THE SEARCH FOR ELECTROMAGNETIC INDUCTION

(1820-1831)

Contents

Samuel Devons
Barnard-Columbia History
of Physics Laboratory
June 1975

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I. Historical Notes

1. Background:(1)

Hans Christian Oersted's (1777-1851) sensational discovery of 1820 marked the end of a long history of speculation about the possible relationship between electricity and magnetism. Oersted's experiments were simple, unambiguous and decisive: not only was the relationship between the electric (Voltaic) current and magnetism established; its extraordinary form was vividly portrayed, and reported in a manner which made its speedy verification by others absurdly easy. Within days of their announcement Oersted's results were reproduced in the major scientific centers of Europe; within a few weeks or months they were rapidly extended. Quickly it was evident that not only could the electric current exert forces on a magnet, but could in fact turn a piece of (non-magnetized) iron into a magnet. (Arago, Davy). Electricity could create magnetism. Ampère, almost from the outset, went much further; magnetism was not only made by electricity - it was nothing but electricity; in some special configuration of (molecular) electric currents. Nor need these currents be, as they were for Oersted, Voltaic. It was soon demonstrated (Davy) that even the brief, intense discharges of ordinary electricity could - using Ampère's solenoid - induce magnetism in iron.

Electricity and magnetism were clearly and firmly related, but the relation was one way - electricity produced magnetism. Yet surely there must be a converse - in some way magnetism must be able to create electricity! It was a belief amply reinforced by other reciprocal relations of electricity - with chemistry and, soon to be discovered (1822), with heat (by Thomas Johann Seebeck (1770-1831)). It was a belief nourished by the prevalent philosophic faith in the interconnectedness of all of "Nature's innumerable workings". The issue was not so much whether magnetism could produce electricity, but how. There were known parallels that suggested an answer - of sorts.

"Induction" or "influence" was a common enough notion, when used to describe familiar electric and magnetic phenomena. A body charged with ordinary (electrostatic) electricity induces electrification in a nearby (conducting) body. Similarly, a magnet induces magnetism in some neighboring magnetic material (soft iron, nickel, etc.). The extension to current electricity seemed self-obvious Irrespective of any precise converse to Oersted's magnetism, the knowledge that electricity induced electricity, and magnetism induced magnetism, made the supposition that, somehow, an electric current must induce an electric current (in a nearby conductor) almost irresistable. (For the followers of Ampère induced magnetism was already such a phenomenon!) Faith in the reciprocity of electricity and magnetism, or in the universality of the induction principle was expressed by persistent attempts at experimental confirmation.

For more than a decade the leading investigators - Ampère and Faraday in particular - repeatedly made deliberate attempts to discover such effects; all with results that were negative, ambiguous or at best unconvincing The methods used were by no means unsuited to the purpose, nor were the instruments always lacking in sensitivity. Yet these experiments failed. But at the same time some remarkable new magnetic phenomena, which were "by chance" (Arago) discovered and then thoroughly examined (Christie, Herschel, Babbage, et al), were in fact blatantly displaying the very phenomena being sought: striking manifestations of electromagnetic-induction, but, incredible as it must now seem, not recognized as such. In one case the effects were strenuously sought - and were absent or not perceived; in the other they were fully manifest - and not recognized! It would be absurd to put this down simply to ineptness in one case, or blindness in the other. Some of the most perceptive minds and brillant experimenters of the time were involved. Like so many discoveries, electromagnetic induction looks so simple afterwards, if only because experiment can be arranged to make it appear so. It is certainly not difficult - even today when the basic principles are thoroughly established - to display these same phenomena in a setting where their proper interpretation can be quite challenging. When we examine the early experiments in some detail - or even better, rework them - we shall not find all that we look for neatly separated from "irrelevant" distraction; and we may then better appreciate how much experimental observation is determined by expectation, both collective and individual. And how different is the interpretation of the same experiment before and after the principle - that guides the eyes as well as the mind - has been discovered!

2. Attempts by Ampère et al:

The first to propose - and to claim to have observed - the reciprocal phenomena was Augustin Fresnel (1788-1827) in one of his rare excursions from the field of optics. In November 1820, only three months after Oersted's announcement, he reported to the Académie des Sciences in Paris that he had succeeded in decomposing water - an incontrovertible test for galvanic electricity - by means of the current generated by a helical wire with a magnet inside. Although this experiment was suggested by the already familiar magnetization of iron by a current helix, the outcome was by no means a foregone conclusion For Fresnel argued with exemplary logic (and the French "school" of physics of this period was nothing if not logical!),

"Not that such a result is a necessary consequence of the original observation, because the magnetic state of steel might, for example, be due only to a new arrangement of its molecules, or to a particular way in which an imponderable fluid is distributed, in which case the magnetic state would not be expected to be able to produce the movement that established it originally."[2]

A necessary consequence or not, the experimental results were spurious and the claim soon retracted Meanwhile, Fresnel's colleague, André Marie Ampère (1775-1836), already deeply involved in what were to be his marathon labors in electrodynamics, was encouraged by Fresnel's claim to add his own - that he too had observed, albeit unclear, signs of the same phenomenon Ampère's conviction seemed to ebb with Fresnel's, but his interest is alerted and remains. A few months later, early in 1821, after some more careful and deliberate investigations - this time based on the analogy with induction - Ampère returns a negative verdict from experiment:[3]

"The proximity of an electric current does not induce another current in a metallic conductor made of copper, even under the most favorable conditions for its influence to be made effective."

This conclusion marks the end of the first phase in Ampère's cycle of belief and disbelief in induced electric currents, and the beginning of a long period of uncertainty and vacillation, and of interest and disinterest. For whether true or not, these induced currents were not, for Ampère, a central issue His real concern was his doctrine of the essentially electrical (Voltaic) nature of magnetism, his exploration of the structure of the dynamical interaction of electricity (electrodynamic interaction of currents), and his attempt to build on this basis a comprehensive mathematical theory from which all observed phenomenon could be "deduced". This theory did not require (or predict) induced currents, although no doubt these could be encompassed within it. On the whole it might be simpler if they did not exist: there was certainly no shortage of unambiguous phenomena for the theory to explain!

But the experiments did continue. The arrangement Ampère used in 1821 (his "instrument"as he was want to designate his apparatus) was well conceived to detect any induced current, if such existed. A closed circular loop of copper was suspended by a torsionless silk thread, so as to lie in the plane of, and close to, a slightly larger fixed circle comprising many turns of insulated copper wire, through which a large voltaic current could be passed (See Fig. opposite) If current-induction occurred, then the current in the fixed coil would cause some - presumably small - current to flow in the suspended loop, and this latter could be detected by bringing up a strong magnet: the suspended loop should be moved from its position of rest. Ampère's original (1821) failure to detect such movement could then be attributed to lack of sensitivity in the "instrument" (as well as lack of any real induction!) Needless to say these experiments were conducted in the spirit of a true analogy to familiar electric and magnetic induction: i.e. a steady current in one condutor should induce a steady current in another placed nearby. There was no suggestion of any transient effects, and no hint that such were looked for or observed.

Early in the following year, 1822, in the course of a visit to Geneva, and taking advantage of the availability there of a more powerful magnet, Ampère repeated this experiment with the aid of a young Swiss collaborator, Auguste de la Rive (1801-1873). This time a positive result was obtained. It seems that the suspended ring was observed to move when the current was set up, and then returned to it original position when the current was disconnected. This was interpreted - according to an explication which Ampère gave very much later - as showing steady induced current persistently whilst the main current passed; and which brought the suspended coil to a new position of equilibrium in which the force of the magnet on the induced current was balanced by the torsion of the suspension When the main current was turned off, this torsion restored the loop to the original equilibrium. The results of this investigation were deemed interesting enough to be reported by Ampère to the Académie later in the year,[4] but not apparently of sufficient importance - or sufficiently certain - to warrant publication, at least by Ampère. There were however the other accounts - a brief one by the young De la Rive [5], which refers to

> "the effect which at first M. Ampère believed to be non-existent" but which now "has been verified by him very definitely while in Geneva."

The "effect" is characterized as the ability of conductors "not otherwise able to acquire permanent magnetism" being able to "at least acquire a sort of temporary magnetism whilst they are under the influence of the current".

Another, fuller account of this experiment was included in the text Manuel d'électricté dynamique (1823) by J. B. F. Demonferrand, an acquaintance of Ampère's. Here are to be found elements of both De la Rive's report and the unpublished, and the later (1833) elaborated version of Ampère himself. In particular, Demonferrand's version emphatically asserts that the observations showed that the induced current flowed in the same direction as the inducing current, an assertion which Ampère himself was rash enough to repeat after the true features of electromagnetic induction had been discovered by Faraday. Demonferrand's book, with the account of the Geneva experiment, was translated in

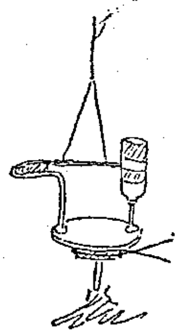
English (J. Cumming, 1827), but a more prominent, and laudatory, appraisal of the experiment and Ampère's work generally, was carried in the Quarterly Review (also 1827). Whatever faith Ampère may have had in his own experiment, here was a clear expression of faith in Ampère by his contemporaries.

The experiments begun in Paris and continued in Geneva did not end with the 1822 version. In 1825, another young member of the Geneva circle, and an associate of de la Rive, Jean Daniel Colladon (1802-1893), repeated the attempt made earlier by Fresnel to "induce" current in a helix by a magnet, but now using a galvanometer as a far more sensitive test of induced current than decomposition of water. To obviate any direct influence which moving the magnet might have on the galvanometer, this was placed some distance away in an adjacent room and connected to the helix by long wires. Unfortunately, lacking an assistant, Colladon had to move from one room to another to examine the response of the galvanometer to changes in the position of the magnet. By the time he arrived, all transient effects had, needless to say, disappeared; and not suspecting these, he naturally regarded the experiment as having failed to provide evidence for electromagnetic induction (See p. 39)

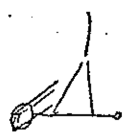
3. Attempts by Faraday

About this same time Faraday at the Royal Institution in London was also making sporadic attempts along similar lines to detect electromagnetic induction. Currents induced in wires laid close alongside separate current-carrying wires, and inside helixes, were also sought in vain (6). Stimulated by the observations of Arago and others with moving conductors (see below), Faraday also tried an arrangement in which the charged Leyden-Jar was suspended so that its two terminals (of opposite polarity) lay close above a rotating copper plate. There was no sign of any interaction (or "induced" electrification) which would cause the jar to turn with the copper plate (7). (Fig. (a)):

Marginal sketches from Faraday's Diary



(a) 1825



(b) 1828

A few years later, 1828, his diary records an attempt to observe induced currents in a manner very similar to the Geneva experiments (7). A closed ring of copper wire is counterbalanced by a small weight and the whole is mounted on a torsionless suspension (See Fig. (b)). The pole of a strong bar magnet is introduced into the ring "Supposing it might exert an influence on it; but upon bringing other magnets near to the wire, could observe no effect whatever..." . Faraday also tried rings not soldered (i.e. open circuited), and also rings of platinum and silver - in all cases no signs of induced currents. Whether he regarded these attempts as inconclusive, or of insufficient interest because of their negative outcome, Faraday did not apparently feel they warranted publication! During this period (1824-1830) electricity was not his major preoccupation.

Faraday's dramatic entry into the area of electrical research was in 1821, when he demonstrated, for the first time, the possibility of continuous rotation based on Oersted's discovery. This achievement led to a correspondence with Ampère in which, whilst ostensibly exchanging views each presented and maintained his own. In style and background and outlook the divergence between the two could hardly be greater: but both became deeply immersed in the new puzzles (for Faraday) and problems (for Ampère) of electromagnetism. Yet unlike Ampère, for whom electromagnetism became the overwhelming preoccupation, Faraday's interest and energies are, after a year or two, turned in other directions. In 1822/23 there are many entries in his Diary showing concern with different forms of electromagnetic rotation - a phenomena which seemed hard to reconcile with Ampère's theory of action <u>along</u> the line joining currents elements. But after that - apart from the ocassional entry in 1825, and again in 1828 - there is no concern with electromagnetism. For most of a decade chemistry, optics, and accoustics are the domains of Faraday's scientific activities. When he does return to the subject in 1831, it is with incredible force and resolution, as if his ideas and intentions which have germinated but remained pent-up in his mind suddenly burst forth. The contrast with his own (and others'?) earlier furtive attempts could hardly have been more dramatic.

4. "Magnetism of Rotation": Dominique Francoise Jean Arago (1756-1853), et al.

Whilst Ampère, Faraday and others were pursuing the indecisive search for 'induced' electricity, a phenomenon - the "Arago Effect" - was 'accidentally' discovered involving just such currents, but despite its thorough examination was not recognized as such for seven or eight years; until Faraday in 1831/32 recognized it as an example of his comprehensive principle of electromagnetic induction.

