent Poles of a large Magnet and a ſmall one are brought into contact, the ſmall one ſhall ſometimes have its Repellency changed into Attraction.





* *Sixthly*, The Attraction and Repulſion of Magnets decreaſe, as the Squares of the diſtances from the reſpective Poles increaſe.

This property, from ſome experiments I have made myſelf, and from thoſe I have ſeen of others, ſeems very probable; but I do not pretend to lay it down as certain, not having made experiments enough yet, to determine it with ſufficient exactneſs.

*Seventhly*, Magnets lift Iron, in an increaſed *ratio* of their Strength for *touching*, *&c.* and probably very nearly in a duplicate *ratio*.

* There have been ſome, who have imagined, that the decreaſe of the Magnetic Attraction and Repulſion is inverſely as the Cubes of the diſtances; others, as the Squares; and others, that it follows no certain *ratio* at all, but that it is much quicker at greater diſtances, than at ſmall ones, and that it is different in different Stones. Amongſt theſe laſt is Dr. *Brook Taylor*, and *P. Muſchenbroek*, who ſeem to have been pretty accurate in their experiments. [See *Philoſoph. Tranſ.* N°. 368 and 390. or Vol. VI. Part II. Page 253 and 255. *Eames*'s Abridgement.] The concluſions of theſe Gentlemen were drawn from their experiments, without their being aware of the *third* property of Magnets, juſt mentioned. If they had made proper allowances for that, together with the increaſe and diminution of power in the Magnets they tried their experiments with, all the irregularities, they complained of, (as far as appears from their relations of them) might very well be accounted for, and the whole of their experiments coincide with the Squares of the diſtances inverſely.

## XV. Experiments with an ELECTRIFIED CUP.

I shall close the account of my experiments with a ſmall ſet, in which, as well as in the laſt, I have little to boaſt beſides the honour of following the inſtructions of Dr. Franklin. He informed me, that he had found cork balls to be wholly unaffected by the electricity of a metal cup, within which they were held; and he deſired me to repeat and aſcertain the fact, giving me leave to make it public.

Accordingly, December the 21ſt. I electrified a tin quart veſſel, ſtanding upon a ſtool of baked wood; and obſerved, that a pair of pith balls, inſulated by being faſtened to the end of a ſtick of glaſs, and hanging intirely within the cup, ſo that no part of the threads were above the mouth of it, remained juſt where they were placed, without being in the leaſt affected by the electricity; but that, if a finger, or any conducting ſubſtance communicating with the earth, touched them, or was even preſented towards them, near the mouth of the cup, they immediately ſeparated, being attracted to the ſides; as they alſo were in raiſing them up, the moment that the threads appeared above the mouth of the cup.

If the balls had hung in the cup a conſiderable time without touching it, and they were taken out immediately after the electricity of the cup was diſcharged, they were found to have acquired no degree of electricity.

If they had touched any part of the cup, though they ſhowed no electricity while they were within it; yet, upon being taken out, they appeared to have acquired ſome; which was more if they had touched a part near the edge of the cup, leſs if they had touched any part more remote from the edge, and leaſt of all if they had touched the bottom only. If they had firſt touched the ſide near the top, and then the bottom, they came out with that ſmall degree of electricity which they would have acquired, if they had touched the bottom only.

In any caſe, if the balls were taken out while the cup remained electrified, they neceſſarily acquired ſome degree of electricity, in paſſing the mouth of the cup.

To purſue this experiment a little farther, I took a ſmall coated phial, ſuch as is repreſented upon the ſtool [*c* Pl. II.] and obſerved, that when I held it by the wire, within the electrified cup, it acquired no charge, the electricity of the cup affecting both the inſide and outſide coating alike. If the external coating touched the bottom of the cup, the phial received a very ſmall charge. If it was made to touch the ſide, it acquired a greater charge; and the nearer to the top it was held, the higher charge it received; the wire of the phial, which communicated with the inſide coating, being farther removed from the influence of the electricity of the cup.