There are different accounts of the 'accident' leading to this discovery. Ross (Ref. I) refers to the observation by a French instrument maker, H. P. Gambey, of the damping of the oscillations of a compass needle when placed over a sheet of copper. Arago himself relates its origin to the chance observation of the increased damping of metals on a compass needle when he was engaged, in 1822, with F. H. Alexander Von Humboldt (1769-1859) in magnetic surveys at

Greenwich. In any event the first announcement of the phenomenon - as an increased damping without change of frequency of a magnet oscillating above, and close to the surface of a metal, or even a non-metal! - was made in a very brief report to the French Académie in 1824. (8) The following year (1825) Arago again reported briefly to the Académie. By 1826, when Arago gave a fuller account of his original investigation and its extension, the subject had stimulated investigations in England, Germany, Switzerland and Italy - as well as in France - and had become a lively topic of controversy; and of the inevitable rival claims for priority!

One of the most extensive inquiries into the nature of the new phenomena was made by Charles Babbage (1792-1871, of calculating-machine fame) and J. F. W. Herschel (1792-1871, the famous astronomer, then Secretary of the Royal Society), and reported in 1825, as "an imperfect and hasty note justified by the great interest", regarding, "the curious experiments of M. Arago described by M. Gay-Lussac during his visit to London in the spring of the present year". (9) By this time, presumably, Arago had observed not only the damping of the oscillations by a stationary metal, but also the deflection, and even the continuous rotation of the compass needle by a rotating copper disc placed beneath it. These effects with copper and some other metals are confirmed by Babbage and Herschel - experimenting at Babbage's home in London; and then the experiment is "reversed"; a powerful (20 lb.) magnet is rotated under a copper disc (6" diameter, 0.05" thick) freely suspended by a silk thread, and the disc is observed to deflect (or rotate) in the same sense as the magnet. Sheets of different materials - paper, glass, wood, copper, lead, etc. and tinned iron are interposed between the suspended disc and the rotating magnet. All have no influence, except the iron! Exactly as one would expect if one were observing some form of magnetism 'induced' by the magnet in the metal! But not the ordinary induced magnetism - the new dynamical effect was only observed when there was a relative motion (rotation) of the magnet and the metal. This is the view Babbage and Herschel - like others - adopted almost from the outset; And persisted with, as a guide to further experiments, for their interpretation, and as a basis for a general 'theory' of the phenomena.

They continued by measuring the deflection by the rotating magnet of suspended discs of various metals; the angle was greatest for copper, and sucessively less for zinc, tin, lead, antimony, mercury and bismuth. It was zero for non-metals - except perhaps for a small effect in carbon (from coal-gas retorts). This correlated, more or less, with the effectiveness of the materials in damping the oscillations of a compass-needle. The best conductors showed the largest "induced magnetism" ! Then - as if they were picturing some currents flowing in the disc - but they were not! - they examined the effect of cutting successively more slots in the suspended (or rotating) metal discs:

The induced magnetism was progressively suppressed. But by filling the slots with tin (or even bismuth - which displays little induced magnetism itself) the 'induced' magnetism was wholly restored. Powdered metals also showed greatly reduced effects. All this was interpreted as an inhibition by the slots, of the propagation of magnetism from one point to another in the disc: a familiar enough effect in ordinary induced magnetism. For the new dynamical induction, metallic conduction is equivalent to magnetic contact!

In an attempt at a basic theory to embrace all the phenomena they propose a principle; a

> "postulatum, viz, that in the (dynamical) induction of magnetism, time enters as an essential element,... that no finite degree of magnetic polarity can be communicated to, or taken from any body whatever susceptible of magnetism in an instant." (1)

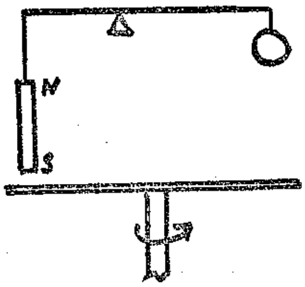
Time is required to lose, as well as to gain, magnetism It seems as if almost against their will they have hit upon the key factor in electro-magnetic induction! But, alas, it is still magnetism, *sui generis*, that they are thinking of. Their picture is of a retentive or coercive power of induced magnetism; and of a process in which magnetization consists of separation of the two magnetic fluids (Austral and Boreal) by infintesimal distances. Motion, plus the delay in magnetic fluid separation or recombination, enables these fluids to be separated by finite distances, and so to exhibit magnetic attractions and repulsions. (See Appendum, p. 17)

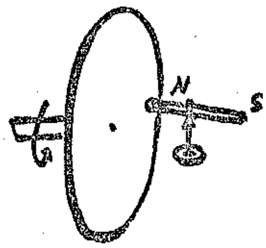
This theory, Babbage and Herschel believe, can also explain the previously reported observations of P. Barlow ((1776-1862), of "Barlow Wheel" fame) of the changes in magnetism, due to rotation, of spheres and shells of soft-iron in the earth's magnetic field. The work of Babbage and Herschel also inspired further experiments by S. H. Christie (1784-1865), an expert on magnetism, (11), using long bar magnets suspended over copper rings, an attempt to establish the law of force of this new magnetism - 1/r4 he concludes; and to his conclusion that his experiments "fully establish the truth of the principle" (of Babbage and Herschel) - although the actual value of the very small time-delays remain to be determined!

In France the investigation of the Arago effect was complicated by the resurrection and re-examination of some old (1802) observations of Coulomb that metals other than the usual iron, etc .- silver in particular - displayed weak magetic properties. Possibly, - Coulomb with his usual "scrupulous precision" had not excluded this - the effect was due to traces of iron as impurity. Biot had repeated the experiments with more care, but still left the question open. In 1824 Antoine César Becquerel (1788-1878, the first of a long line of physicists) re-examined the question, using Voltaic current carrying coils instead of permanent magnets.(12) Induced magnetism in ordinarily non-magnetic materials - peroxide of iron and copper for example - was indeed observed, but in contrast to Coulomb's experiments, the forces here seemed different in character from those with iron. Becquerel was in fact observing diamagnetism! Not surprisingly there were suggestions that in the Arago effect, this (dubious) weak magnetism of metals was enhanced. When in 1826 Arago himself reviews the whole subject, he categorically rejects any relationship to Coulomb's observation; and for good reason. The order of the metals displaying the new effect is quite different from Coulomb's order; copper, zinc, brass, tin and lead, versus lead, tin, silver, copper, gold. Meanwhile Felix Savery (1791 - 1841), also prompted by Arago's discoveries, examines whether the force of an electric current which is able to magnetize iron needles can be screened by different materials placed between the current-carrying wire and the needle. This, he does both for currents furnished by a Voltaic pile, and for rapid discharges of ordinary electricity (from a Leyden battery). The interposed metal sheets do modify the magnetism - and more so in the case of the discharge than the Voltaic circuit. For weak discharges at least, the effect is a reduction in magnetisation, and the ordering of the metals, in their effectiveness, is similar to that for Arago's damping effect. Once again a magnetic property is correlated with electrical conductivity, but the presence of induced currents is wholly unsuspected!

The experiments of T. J. Seebeck (1770-1831) pointed in the same unrecognized direction (13). In a paper entitled "On the Magnetism Excited by Induction (Verteilung) in All Metals", he reports general confirmation of the Arago damping phenomena, and a detailed examination of its magnitude for many metals; and the effect, in the case of zinc and copper, of varying the thickness of metals. Increaded thickness - up to a point - increases the damping; and the best electrical conductors are most effective. He also examines the effectiveness of a variety of "alloys" of Cu and Bi, which though each metal is separately active, show very little damping. Even an alloy of Antimony and Iron gives zero effect; but this is not surprising: solutions of iron in bismuth were known which displayed no ordinary induced magetism.

Since, in its most dramatic form, the Arago experiment exhibits a compass needle being 'dragged around' by a rotating disc, it was only to be expected that the new magnetism would be initially regarded as giving rise to an *attractive* force between moving metal and magnet - i.e. an induced magnetism of polarity opposite to that of the inducing force (The interpretation of the damping phenomena in these terms was more obscure). But there were those, including Arago himself, who questioned this 'obvious' interpretation: and supported these doubts by experiment. Indeed, whatever the dragging force might be, the *whole* of the induced force could, it was argued, have three components: which conveniently taken as tangential, radially outwards, and perpendicular to the plane of the (moving) metal. By suspending a long bar magnet vertically on a counter-balanced arm with one pole near the metal, the vertical force





on the magnet appeared repulsive! The tangential force responsible for dragging is of course attractive, but the radial force has a more complex character which depends on the location of the interaction in the disc as a whole. These results were confirmed in an extensive investigation (14) by Georg Frederick Pohl (1788-1849) of Berlin. The novel features of his work are that he uses a plate rotating in a *vertical* plane, and so can deploy an ordinary declination needle to 'measure' both the tangential and radial components of the force due to a single pole, at various radii, etc. His results, he believes, confirm a highly involved and exotic (and Aristotelian sounding) theory of induced magnetism (for all materials) of his own. Its principle, expounded in a single sentence which occupies one whole page! - replaced attraction by repulsion as a universal property of magnetism. "Hopefully", Pohl concludes "the mathematical theory of this class of phenomena will soon be developed - at least to the point sufficiently quantitative to give agreement with experiment." On a more practical level Pohl verifies that a suspended galvanic circuit can, like a compass needle, be deflected by rotating metal discs.

More prosaic and precise than Pohl's theory but no less ambitious, was the attempt by the celebrated and indomitable Simeon

Denis Poisson (1781-1840) to embrace the Arago effects within a comprehensive, mathematical analysis of magnetism that he was elaborating (15). Its basic physics is that of the revered Coulomb: two magnetic fluids, austral and boreal, separable only by infinitesmal "molecular" dimensions. Its lasting contribution is the demonstration of the mathematical equivalence of any distribution of (dipole) magnetisation with a configuration of surface and volume magnetic 'charges'. Initially (1824) this theory is developed in the context of the ordinary induced, or 'permanent', magnetism of stationary bodies; but later (1826), when Poisson becomes acquainted with the Arago effect, he stretches its physical basis (and assumptions) to include the effects of motion. He accepts the existence of the three components of force (repulsion as well as attraction in the rotating-disc experiments), acting mutually between the magnet and each element of the moving conductor; and like Babbage and Herschel, introduces the notion that for moving bodies there is, in addition to the static force, a time dependance. But Poisson is more explicit; this time-dependance is the same for all the foree components, and reduces to a constant after a short interval. The consequences of this 'theorem' he is able to follow by a "calcul rigoureux "; and to assure himself that they are in agreement with Barlow's measurements and the essential features of the Arago experiments. There is no mention in Poisson's papers of the doctrine that Ampère has been preaching for several years - that magnetism is a manifestation of electricity!

Whilst it is no surprise to find the experts in magnetism striving to extend the orthodox theory of magnetic-fluids to embrace the strange new phenomena, and inventing their "magnetism of rotation", surely Ampère himself was under no such spell? Had he not already witnessed induced currents - rather than induced magnetism-in his own experiments? Certainly he was not unaware of the work of his colleague (and editor of Annales) Arago. Indeed in 1826

> "Ampère was approached by Arago himself who wished to make use of the Voltaic pile and other equipment belonging to the Collège de France for a continuation of his researching. Arago had in mind substituting a solenoidal electromagnet for the magnetic needle in his original experiment " (Ref. I, p. 195).

With Ampère's approval the experiment (similar to Pohl's, Ref. 14) was mounted, and after some mishaps, was successfully completed by Colladon (who was then in Paris) under Ampère's supervision. Ampère apparently was satisfied that it was just one more piece of evidence in support of his theory: the identity of magnetism with electricity; in this case the equivalence of a bar-magnet with a solenoid. How did he picture the 'induced magnetism' (or whatever else) in the rotating metal, - if he comtemplated this question at all? It seems that at this time his faith in his own demonstration of the reality of currents induced by currents had weakened. He seemed content to accept Collandon's (App.) failure to observe induced currents; at least he offered no encouragement to continue this search Moreover, Becquerel (12) referring to the Ampère-De la Rive experiment in connection with his own investigations of weak magnetism, remarks that although this experiment seemed to demonstate current-current induction "Monsieur Ampère has subsequently become convinced that this is not so!" (1)

Was Ampère familiar with the experiments with the slotted discs - so strikingly suggestive of current flow? Or of the correlation of the Arago effect in different materials with electrical conductivity (which was at least qualitatively recognized)? It is certainly ironical that faced with the most spectacular - albeit complex - demonstration of electromagnetic induction, Ampère failed to reçognize or had lost interest in the phenomenon he had long sought. Ampere was not alone.

After a couple of years the great surge of interest in the Arago phenomena waned. The puzzle remained unsolved. All who theorised about 'induced' magnetism of rotation could only succeed by closing their eyes to the facts - especially the existence of repulsive as well as attractive forces; or by taking refuge in unfulfilled and unrealizable dreams and speculations. Undoubtedly the phenomena were too complex to provide a striking and convincing demonstration of the simple but revolutionary new principle that was needed. In any event it was not until 1832, after Faraday had taken up the whole issue of electromagnetic induction afresh, that the meaning of the Arago effect was revealed.

5. Retrospect

In 1822 Faraday had entered in his day book, amongst other "notes, hints, suggestions and objects of pursuit," the prophetic exhortation to: "Convert magnetism into Electricity".

Within ten years the prophecy was fulfiled; and the title headings of the beginning of his very first, definitive paper (Nov 1831) (16) show just how, in doing so, he had found the key to all the mysteries:

> " 1. One the Induction of Electric Currents. 2. On the Evolution of Electricity from Magnetism. 3. On a New Electrical Condition of Matter. 4. On Arago's Magnetic Phenomena."

And here in the first demonstration of the continuous generation of electricity from magnetism, (what a remarkable echo of his earlier triumph - the generation of continuous rotation by electromagnetism!) he found the secret of the mysterious Arago effect! This was just one by-product of his new discoveries, which were more significant for the future development of electromagnetism which they opened up, than in clearing away some long-standing riddles. Nevertheless some backward glancing and heart-searching questioning was inevitable. Why, now that the 'true' answers lay revealed, had they eluded all the earlier probings of Ampère, Arago, and others, and even of Faraday himself? Or had they? The perennial issue of priorities - not untinged with national pride - inevitably arose. Had electromagnetic induction really been discovered earlier, even if only dimly perceived and unconvincingly reported?

For Arago himself, the matter was relatively simple His own claims had never been more than to have discovered the new phenomena and to insist on their correct description. In-so-far as he ventured to explain them, it was only the conjectures of others that he reported (In fact he attributed the first suggestion of 'induced magnetism" to his young colleague Duhamel), and not necessarily with conviction. Reviewing these events in retrospect, Faraday praises Arago's honesty and open-mindedness when confronted with the new phenomena: (Quoted in Ref. I, Page 194)

> "What an education Arago's mind must have received in relation to philosophic reservation...what a fine example he has left us of that condition of judgement to which we should strive to attain."

"Philosophic reservation" and freedom from preconceived ideas can hardly explain Ampère's indifferent success; and in his case the phenomena he examined were hardly of the same circumstantial complexity. Ampère had, like Faraday, seemingly posed the right questions; were his experiments capable, sensitive and direct enough to give the replies? After the event, there seemed little doubt; so little that Ampère was rashly tempted to claim that not only had his experiments with de la Rive demonstrated electromagnetic induction, but that this was in essence predicted by his own "theory that traced all magnetic phenomena to the production of electricity in motion" Late in 1831, on learning of Faraday's success, but without full knowledge of all its particulars, Ampère hastily and imprudently, publishes a revised version of his now almost forgotten experiments with de la Rive (17). If before there had been some doubt about what was observed, (which was not lessened by Ampère's own vacillations as to its implication), now Ampère made matters more explicit; but unfortunately not more correct.

Absent from the new account is any hint that the induced current is a transient effect, or any clear statement about its direction (with respect to the primary current), and still less of the extreme importance of these features.

Faraday's reply to this claim is prompt and indignant. It is contained in a footnote to his first (Nov. 1831) paper(16):

> "The Lycée, No.36, for January 1st, has a long and rather premature article, in which it endeavours to show anticipations by French philosophers of my researches. It however mistakes the erroneous results of MM. Fresnel and Ampère for true ones, and then imagines my true results are like those erroneous ones. I notice it here, however, for the purpose of doing honour to Fresnel in a much higher degree than would have been merited by a feeble anticipation of the present investigations. That great philosopher, at the same time with myself and fifty other persons, made experiments which the present paper proves could give no expected result. He was deceived for the moment, and published his imaginary success; but on more carefully repeating his trials, he could find no proof of their accuracy; and, in the high and pure philosophic desire to remove error as well as discover truth, he recanted his first statement."

Faraday is here lumping together all the earlier attempts of Ampère, Fresnel and others; and he can hardly be blamed for failing to recognize the particular merits of one experiment whose outcome now, in contrast to earlier accounts, is now claimed to be so clear and decisive.

Much more revealing is Ampère's own frank self-appraisal (and criticism), contained in letters he wrote to his erstwhile collaborators in Geneva: (Letters to De la Rive, April 1833, November 1833 - Quoted in Ref. I, pp. 211-212):

> It is a fact that we were the first, in 1822, to obtain an electric current by influence, or induction as M. Faraday says, at the moment when we established the current within a spiral that surrounded a circle made of a thin sheet bent in this way [*see* Fig. 4] and suspended by a silk thread GH from a bracket K; that the effect made itself manifest by the attraction or repulsion exerted by a strong horse-shoe magnet that we had borrowed from M. Pictet, according to which pole was in the interior of the circle at B and which was outside at D. Unfortunately neither you nor I thought to analyse this phenomenon and to explore all its circumstances. We would have seen, what M. Faraday has since discovered, that the current lasts only for an instant and that it runs in the contrary direction to the current flowing in the spiral circuit, which produced it by induction.

Faraday has certainly made one of the most beautiful discoveries of all the electro-magnetic phenomena; but he is not the author of the very fact of the production of a current by induction, since we obtained this current in 1822...

The thin foil bent into a circle is either drawn towards or carried away from the poles of the horse-shoe magnet, to remain almost in the same position that it first assumed, as long as the exciting current continues to flow in the spiral circuit; precisely because, the first action being only momentary, there is no other while the current continues. Then, when it is stopped, the circle of foil returns to its original position, because a current in the opposite direction has been created in it. It was this return, which I attributed to the torsional force of the wire, that made me think of the persistence of the first action (as long as the current lasted) making an equilibrium with a supposed torsional force that did not really exist. As for the direction of the currents, whether the same or contrary, I had never in fact made the necessary experiments to determine it. But it is a fact that, in the three or four places in my memoirs or books in which I had spoken of it, I always avoided declaring its direction, because I always proposed to undertake a complete work on the induced currents, which I never did.

The same year, when passions were calmer, Ampère made his own peace with Faraday: (Letter of Ampère to Faraday, April 1833, Ref. 1, p. 213; see also Faraday's apologetic explanation: Experimental Researches (Philosophical Transactions, 1833, pp. 107-109).

At that time I had but one aim in making these experiments. I was searching exclusively (as you will recognize on looking at what I have published at that period, when I described the apparatus which I used) to resolve this question: Do electric currents, which are the cause of magnetic attractions and repulsions, pre-exist, before magnetization, around molecules of iron, or steel, or the two other metals where magnetic effects are observed; but exist in such a position that they cannot exercise any action beyond. Or are the currents produced, at the moment of magnetizing, by the influence of neighbouring currents?

When, in my first experiments of July 1821, I obtained no current of this sort, I reasoned (*Annales de Chimie et de Physique*, vol. 18, p. 377, and *Recueil d'observations électro-dynamiques*, p. 165) that, since a current was not able to produce another one by influence, then, necessarily, magnetization takes place because the current, or the bar magnet that does the magnetizing, only acts upon pre-existing currents in the iron or steel. But, when the experiment that I made in Geneva in 1822 with M. Auguste de la Rive obliged me to retract and admit the production of currents by influence, I thought that the great question of the pre-existence of molecular currents in metals able to be magnetized, was not to be answered in this manner and that it must remain undecided until it could be resolved by other methods; and I placed no further importance on these experiments, which I erred in not having studied more deeply.

Here in Ampère's own words is an explanation of one major reason for his failure; one which is certainly confirmed by what he had written at the time (1821) the controversial experiment was performed. Reporting what he believed is a negative result, he writes in a reply to an inquiry from Van Beck (at Utrecht) (18):

> "It is from this experiment that I have concluded, at the time I made it, that the electric currents, whose existence around each particle of a magnet I have already admitted, equally exist around these particles in iron, nickel, and cobalt, before the magnetization; but that they are to be found oriented in all directions, from which no external action can result..."

Magnetism, whether by another magnet or a current-conductor, is to be thought of as some alignment of the 'molecular' currents, which in the case of materials commonly regarded as magnetic are free to turn, in others not so.

This possibility of the permanent existence of circulating currents in magnets was certainly a thorny issue - as the earliest discussions with Fresnel show; but it was also central to Ampère's whole approach to magnetism. For induced magnetism Ampère wanted currents, but molecular - preferably pre-existing - rather than macroscopic. He was clearly happy to accept the absence of induced currents in wires. As for the Arago effect, regarded as induced magnetism, this too could be disposed of as some sort of alignment of pre-existing currents, which for some reason or other became free to turn only when the metal was in motion.

Throughout Ampère seems to have regarded experiment not only as primarily a test of a particular theory, but even more as providing an answer - yes or no - to some particular question he had in mind. And the question seems to have been invariably posed <u>before</u> the experiment was made - indeed, usually before the apparatus was built. Not only in his efforts to examine electromagnetic induction, but in all his work, we rarely see any full report of what he actually <u>observed</u>, but rather the conclusions he drew from the observations he made. Which often leaves us asking the question what did he observe? And what might he have observed? Some help in answering these questions will undoubtedly be got from repeating the experiments themselves and observing for ourselves.

In any replication of these early experiments we may now make, we must bear in mind that at the time when they were originally made (and indeed for decades afterwards), the nature of the Voltaic electric current, and its relationship to 'ordinary' electricity were anything but clear. In the whole range of new experimentation initiated by Oersted's discovery, there was great concern to separate - as well as to relate - the new phenomena and forces from the familiar electric ones. Amongst others, one feature of the new 'current' stood out in marked contrast to the old: its continuity. Indeed it was almost the hallmark of Voltaic electricity that it was no sudden electrical discharge, as were, characteristically, currents in the old electricity. Unwittingly or not, a 'steady state' seemed almost a prerequisite for observing true Voltaic effects - of which the magnetic phenomena were the newest and most intriguing ones. Often experimental conclusions were indeed vitiated because the Voltaic currents were not steady - although this was not usually due to the intervention of 'ordinary' (i.e. electrostatic) effects. But one has only to contrast the phenomena of a Leyden jar discharge with the discharge (as it was so often called!) of a Voltaic pile (c.f. Savery's experiments) to understand how wary experimenters were of anything transient, and how instinctively they sought steady conditions.

It is much easier to recognize the idiosyncracies of a particular individual than the influence of the character of a period in the development of science. Ampère was not alone in failing to recognize the essential features of time dependence in electromagnetic processes. For him, as for his contemporaries, it was, consciously or not, as much a matter of avoiding these as exploring them. And when such considerations could,- apparently without danger, since no Voltaic discharges were involved - be introduced, as in the Arago experiments, the time dependence introduced was, alas, an irrelevant one! Today we have a whole domain of "quasi-stationary" electrical current phenomena with proper criteria for establishing an appropriate time-scale. But such prescience was not vouchsafed for those who struggled with the puzzling phenomena of the 1820's.

Addendum (c.f. p. 8)

Babbage and Herschel did indeed mention Ampère's electrodynamics theory of magnetism as possibly related to their induced magnetism of motion. However the issue was confused by the still persistent report (insisted on by Arago himself!) that even non-conducting materials displayed the Arago effect. This seemed to eliminate real currents:

> "...if the electrodynamical theory of magnetism be well founded, it is difficult to conceive how that internal circulation of electricity, which has been regarded as necessary for the production of magnetism, can be excited or maintained in non-conducting bodies." (Babbage and Herschel, Ref. 9).

II

Experiments

(See Part III for Details of Apparatus)

A.1. Damping of Oscillations (Arago, Babbage, Herschel, Seebeck, etc.)

i) Time the period of oscillations (for small angles ± 20°) with magnet-needle suspended at various heights (0.5 - 5 cm) with no metal (Period with magnet described ~ 1.5 seconds).

ii) Measure Damping. Choose convenient range of angular amplitudes, e.g. initial ± 30°; final ± 10°. Measure damping-time at different heights; no metal. (Typical value 40")

iii) Damping time (T): as function of distance from metal surface. Typical values:

Copper disc, 1/32" thick: d=2 cm, T=17" ; d=1.3 cm, T=10"

Aluminum foil, 0.004": d=2 cm, T=30" ; d=1.3 cm, T=26"

iv) Damping time as function of thickness of metal, at constant thickness; typical values:

1/16" zinc: T = 20" ; 1/8" zinc: T = 13"

v) Effect of Different Metals: copper, zinc, aluminum, lead, etc. Metal sheets of equal thickness should be used if possible. E.g. aluminum 0.008" at 1.3 cm; T = 24" ; lead 0.025" at 1.3 cm; T = 32". Where materials of different thicknesses are compared, the differences in height should be compensated by placing sheets of paper (cardboard, lucite, etc.) under the metal plates.

These experiments demonstrate:

1) That the damping effect is produced by proximity to a metal.

2) That it decreases rapidly as the separation is increased (an approximate empirical law may be obtained. Is it the same for all metals?).

3) That metals are ordered in a specific way, (correlated with later observations, and with electrical conductivity).

These experimental arrangements may be reversed, with disc of metal suspended over a fixed magnet, and their oscillations observed; but in this case a torsional (wire) suspension will be required. With this arrangement the effect of cutting slots in the disc can also be examined (c.f. A.2. (vii) below).

A.2. Rotating Metal Disc and Converse (Arago, Herschel and Babbage)

i) Modern cheap commercial compass-needles are so badly mounted - or poorly magnetized - that effects observable in the 1820's are not easily reproduced with these. Detectable deviation of a common (say 2") needle, 1" or less above a copper (1/8" thick) plate rotating at 5 revs/sec, should be observable. For more extensive observations a specially mounted (strong) magnet is better. (See below, pp25,25a for details)

ii) Check that the deviation is not due to air drag by interposing and removing the screen (lucite, cardboard, etc.).

iii) Deviation of magnet as function of distance from metal.