May we not infer from this experiment, that the attraction of electricity is ſubject to the ſame laws with that of gravitation, and is therefore according to the ſquares of the diſtances; ſince it is eaſily demonſtrated, that were the earth in the form of a ſhell, a body in the inſide of it would not be attracted to one ſide more than another.

The earliest published reference to the inverse square law for electricity. From Joseph Priestley's <u>The History and Present State of Electricity</u>, Vol. I. pp. 372-375.

(1st Edition 1767, 3rd Edition 1775; Johnson reprint 1966)

Appendix (iii)

Extract from J. Robison's A System of Mechanical Philosophy. Also published in an article on "Electricity" in Supplement to the Encyclopedia of Arts and Sciences (Philadelphia, 1803).

Here Robison implies that he had measured the law-of-force between charged spheres as early as 1769. His electric balance is not as sensitive, nor, perhaps, as versatile as Coulomb's torsion balance. Yet he seems to have achieved sufficient skill in its use to observe the departure from the inverse square law with finite spheres. The contrast in tone between Robison and Coulomb is noteworthy!



The writer of this article made many experiments with this view above 30 years ago, and flatters himself that he has not been unsuccessful in his attempts. These were conducted in the most obvious and simple manner, suggested by the reasonings of Mr. Æpinus; and it was with singular pleasure that, some years after, he perused the excellent dissertation of Mr. Cavendish in the Philosophical Transactions, vol. 61. where he obtained a much fuller conviction of the truth of the conclusion which he had drawn, in a ruder way, from more familiar appearances. Mr. Cavendish, has, with singular sagacity and address, employed his mathematical knowledge in a way that opened the road to a much farther and more scientific prosecution of the discovery, if it can be called by that name. After this, Mr. Coulomb, a distinguished member of the French academy of sciences, engaged in the same research in a way still more refined; and supported his conclusions by some of the most valuable experiments that have been offered to the public. We shall now give a very brief account of this argument: and have premised these historical remarks; because the writer, although he had established the general conclusion, and had read an account of his investigation in a public society in 1769, in which it was applied to the most remarkable facts then known in electricity, has no claim to the more elaborate proofs of the same doctrine, which are given in some of the following paragraphs. These are but an application of Mr. Cavendish's more cautious and general mathematical procedure, to the function which the writer apprehends to be sufficiently established by observation.

The most unexceptionable experiments with which we can begin, seem to be the repulsions observable between two *small* spheres. Whatever be the law of distribution of the particles in a sphere, the general action of its particles on the particles of another sphere will follow a law which will not differ much from the law of action between two particles, if the diameters of the spheres be small in proportion to their distance from each other. The investigation was therefore begun with them. But the subject required an electrometer susceptible of comparison with others, and that could exhibit absolute measures. The one employed was made in the following manner; and we give it to the public as a valuable philosophical instrument.

85. Fig. 15. represents the electrometer in front. A is a polished brass ball, $\frac{1}{4}$th of an inch in diameter. It is fixed on the point of a needle three inches long, as slender as can be had of that length. The other end of the needle passes through a ball of amber or glass, or other firm non-conducting substance, about half or three-fourths of an inch in diameter; but the end must not reach quite to the surface, although the ball is completely perforated. From this ball rises a slender glass rod FEL, three inches long from F to E, where it bends at right angles, and is continued on to L, immediately over the centre of the ball A. At L is fixed a piece of amber C, formed into two parallel cheeks, between which hangs the stalk DCB of the electrometer. This is formed by dipping a strong and dry silk thread, or fine cord, in melted sealing wax, and holding it perpendicular till it remain covered with a thin coating, and be fully penetrated by it. It must be kept extended, that it may be very straight; and it must be rendered smooth, by holding it before a clear fire. This stalk is fastened into a small cube of amber, perforated on purpose, and having fine holes drilled in two of its opposite sides. The cheeks of the piece C are wide enough to allow this cube to move freely between them, round two fine pins, which are thrust through the holes in the cheeks, and reach about half way to the stalk. The lower part of the stalk is about three inches