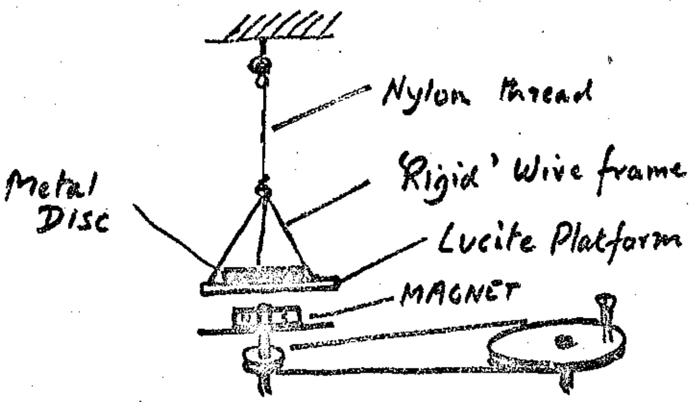
iv) Deviation of magnet for rotating plate of different metals. Here the correlation can be made with the metal used in A.1.

v) Try replacing the lucite screen with other materials (copper, aluminum, iron, etc.). Is the "force" transmitted through all the materials?

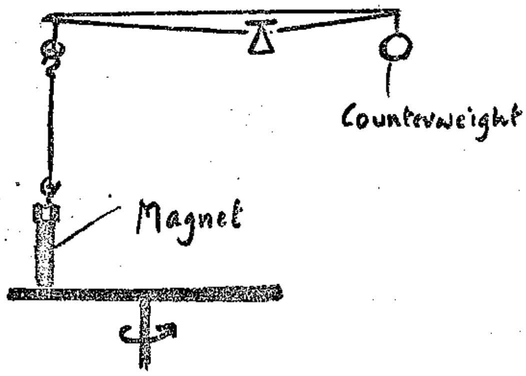
vi) The variation of the deviation with position of the magnet with respect to the plate should also be examined.

vii) Solid copper plates can be replaced with (radially) slotted discs, and the magnet deviations, for comparable rotation speeds, observed. (c.f. p. 8)

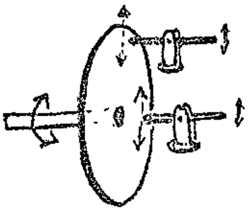
viii) The roles of magnet and disc can be interchanged. A small lucite tray is suspended by a thin wire Different metal discs of, say 3" diameter, are placed on the tray. A strong 2" bar magnet is clamped (firmly!) on the rotating platform. Deviations of the discs are observed as the magnet is rotated steadily. (This arrangement is not suitable for demonstrating continuous rotation: the suspension soon breaks!)



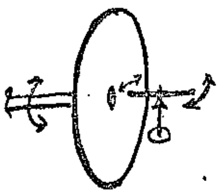
ix) Examination of forces due to "Single Pole" (Arago, Pohl, etc.). A bar magnet is suspended from a balanced arm, so that one end is close (few mm) to the surface of the rotating disc. The effect on the equilibrium (in the vertical plane) is examined for: different heights; different radial positions on the disc; different senses of rotation.



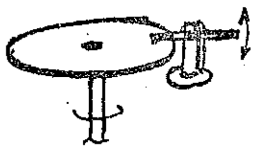
Similar observations can be made with a magnet mounted so as to turn freely in a vertical plane (Dip-needle); and with the copper disc rotating also in a vertical plane about a horizontal axis. With one pole close to the disc, components of the force, both radially, at (a), and tangentially, at (b), can be examined for various radial positions.



also:



and:



It need hardly be mentioned that the detailed theoretical analysis of all these "Arago-disc" phenomena is quite complex. The emphasis should be on what might be learned or conjectured from experiment. For example: the importance of the conductivity of the metals; the effect of overall configuration (not simply local force!), and symmetry of the forces, the significance - if any - of the Earth's magnetism.

IIa <u>Additional Experiments</u>

<u>A.1.*</u> In a more detailed investigation, the precise damping of the oscillations can be examined for "logarithmic" decrease. What can be deduced from this?

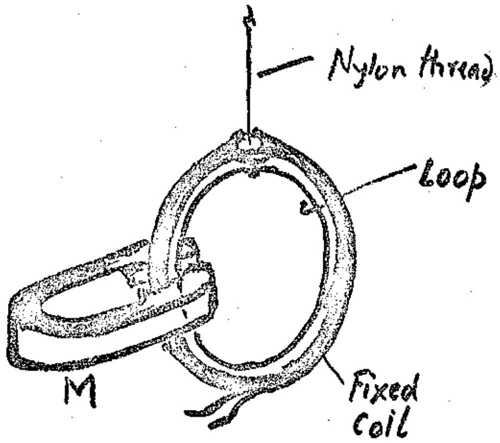
<u>A.2.*</u> With suspended/pivoted magnet at a distance where only deflections are observed, the angle of this deflection, Θ, can be examined as a function of speed of rotation, ω. (Sin $\propto \omega$. Significance?)

<u>A.2.ix)*</u> Does the conductivity of the disc-material have the same (proportionately) influence on the "repulsion" as on the dragging force?

<u>A.2.ix)*</u> Use a double suspended magnet arrangement - one above, one below the disc. Observe the dragging force when the magnets are aligned (a) parallel and (b) anti-parallel. What is the significance of this experiment for the "induced magnetism of rotation" theories?

B. <u>Ampère-De la Rive Experiment</u>

i) The single-turn copper loop is suspended from a torsionless thread and crefully centered. (Ampère in some places describes this suspension as a "very fine wire" (1822); in others as a silk thread (1833).) It will drift around; wait until it comes more-or-less to rest.



Now bring the magnet, M, into position. What is observed? Move the magnet about: What happens to the loop? (Recall that Ampère reports <u>no interaction</u> between the loop and the magnet when no current flows in the primary coil! Possibly this was a test that the loop was iron free?)

ii) Examine the effect of placing the magnet in different positions, so that the loop "feels" a different part of the magnet gap (and hence a different average field).

iii) When the loop is steady, switch on/ hold for a few seconds/ switch off the primary current (~5-10 amp). Examine the effect of varying the position of the magnet-gap, as in ii).

iv) Remove magnet M: Try the effect of the primary current alone.

An approximate (~factor of two) quantitative analysis of these effects can be made.

Comment on Ampère's statement: "There had, however, been no reaction between the loop and the magnet before the electric current passed through the spiral surrounding the ring"; and the significance of his"...plan to repeat (the experiment) immediately with a ring-current made of a highly purified non-magnetic metal."

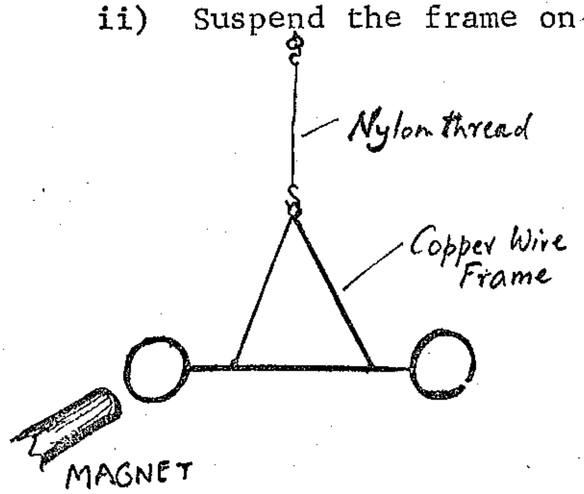
The effects undoubtedly depend on the strength of the magnet available to Ampère. This should be considered in relation to any effect observed in i), ii), iii) above.

C. Faraday's Experiment

This is essentially the same as the Ampère-De la Rive Experiment: Similar lessons can be derived from it.

i) Two or three different frames of different dimensions are used.

ii) Suspend the frame on nylon thread and leave it until it comes more-or-less to rest.



iii) Bring the magnet (~ 4" X ½" diameter) up to the closed ring. Gently, hold the ring steady, with magnet inserted: then withdraw it sharply. Observe what happens in each case. Repeat with the "open" ring.

iv) Estimate (roughly) the angular velocity imparted to the ring (etc.) when the magnet is withdrawn.

v) Time the period of oscillations of the magnet in the Earth's field (Assuming $B_{(Horiz: Earth)} \sim 0.2$ Gauss).

Estimate the magnetic moment (or pole-strength) of the magnet. Show that the observed effects can be quantitatively (if roughly) explained.

vi) Try the Faraday experiment itself: With the magnet inserted in the closed ring, and the latter steady, bring up another similar magnet.

Comment: Compare your own, with Faraday's observations in vi). Why did Faraday not report the phenomena observed in iii)?

Addendum: Re: Experiments A.2. viii)

To assess the "force", f, exerted by a magnet, rotating at constant speed, on a suspended disc, Babbage and Herschel measured the time, t, for the disc to make S revolutions, starting from rest. Assuming the "force" to be constant then,

$$f \propto S/t^2.$$

(Typical values: S, 1 to 5 ; t, 50 to 500 sec.)

III   Equipment

Notes regarding Drawings/Photographs

The drawings do not all correspond to the photographs of equipment which has been built and used here. On the basis of our experience the apparatus may often be scaled down somewhat to give added convenience. In some cases it has been simplified a little.

Many variants of the experiments are possible; experience soon shows what is feasible and suitable. Much of the equipment can be readily improvised - by the student! And, of course, the actual design or choice of a suitable method is an invaluable lesson to be learned from any experiment.

Apparatus for A.1. (p. 18). A simple arrangement is shown. Suitable magnets are available commercially (e.g. from Edmunds) with obvious modifications, the apparatus can be used for A.1.v). 4" diameter discs of various metals (ranging in thickness from 0.005" or so to 0.25") are also required. Non-metallic discs (lucite, etc.) may be used to adjust the spacing between metal and magnet. (p. 25)

Apparatus for A.2. (p. 19). The arrangement shown is for the basic experiment with a solid copper disc rotating. Similar discs of other metals (and one suitable for clamping a bar magnet) should also be made. No details are shown for A.2.ix): Suitable arrangements are easily improvised. (p. 26,27)

Apparatus for B. No details are shown of the magnet and its mounting. The shape/dimensions of the latter obviously depend on the magnet available. It should have a gap field ~2000 gauss (or more) and a pole spacing of not less than 1". The mounting should be designed so that the magnet can be slid in and out of position gradually. (c.f. Edmunds # 70,476 for possible magnet.)

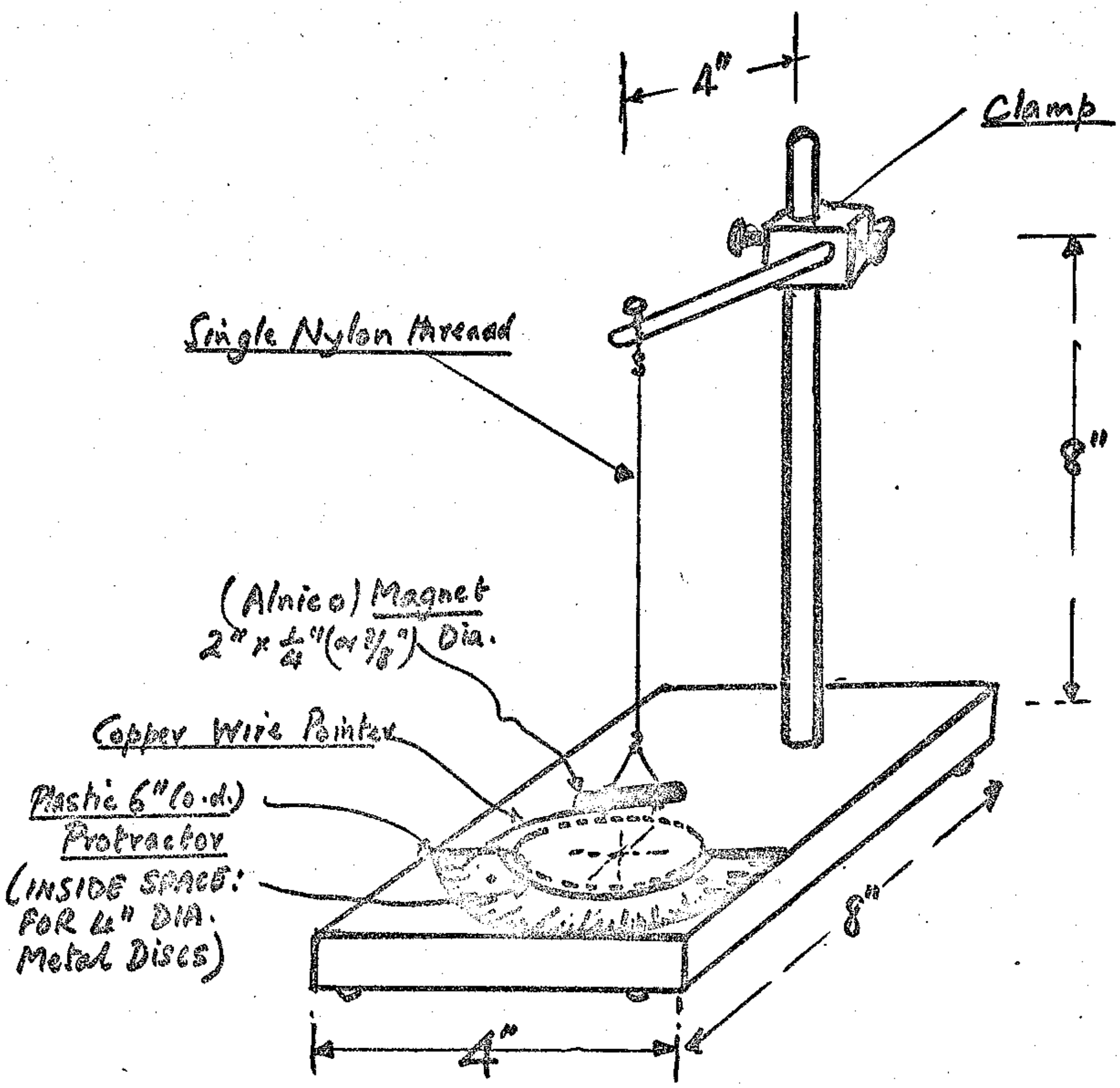
Both the fixed coil and the suspended loop should be fairly accurately circular. The coil after winding and temporarily taping, can be "fixed" by application of epoxy resin. For observing the deflections about 5-10 amp (from low voltage supply or car battery) are needed. To avoid overheating, the circuit should be closed for only a few seconds at a time (use a return-open tapping key). (pp. 28,29).

Apparatus for C. Simple and easily made. An Alneco magnet ~ 4" X ½" diameter is suitable; but, others with smaller intrinsic intensity of magnetisation may well be used for comparison. (p.25a).

N.B. Strong and essentially torsion-free nylon supporting threads may be extracted from (old) nylon hose. (An almost inexhaustible supply!)

ARAGO OSCILLATIONS

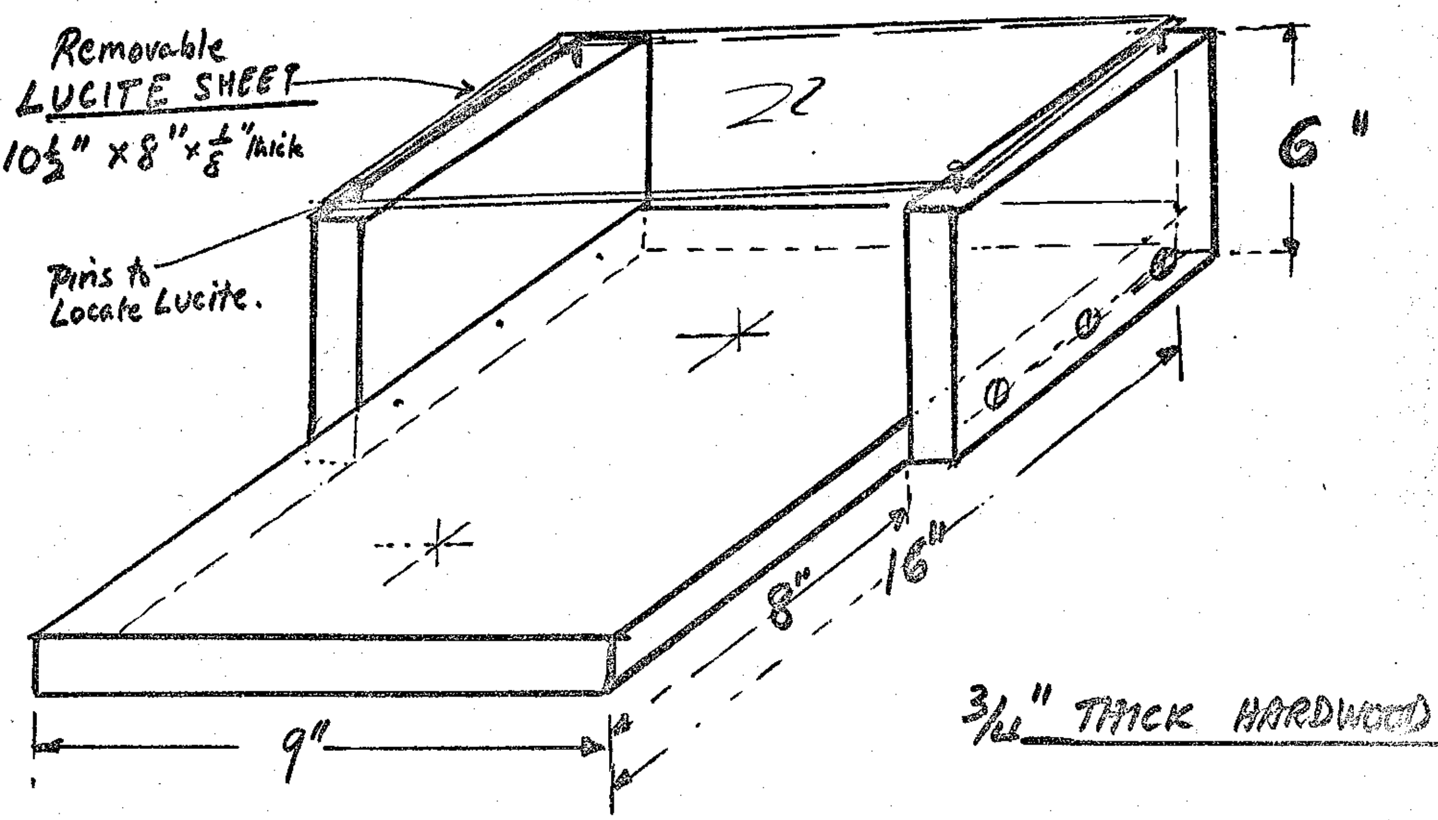
Fig. 1



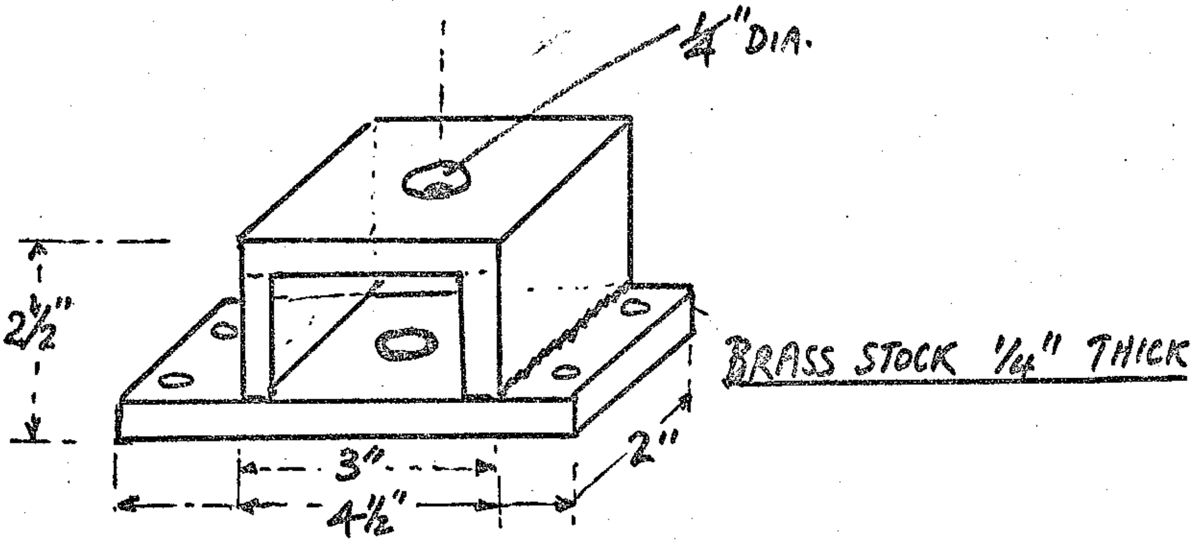
ALL MATERIALS NON-MAGNETIC (WOOD, ALUMINIUM etc.)

ARAGO: DAMPED OSCILLATIONS.

ARAGO WHEEL. (1)

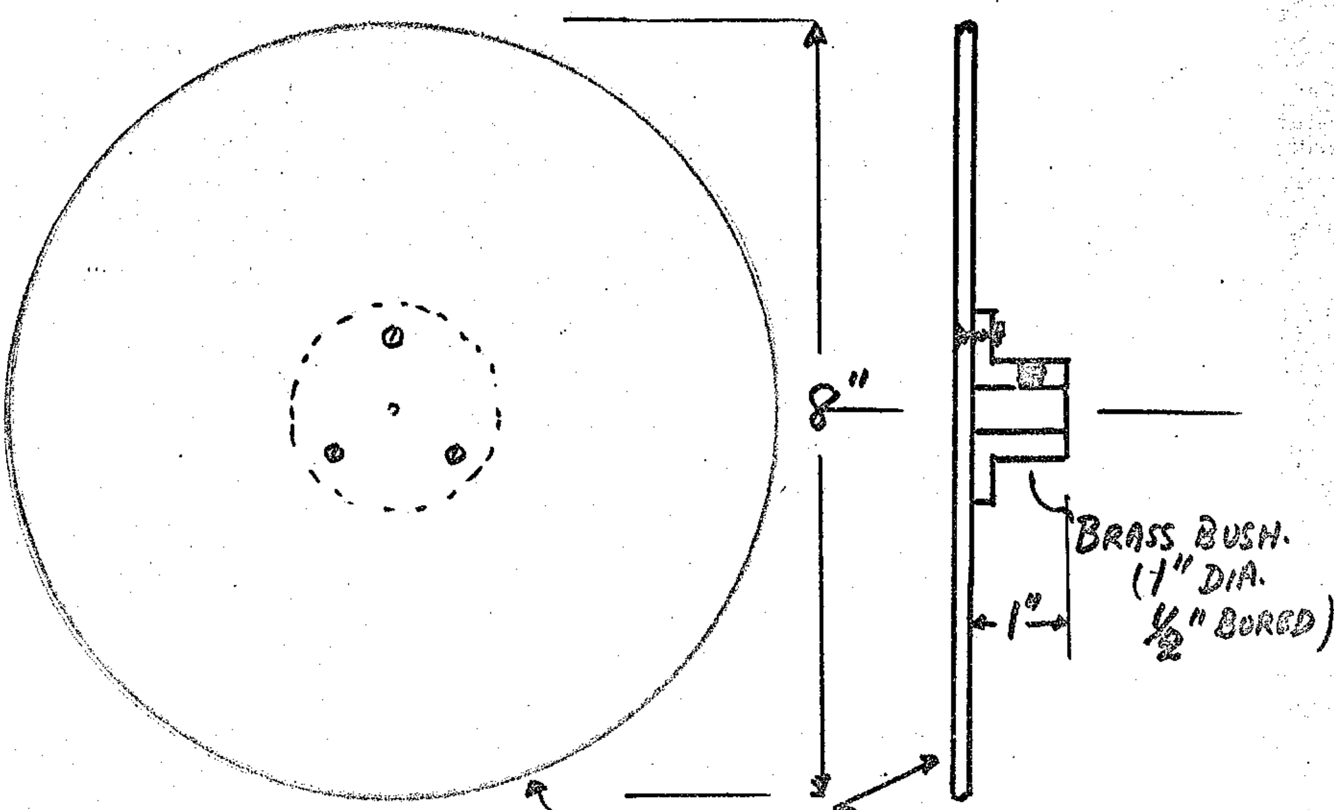


WOODEN BASE.

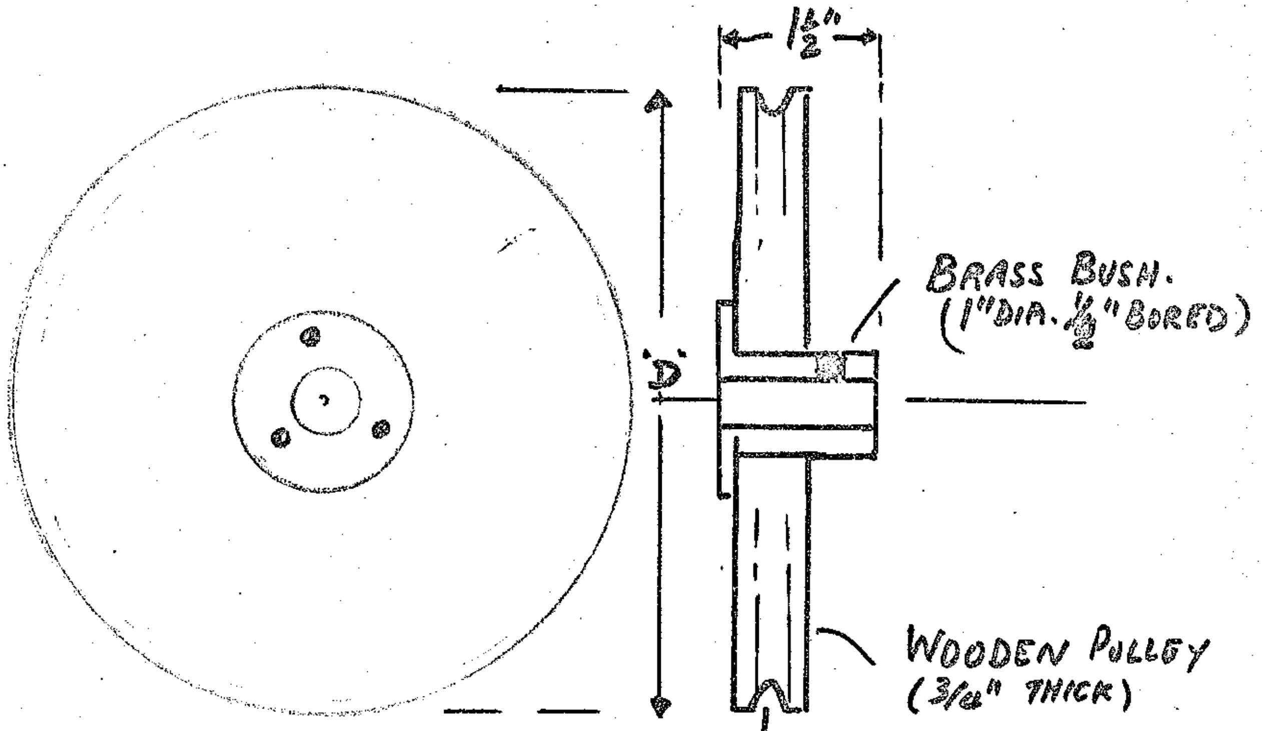


BEARINGS (×2)