long, and terminates in a gilt and burnished cork-ball (or a ball of thin metal), a quarter of an inch in diameter. The upper part CD is of the same length, and passes, with some friction, through a small cork-ball. This part of the instrument is so proportioned, that when FE is perpendicular to the horizon, and DCB hangs freely, the balls B and A just touch each other. Fig. 16. gives a side perspective view of the instrument. The ball F is fixed on the end of the glass rod FI, which passes perpendicularly through the centre of a graduated circle GHO, and has a knob handle of boxwood on the farther end I. This glass rod turns stiffly, but smoothly, in the head of the pillar HK, &c. and has an index NH, which turns round it. This index is set parallel to the line LA, drawn through the centre of the fixed ball of the electrometer. The circle is divided into 360 degrees, and 0 is placed uppermost, and 90 on the right hand. Thus the index will point out the angle which LA makes with the vertical. It will be convenient to have another index, turning stiffly on the same axis, and extending a good way beyond the circle.

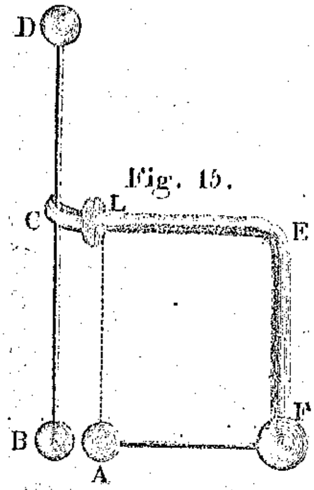


Fig. 15.

This instrument is used in the following manner: A connection is made with the body whose electricity is to be examined, by sticking the point of the connecting wire into the hole at F till it touch the end of the needle; or if we would merely electrify the balls A and B, and then leave them insulated, we have only to touch one of them with an electrified body. Now take hold of the handle I, and turn it to the right till the index reach 90. In this position, the line LA is horizontal, and so is CB; and the moveable ball B is resting on A, and is carried by it. Now electrify the balls, and gently turn the handle backwards, bringing the index back towards 0, &c. noticing carefully the two balls. It will happen that, in some particular position of the index, they will be observed to separate. Bring them together again, and again cause them to separate, till the exact position at separation is ascertained. This will shew their repulsive force in contact, or at the distance of their centres, equal to the sum of their radii. Having determined this point, turn the instrument still more toward the vertical position. The balls will now separate more and more. Let an assistant turn the long index so as to make it parallel to the stalk of the electrometer, by making the one hide the other from his view. The mathematical reader will see that this electrometer has the properties ascribed to it. It will give absolute measures: for by poizing the stalk, by laying some grains weight on the cork-ball D, till it becomes horizontal and perfectly balanced, and computing for the proportional lengths of BC and DC, we know exactly the number of grains with which the balls must repel each other (when the stalk is in a horizontal position), in order *merely to separate*. Then a very simple computation will tell us the grains of repulsion when they separate in any oblique position of the stalk; and another computation, by the resolution of forces, will shew us the repulsion exerted between them when AL is oblique, and BC makes any given angle with it.