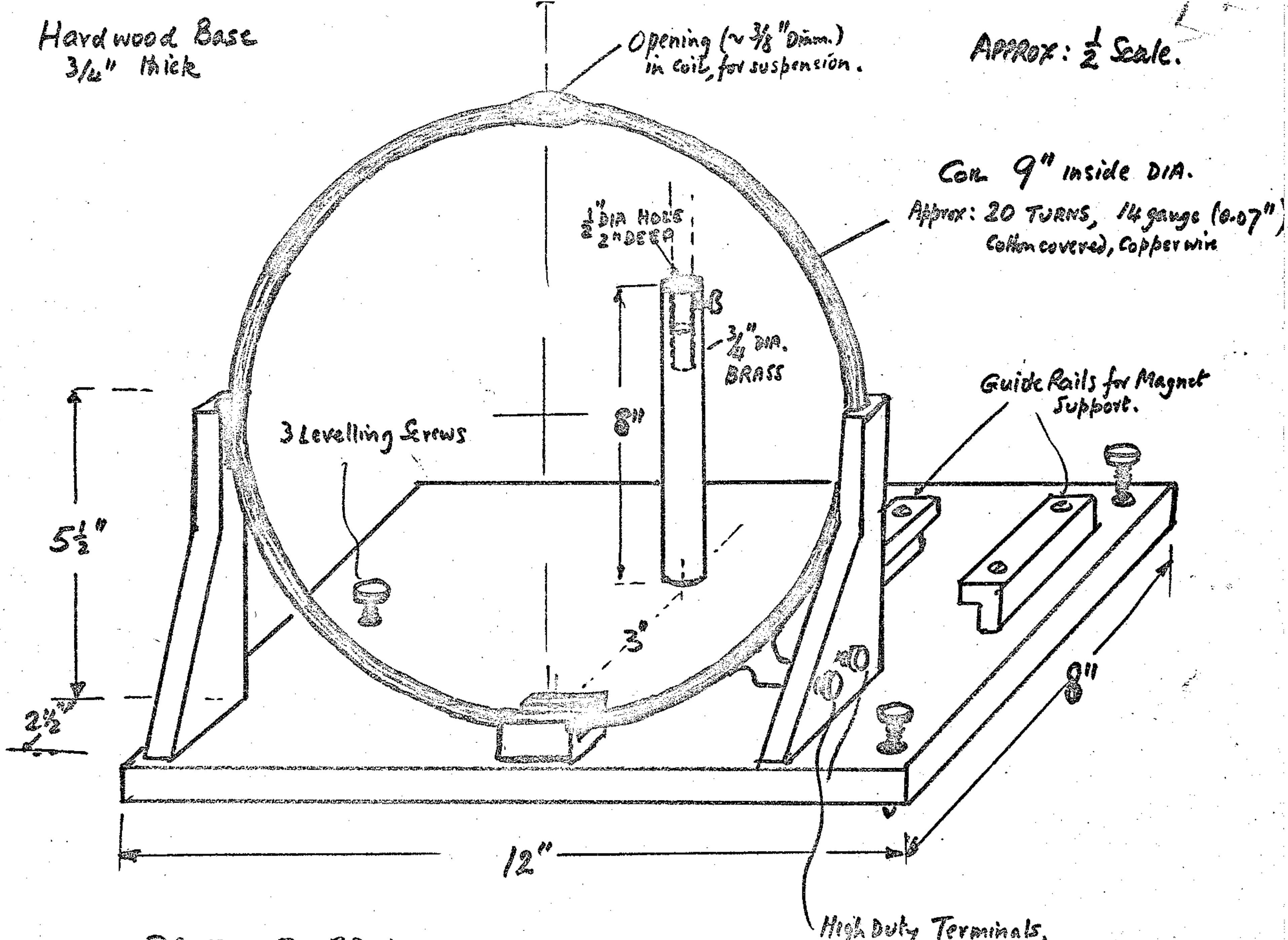
ARAGO WHEEL



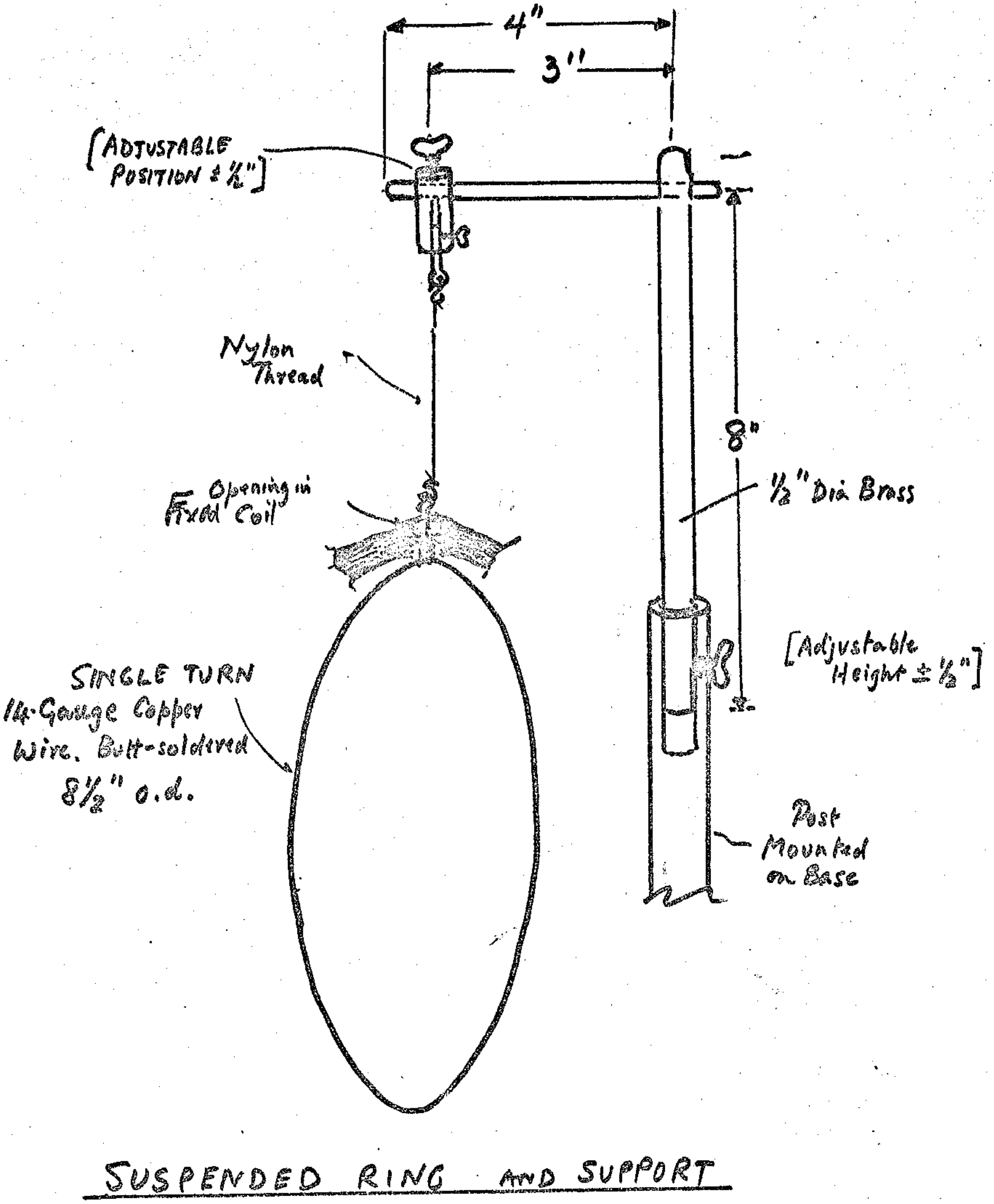
SCALE APPROX : ½



2 PULLEYS { 'D' = 6"
'D' = 3"



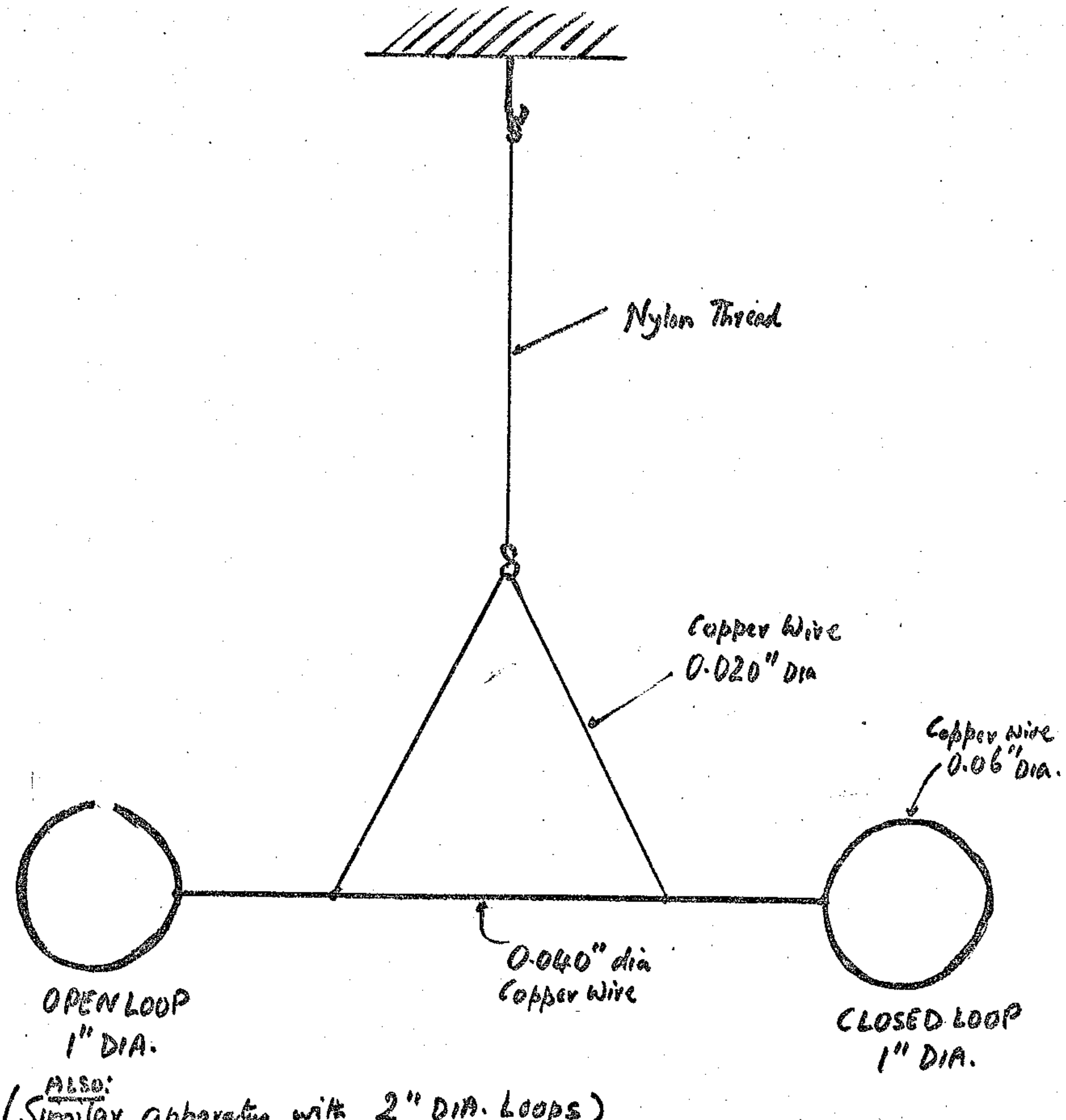
BASE ＆ FIXED COIL



SUSPENDED RING AND SUPPORT

AMPÈRE De la RIVE EXPERIMENT II.

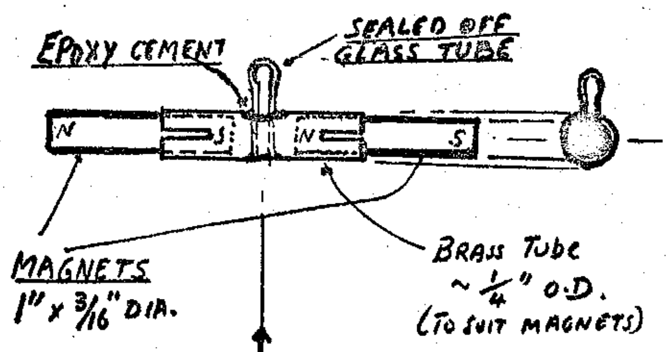
FARADAY's EXPERIMENTS



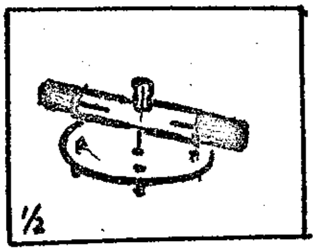
ALSO:
(Similar apparatus with 2" DIA. Loops)

FULL SCALE
(ALL DIMENSIONS APPROXIMATE)

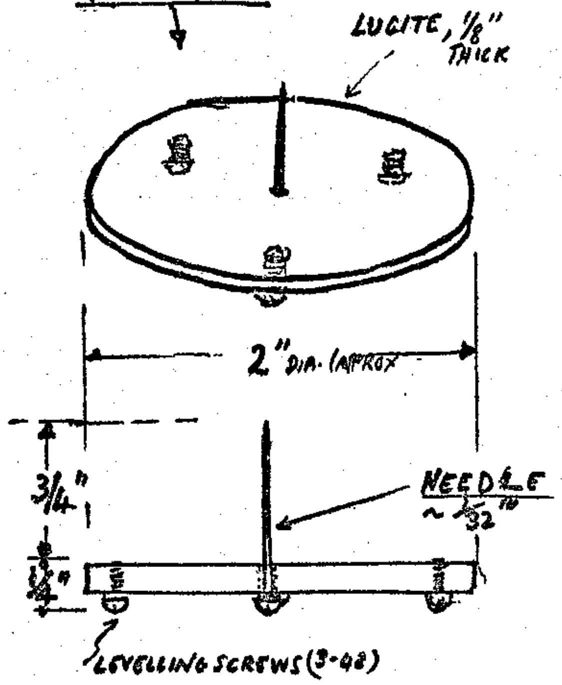
'COMPASS-NEEDLE' TYPE
SCALE APPROX 1:1



NEEDLE

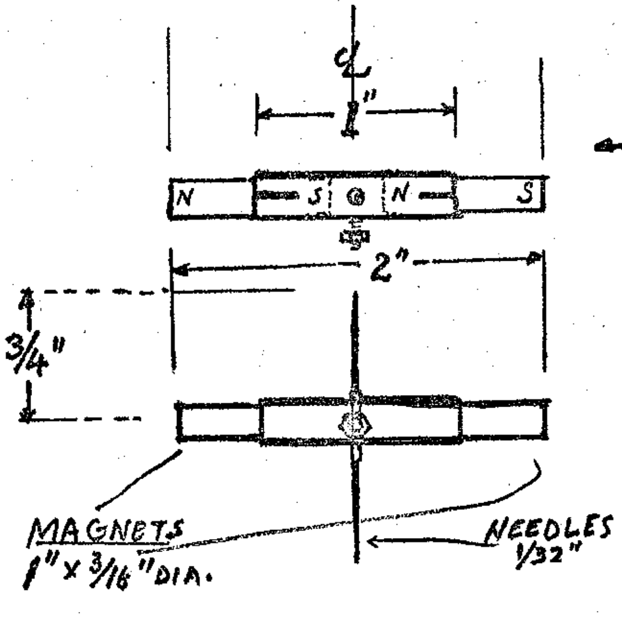


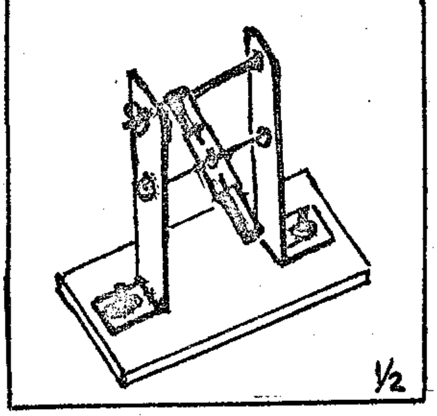
PIVOT



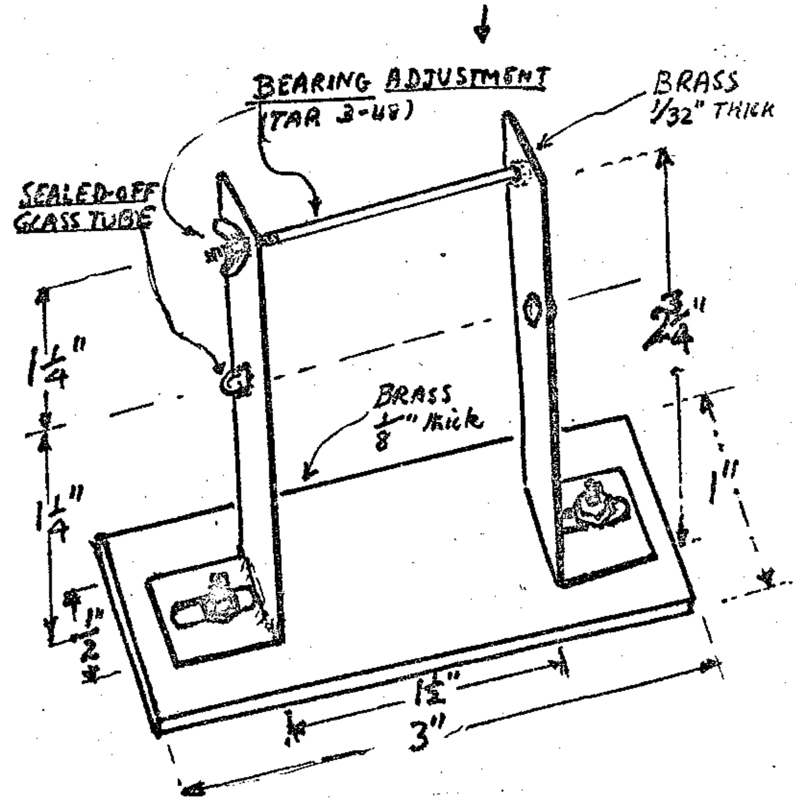
'DIP-NEEDLE' TYPE
SCALE APPROX 1:1

'NEEDLE'

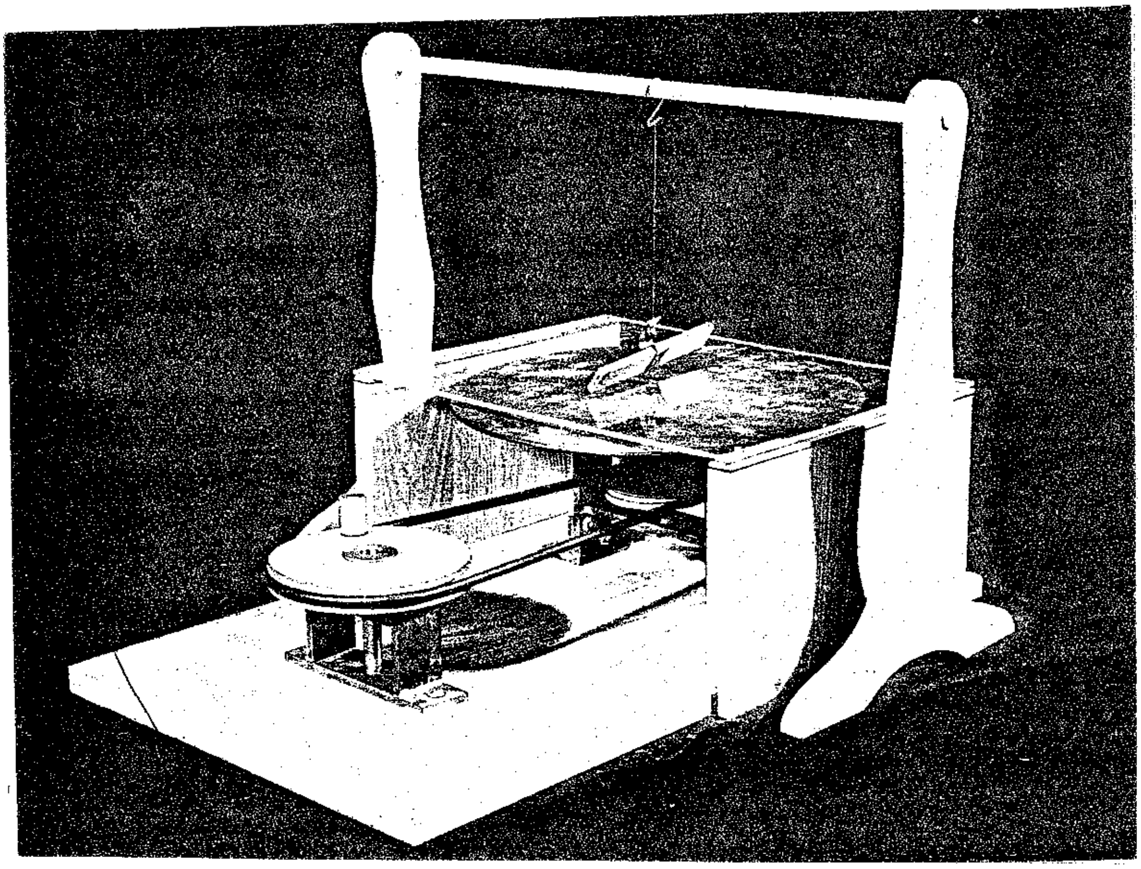




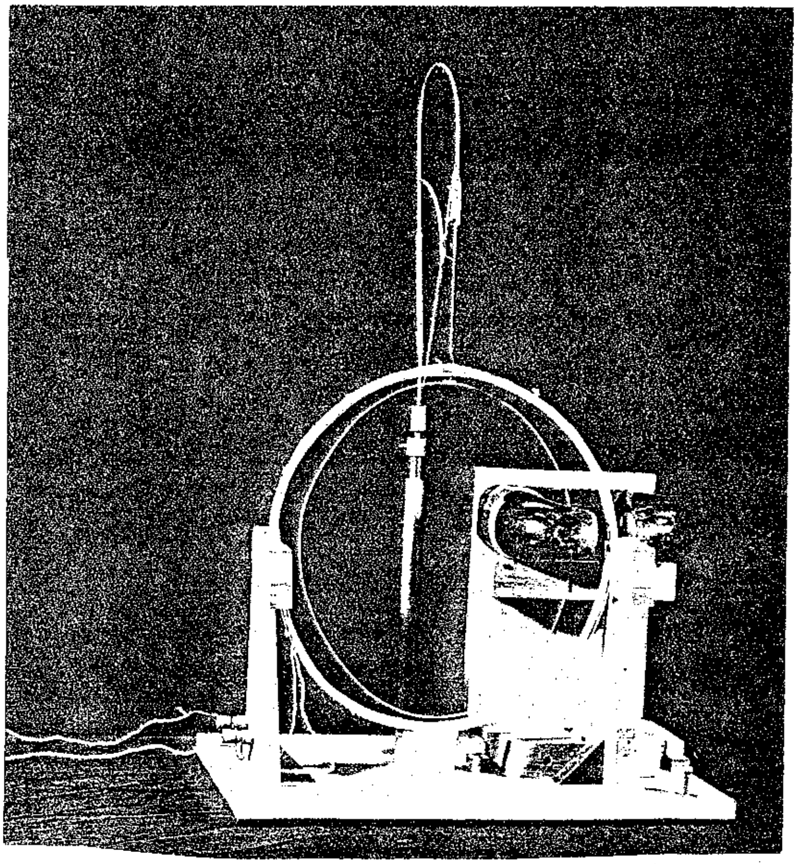
BEARING MOUNT



PIVOTED MAGNETS: FOR ARAGO EXPERIMENTS



ARAGO WHEEL (v. pp. 26, 27)



AMPERE-DE LA RIVE EXPERIMENT (v. pp. 28, 29)

IV Theoretical Notes

A. The calculation of the currents induced by a magnet in a moving conductor, and hence the force on the magnet, is in general a complex business. With proper management and interpretation, the Arago Effect - treated as an experimental inquiry - can provide many insights into the (qualitative) nature of these currents.

In a first approximation one may consider the induced currents as arising from the influence of the (external) magnet on the moving conductor; that is to say the magnetic-field due to the induced currents themselves is neglected as far as the distribution of these currents is concerned. This is equivalent to neglecting the inductance of the moving metal - a reasonable assumption if the velocities are small.