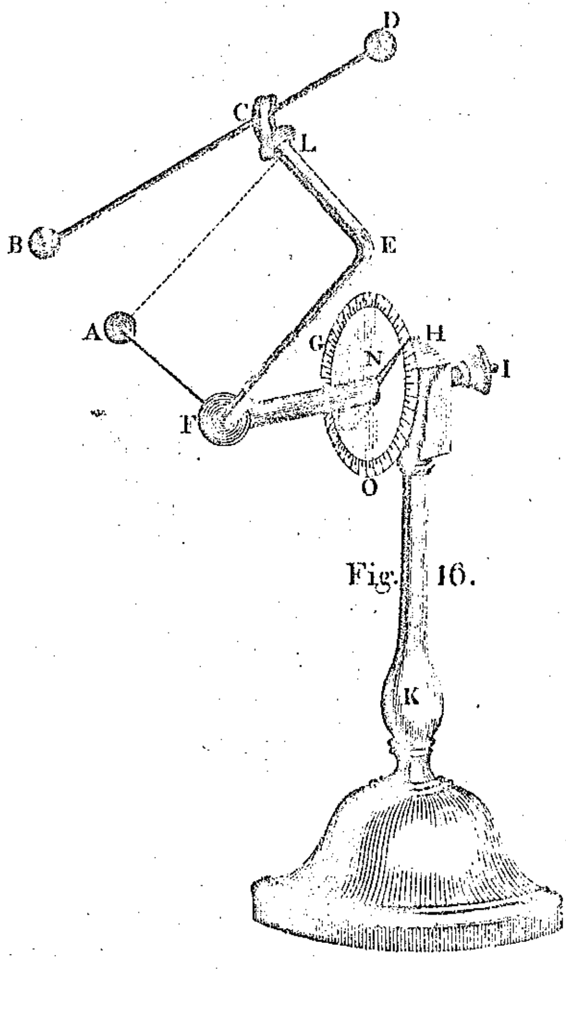


Fig. 16.

All this is too obvious to need any farther explanation. The reason for giving the connection between A and C such a circuitous form, was to avoid all action between the fixed and the moveable part of the electrometer, except what is exerted between the two balls A and B. The needle AF, indeed, may act a little, and might have been avoided, by making the horizontal axis FI to join with A: but as it was wanted to make the instrument of more general use, and frequently to connect it with an electrical machine, a battery, or a large body, no mode of connection offered itself which would not have been more faulty in this respect. The neatest and most compendious form would have been to attach the axis FI to C, and to make CA and CB stiff metalline wires, in the same manner as Mr. Brookes's electrometer is made. But as the whole of their lengths would have acted, this construction would have been very improper in the investigation of the law of electric repulsion. As it now stands, we imagine that it has considerable advantages over Mr. Brookes's construction; and also over Mr. De Luc's comparable electrometer, described in his Essays on Meteorology. It has even advantages over Mr. Coulomb's incomparably more delicate electrometer, which is sensible, and *can measure* repulsions which do not exceed the 50,000 of a grain; for the instrument which we have described will measure the *attractions* of the oppositely electrified bodies; a thing which Mr. Coulomb could not do without a great circuit of experiments. For instead of making the ball B *above* A, by inclining the instrument to the right hand, we may incline it to the left; and then, by electrifying one of the balls positively, and the other negatively, when at a great distance from each other, their mutual attraction will cause them to approach; CB will deviate from the vertical toward A; and we can compute the force by means of this deviation.

We must remind the person who would make observations with this instrument, that every part of it must be secured against dissipation as much as possible, by varnishing all its parts, by having all angles, points, and roughnesses removed, and by choosing a dry state of the air, and a warm room; and, because it is impossible to prevent dissipation altogether, we must make a previous course of experiments, in a variety of circumstances, in order to determine the diminution per minute corresponding to the circumstances of the experiments that are to be made with further views.

We trust that the reader will accept of this particular account of an instrument which promises to be of considerable service to the curious naturalist; and we now proceed with an account of the conclusions which have been drawn from observations made with it.

Here we could give a particular narration of some of the experiments, and the computations made from them; but we omit this, because it is really unnecessary. It suffices to say, that the writer has made many hundreds, with different instruments, of different sizes, some of them with balls of an inch diameter, and radii of 18 inches. Their coincidence with each other was far beyond his expectation, and he has not one in his notes which deviate from the medium $\frac{1}{5}$ of the whole force, and but few that have deviated $\frac{1}{12}$. The deviations were as frequently in excess as in defect. His custom was to measure all the forces by a linear scale, and express them by straight lines erected as ordinates to a base, on which he set off the distances from a fixed point; he then drew the most regular curve that he could through the summits of these ordinates. This method shews, in the most palpable manner, the coincidence or irregularity of the experiments.

86. The result of the whole was, that the mutual repulsion of two spheres, electrified positively or negatively, was very nearly in the inverse proportion of the squares of the distances of their centres, or rather in a proportion somewhat greater, approaching to $\frac{1}{x^{2.06}}$. No difference was observed although one of the spheres was much larger than the other; and this circumstance enables us to make a considerable improvement on the electrometer. Let the ball A be made an inch in diameter, while B is but $\frac{1}{4}$ of an inch. This greatly diminishes the proportion of the irregular actions of the rest of the apparatus to the whole force, and also diminishes the dissipation when the general intensity is the same.

87. When the experiments were repeated with balls having opposite electricities, and which therefore attracted each other, the results were not altogether so regular, and a few irregularities amounted to $\frac{1}{6}$ of the whole; but these anomalies were as often on one side of the medium as on the other. This series of experiments gave a result which deviated as little as the former (or rather less) from the inverse duplicate ratio of the distances; but the deviation was in defect as the other was in excess.

We therefore think that it may be concluded, that the action between two spheres is exactly in the inverse duplicate ratio of the distance of their centres, and that this difference between the observed attractions and repulsions is owing to some unperceived cause in the form of the experiment.

88. It must be observed also, that the attractions and repulsions, with the same density and the same distances, were, to all sense, equal, except in the forementioned anomalous experiments. The mathematical reader will see, that the above-mentioned irregularities are imperfections of experiment, and that the gradations of this function of the distances are too great to be much affected by such small anomalies. The indication of the law is precise enough to make it worth while to adopt it, in the mean time, *as a hypothesis*, and then to select, with judgment, some legitimate consequences which will admit of an exact comparison with experiment, on so large a scale, that the unavoidable errors of observation shall bear but an insignificant proportion to the whole quantity. We shall attempt this: and it is peculiarly fortunate, that this observed law of action between two spheres gives the most easy access to the law of action between the particles which compose them; for Sir Isaac Newton has demonstrated (and it is one of his most precious theorems,) that if the particles of matter act on each other with a force which varies in the inverse duplicate ratio of the distances, then spheres, consisting of such particles, and of equal density at equal distances from the centre, also act on each other with forces varying in the same proportion of the distances of their centres. He demonstrates the same thing of hollow spherical shells. He demonstrates that they act on each other with the same force as if all their matter were collected in their centres. And, lastly, he demonstrates that if the law of action between the particles be different from this, the sensible action of spheres, or of hollow spherical shells, will also be different (see *Principia*, I. Prop. 74.)

89. Therefore we may conclude, that the law of electric attraction and repulsion is similar to that of gravitation, and that each of those forces diminishes in the same proportion that the square of the distance between the particles increases. We have obtained much useful information from this discovery. We have now full confirmation of the propositions concerning the mutual action of two bodies, each overcharged at one end and undercharged at the other. Their evidence before given amounted only to a reasonable probability; but we now see, that the curve line, whose ordinates represent the forces, is really convex to the abscissa, and that $Z + z'$ is always greater than $Z' + z$; from which circumstance all the rest follows of course.

(1785)

*Deuxième méthode expérimentale pour déterminer la loi suivant laquelle un globe de 1 ou 2 pieds de diamètre attire un petit corps électrisé d'une électricité de nature différente de la sienne.*

La méthode que nous allons suivre est analogue à celle que nous avons employée dans le septième Volume des *Savants étrangers* pour déterminer la force magnétique d'une lame d'acier,

. . . . . . .

G est un globe de cuivre ou de carton, couvert d'étain, porté par quatre piliers de verre enduits de cire d'Espagne, et surmontés chacun, pour rendre l'isolement plus parfait, de quatre bâtons de cire d'Espagne de 3 à 4 pouces de longueur. Ces quatre piliers sont fixés par leur partie inférieure à un plateau que l'on place sur une petite tablette à coulisse, qui peut, ainsi que l'indique la figure, s'arrêter à la hauteur la plus commode pour l'expérience; la règle EO peut aussi, au moyen de la vis E, s'arrêter à la hauteur convenable.

Tout étant ainsi préparé, on place le globe G de manière que son diamètre horizontal $Gr$ réponde au centre de la plaque $l$, qui en est éloignée de quelques pouces. On donne une étincelle électrique au globe au moyen de la bouteille de Leyde, on présente un corps conducteur à la plaque $l$, et l'action du globe électrisé sur le fluide électrique de la plaque non électrisée donne à cette plaque une électricité de différente nature de celle du globe, en sorte que, en retirant le corps conducteur, le globe et la plaque agissent l'un sur l'autre par attraction.

**Expérience.**

Le globe G avait 1 pied (32,48) de diamètre, la plaque $l$ avait 7 lignes (1,58), l'aiguille de gomme-laque $lg$ 15 lignes (3,38) de longueur; le fil de suspension $sc$ était une soie telle qu'elle sort du cocon, de 8 lignes (1,80) de longueur : lorsque la poupée était au point O, la plaque $l$ touchait le globe en $r$ et, à mesure que l'on éloignait la poupée vers E, la plaque s'éloignait du centre du globe de la quantité donnée par les divisions 0, 3, 6, 9, 12 pouces, et le globe étant électrisé d'une électricité appelée *électricité positive*, la plaque de l'électricité négative par le procédé indiqué, on a eu :

*Premier essai.* — La plaque $l$, placée à 3 pouces (8,12) de distance de la surface du globe ou à 9 pouces (24,36) de son centre, a donné 15 oscillations en 20ˢ.

*Deuxième essai.* — La plaque $l$ éloignée de 18 pouces (48,72) du centre du globe, on a eu 15 oscillations en 40ˢ.

*Troisième essai.* — La plaque $l$ éloignée de 24 pouces (64,97) du centre du globe, on a eu 15 oscillations en 60ˢ.

*Explication et résultat de cette expérience.*

Quand tous les points d'une surface sphérique agissent par une force attractive ou répulsive en raison inverse du carré des distances sur un point placé à une distance quelconque de cette surface, on sait que l'action est la même que si toute la surface sphérique était concentrée au centre de la sphère.

Mais, comme dans notre expérience la plaque $l$ n'a que 7 lignes de diamètre et que dans les essais sa moindre distance au centre de la sphère a été de 9 pouces, on peut, sans erreur sensible, supposer toutes les lignes qui vont du centre de la sphère à un point de la plaque, parallèles et égales; et, par conséquent, l'action totale de la plaque peut être supposée réunie à son centre ainsi que l'action du globe; en sorte que, dans les petites oscillations de l'aiguille, l'action qui fait osciller l'aiguille sera une quantité constante pour une distance donnée et agira suivant la direction qui joint les deux centres. Ainsi, si l'on nomme $\varphi$ la force, T le temps d'un certain nombre d'oscillations, on aura T proportionnel à $\frac{1}{\sqrt{\varphi}}$; mais, si $d$ est la distance $Gl$ du centre du globe au centre de la plaque, et que les forces attractives soient proportionnelles à l'inverse du carré des distances ou à $\frac{1}{d^2}$, il en résultera que T sera proportionnel à $d$ ou à la distance; en sorte que, en faisant dans nos essais varier la distance, le temps d'un même nombre d'oscillations a dû être comme la distance du centre de la plaque au centre du globe. Comparons cette théorie avec l'expérience.

| | Distance des centres. | Durée de 15 oscillations. |
|---|---|---|
| *Premier essai*........ | 9ᵖ | 20ˢ |
| *Deuxième essai*....... | 18 | 41 |
| *Troisième essai*...... | 24 | 60 |

Les distances sont ici comme les nombres 3, 6, 8.
Les temps d'un même nombre d'oscillations :: 20, 41, 60.
Par la théorie, ils auraient dû être :: 20, 40, 54. 53

8/3 × 20

(Appendix (iv) )

## Extract from Coulomb's 2nd Memoire 1785

Second experimental method to determine the law according to which a sphere of 1 or 2 feet diameter attracts a small body electrified with electricity of a sort opposite from its own.

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The method we shall follow is analogous to that which we have used to determine the magnetic force of a steel plate, as described in the 7th volume of "Savants etrangers".

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G is a sphere of copper or cardboard covered with tin, and supported by four columns of glass, covered in Spanish wax, and each topped by a spigot of Spanish wax 3" or 4" long, to ensure perfect insulation. The lower ends of these columns are fixed to a tray which rests on a small table which can be adjusted to a height most suited to the experiment (See fig: p. 29). The scale EO is also adjustable to a suitable height by means of the clamp E. When all is thus prepared, the sphere G is placed so that its horizontal diameter, Gr, is aligned with the center of the disc $\ell$, which is several inches away. Using a Leyden jar a small spark is given to the sphere. The disc $\ell$ is then touched by a conducting body*, and the action of the electrified sphere on the electric fluid of the unelectrified disc, imparts to this disc an electricity opposite (in "sign") to that of the sphere; so that on removing the conducting body, the sphere and the disc interact on each other attractively.

### Experiment

The sphere G had a diameter of 1 foot (32.48 cm), the disc $\ell$ a diameter of 7 "lines" (1.58 cm), and the needle of "gum-lac" $\ell$g was 15 lines long (3.38 cm). The suspension filament sc was silk straight from the cocoon, 8 lines long (1.8 cm). When the carraige was at the point 0, the disc $\ell$ touched the sphere at r, and as the carraige is moved towards E, the disc moves further from the center of the sphere, and its position is given by scale-marks corresponding to 0",3",6",9" and 12". Since with the procedure indicated the sphere is electrified with so called positive and the disc with negative electricity we obtained:

First test: With the disc $\ell$ at 3" (8.12 cm) distant from the surface of the sphere, or 9" (24.36 cm) from its center, there were 15 oscillations in 20 seconds.

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* A very fine wire, at the end of a stick, held by hand. (SD)

Second test: With the disc $\ell$ at 18" (48.72 cm) from the center of the sphere there were 15 oscillations in 40 seconds.

Third test: With the disc $\ell$ at 24" (64.97 cm) from the center of the sphere, there were 15 oscillations in 60 seconds.

---

## Interpretation and Result of this Experiment

When every point of a spherical-surface acts on a point at any distance whatever from this suface, by an attractive or repulsive force which is proportional to inverse-square of the distance, one knows that the action is the same as if the whole spherical surface were concentrated at the center of the sphere. And since in our experiment the disc $\ell$ was only 7 lines in diameter, and in the measurements its closest distance to the center of the sphere was 9", one may without appreciable error, take all the lines which go from the center of the sphere to any point on the disc, to be equal and parallel. Consequently the whole action of the disc may be supposed concentrated at its center, just like that of the sphere. Thus, in the small oscillations of the needle, the force which makes it oscillate is constant for a given distance, and acts along the line joining the two centers. So, if one calls $\varphi$ the force, T the time for a particular number of oscillations, one should have proportional to $1/\sqrt{\varphi}$ ; but if d is the distance $G\ell$ between centers, and if the attractive forces are proportional to the inverse-square of the distances, or to $1/d^2$, it follows that T will be proportional to d, or the distance. So that when in our measurements we vary the distance, the time of the fixed number of oscillations should vary like the distance between centers of sphere and disc.

We compare this theory with experiment:

| | Distance Between Centers | Time for 15 Oscillations |
|---|---|---|
| First Test | 9" | 20 Seconds |
| Second Test | 18" | 41 Seconds |
| Third Test | 24" | 60 Seconds |

The distances here are in the ratio 3 : 6 : 8.
The times of the same number of oscillations :: 20 : 41 : 60.
According to theory these should be ::20 : 40 : 54.

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(Note: The figures in parenthesis are dimensions converted to centimeters. These were not in Coulomb's original memoire!)