The current density in the metal is then assumed to be:

$$\vec{J} = K\,(-\bar{\nabla}\Phi + \bar{V} \times \bar{B}); \qquad (1)$$

Here K is the conductivity; Φ the electrostatic potential due to the free charge distribution, which results from the motion; $\bar{V}$, the velocity, and $\bar{B}$, the magnetic field. In the "first approximation" $\bar{B}$ is equated with the field due to the external magnets. In a steady state:

$$\vec{\nabla}\cdot\vec{J} = 0; \qquad (2)$$

(and at the boundaries $\vec{J}\cdot\hat{n} = 0$, where $\hat{n}$ is a normal to the boundary surface.). From (1) and (2) we have:

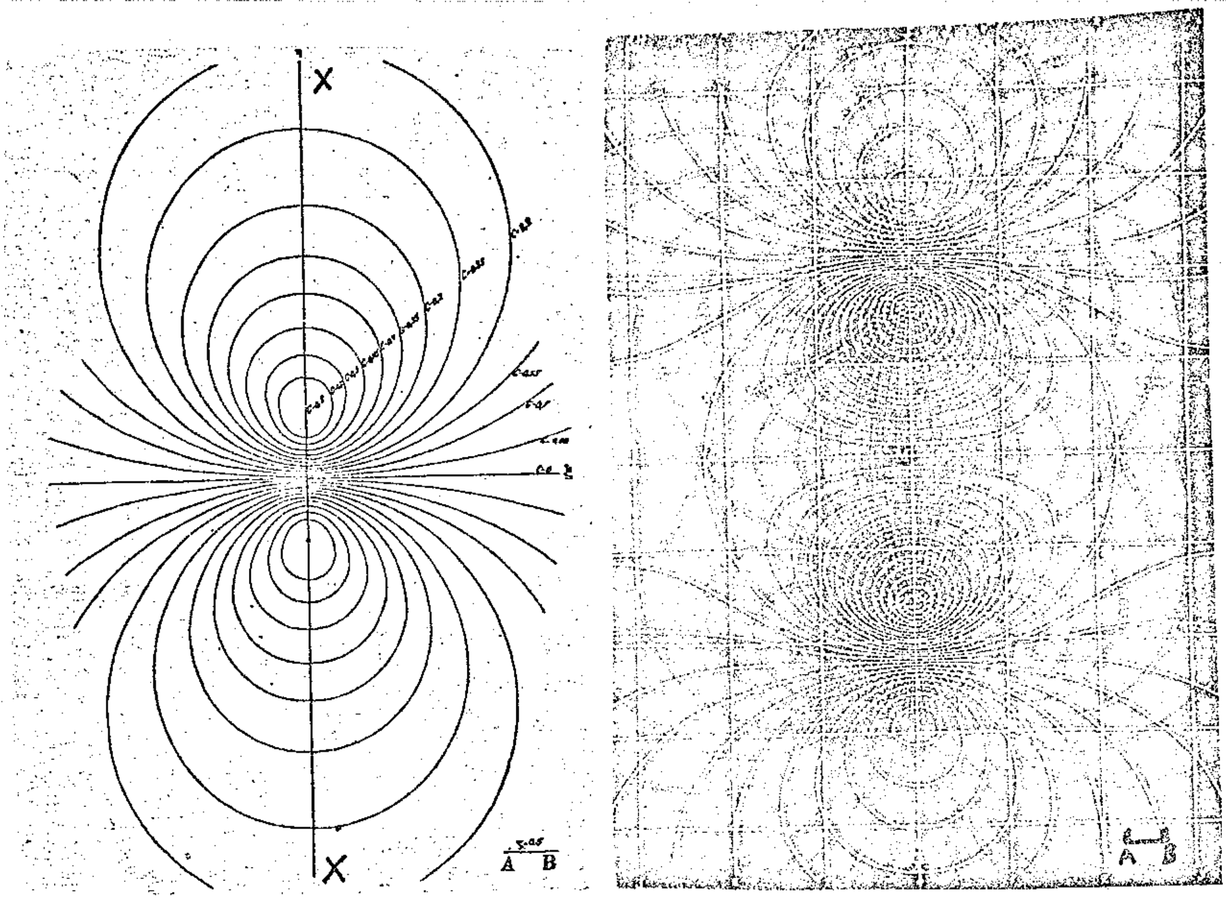
$$\nabla^2\Phi = \bar{\nabla}\cdot(\bar{V} \times \bar{B})$$
$$= \bar{B}\cdot(\bar{\nabla} \times \bar{V}).$$

For a circular disc, with constant angular velocity $\vec{\omega}$ (Z - axis), we have $\bar{V} = \bar{\omega} \times \bar{r}$, and hence

$$\nabla^2\Phi = 2B_z\,|\omega|.$$

This equation (equivalent to a free charge distribution in the metal of $-1/2\pi \cdot B_z \cdot |\omega|$), together with the boundary conditions, leads (in principle!) to a solution for Φ and hence $\vec{J}$. From $\vec{J} \times B$ the forces are calculated.

For the simple case of a single pole placed over a rotating disc, the lines of current flow and the equipotentials (not orthogonal - see equation (1)) look like; (a):



(a) Single Pole (b) Two Poles

(A B represents height of poles above plane; Current——; Equipotential.....)
X - X Direction of motion.

The simple theory implies linear super-position For two poles N and S, (ideal equivalent of a bar magnet) the pattern is shown above, (b).

(See Bibliography for theoretical references)

B. Here we have two effects to consider: a) the interaction of the magnet with the wire-loop, independently of the primary coil; b) the response of the loop, initially at rest, to a change in the current in the primary.

(a) Any piece of metal (conductor) interacts with a magnet, when there is relative motion of the two! We assume the motion is relatively slow compared with the "time-constant" of the loop: L/R (L is self-inductance, R the resistance); i.e. the same approximation as discussed in A, above. As a simple example, consider the impulse imparted to the loop if the magnet is moved from a position on the left, to that on the right:

$$\text{E.M.F.} = -\frac{\partial \mathcal{F}}{\partial t}, \quad \text{current} = -1/R \cdot \frac{\partial \mathcal{F}}{\partial t} \quad (\mathcal{F} \text{ is flux linkage})$$

$$\text{Force} = \frac{1}{R} \cdot \frac{\partial \mathcal{F}}{\partial t} \cdot \bar{\bar{B}} \cdot \bar{\bar{L}} .$$

(Where $\bar{\bar{B}}$ is typical field, and $\bar{\bar{L}}$ is the length of wire in the magnet gap during motion)

Then Impulse is:

$$\mathcal{J} = \frac{1}{R} \cdot \frac{\partial \mathcal{F}}{\partial t} \cdot \bar{\bar{B}} \cdot \bar{\bar{L}} = \frac{1}{R} \left[\mathcal{F}\right] \bar{\bar{B}} \cdot \bar{\bar{L}} .$$

As an estimate of $[\mathcal{F}]$ we can use $\bar{\bar{B}}\bar{\bar{L}}^2$, so that

$$\mathcal{J} \sim \frac{1}{R} \bar{\bar{B}}^2 \bar{\bar{L}}^3$$

Typical values:

R : 10^{-2} ohms ($=10^7$ emu)
$\bar{\bar{B}}$: 2000 gauss ,
$\bar{\bar{L}}$: 4 cm ,
$\mathcal{J}$ = 25 dyne-sec.

<u>Order of Magnitude of Angular Velocity Imparted, ω_0</u>. (Mass $\sim$ 3 gm, $a \sim 10$ cm)

$$\omega_0 = \frac{\mathcal{J} a}{I} = \frac{\mathcal{J} \cdot a}{\frac{1}{2} M a^2} = \frac{2\mathcal{J}}{Ma} \sim 1 \text{ radian/second!}$$

Notice that the impulse, and ω_0 , depend strongly on the strength of the magnet: $\bar{\bar{B}}^2 \times \bar{\bar{L}}^3$! (And it has been assumed that the field is negligible when the magnet starts and stops its movement.)

(b) <u>Loop starts from rest in field $\bar{\bar{B}}$</u>. Current change in primary coil: di_1/dt ; n - turns. For single turn , radius a, $\mathcal{F} = \mathcal{L}_{(1)} i$, where $\mathcal{L}_{(1)}$ is self-inductance for one turn. For n turns

$$\frac{d\mathcal{F}}{dt} = n \mathcal{L}_{(1)} \frac{di_1}{dt} \quad ;$$

Current in the suspended loop (neglect self-inductance), $i_{(2)} = n \cdot \mathcal{L}_{(1)} \cdot di_1/dt \cdot 1/R$ (Assuming all flux in (1), links (2).)

$$\text{Force exerted by magnet} = n\,\frac{\mathcal{L}_1}{R} \cdot \frac{di_1}{dt} \cdot \bar{\bar{B}} \cdot \bar{\bar{L}} \;;$$

$$\text{Impulse} = \frac{n\mathcal{L}_{(1)}}{R}\,\bar{\bar{B}} \cdot \bar{\bar{L}} \cdot \frac{di_1}{dt} \quad ,$$

$$= \frac{n\mathcal{L}_{(1)}}{R} \cdot \bar{\bar{B}} \cdot \bar{\bar{L}} \cdot [i_1] \;.$$

Using values (e.m.u.):

$n = 20$,
$\mathcal{L}_{(1)} = 10^3$ cm (~ 1 μ-Henry)
$R = 10^7$ e.m.u.
$\bar{\bar{B}} = 2 \times 10^3$
$\bar{\bar{L}} = 4$,

Then,

<u>Impulse</u> $\sim 1.6 \times [i_1{}']$ dyne-sec (with $i_1{}'$ in Ampères)

Initial angular velocity of loop $\sim \dfrac{2 \times 1.6\,[i_1{}']}{M\,a}$ (see above)

$$\omega\,(t_0) \sim \frac{1}{10}\,[i_1{}'] \;.$$

This depends <u>linearly on $\bar{\bar{B}}$ and $\bar{\bar{L}}$!</u>

<u>Subsequent</u> <u>motion</u> is electromagnetically <u>damped</u>.

Induced current due to motion of loop in magnet: $\sim \dfrac{\bar{\bar{B}} \cdot \bar{\bar{L}}}{R} \cdot a \cdot \omega$

Force on loop due to current $\dfrac{\bar{\bar{B}}^2 \bar{\bar{L}}^2}{R} \cdot a \cdot \omega$.

Equation of motion of loop (no current in primary) when in magnet gap:

$$I\frac{d\omega}{dt} + \left(\frac{\bar{\bar{B}}^2\bar{\bar{L}}^2 a}{R}\right)\omega = 0 \;,$$

i.e. $\omega(t) = \omega_0 e^{-\kappa t}$, $(\kappa = \bar{\bar{B}}^2 \bar{\bar{L}}^2 a / RI)$

and the deflection

$$\theta_{(t)} = \frac{\theta_0}{\chi}\,(1 - e^{-\chi t}) \;.$$

<u>Final deflection</u> (<u>assuming still in magnet gap</u>!)

$$\theta(t \rightarrow \infty) = \frac{\omega_0}{\chi} \;.$$

So if the loop receives an impulse (current change in primary coil) then, since

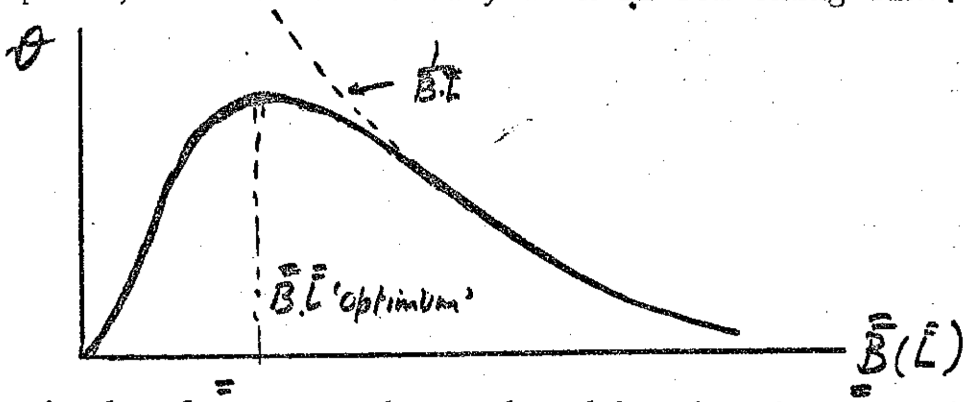
$$\omega_0 \sim \bar{\bar{B}} \cdot \bar{\bar{L}} \;; \text{ and } \chi \sim \bar{\bar{B}}^2 \bar{\bar{L}}^2 \;,$$

then

$$\vartheta(\infty) \sim \frac{1}{\bar{\bar{B}} \cdot \bar{\bar{L}}} \;,$$ i.e. larger, the

<u>weaker</u> the magnet, and "theoretically" becoming infinite for $\bar{\bar{B}} \cdot \bar{\bar{L}} = 0$!

Of course all other damping forces have been ignored! Never-the less it is clear, that insofar as the part of the loop in the field stays in a more-or-less uniform field, the deflection may well be smaller for stronger fields! But for very weak fields other frictional forces will dominate; so that one would expect the (impulse) deflection to vary with $\bar{\bar{B}} \cdot \bar{\bar{L}}$ something like:



The magnitude of $\bar{\bar{B}}_{opt}$ may be explored by changing $\bar{\bar{B}}$ - either by changing the position of the magnet, or the pole separation. $\bar{\bar{B}}_{opt}$ should be assessed for a particular loop arrangement.

C. This is very similar to A. The total flux-change in inserting (or withdrawing) one end of a bar magnet through the small closed loop is easily assessed: it is $\sim 4\pi m$, where m is the pole-strength. This latter is readily estimated from the measured magnetic moment and dimensions of the magnet.

The impulse is approximately:

$$\frac{3}{2}\pi^3 \cdot \frac{m^2}{aR} ,$$

where a is the radius and R the resistance of the small loop. (Typical value ~ 2 dyne/cm, for m ~ 200 cgs.) Notice the dependence on m^2 and $1/a$. This can be checked experimentally and a conjecture made about Faraday's failure to observe this effect!

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(Abbreviations: A d CP: Annales de Chimie et de Physique
PT: Philosophical Transactions of the Royal Society)

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Expérience d'induction électromagnétique tentée à Genève en 1825,

Par M. D. COLLADON.

Je crois pouvoir dire quelques mots d'une expérience que j'ai faite à Genève pendant l'été de 1825, et qui, par suite de circonstances tout à fait spéciales, ne m'avait donné qu'un résultat négatif, dont j'avais fait part, dès cette époque, à quelques personnes, notamment à MM. Aug. de la Rive, J.-L. Prevost, Ch. Sturm et, deux ou trois ans plus tard, à M. Ampère, qui ne fit aucune remarque critique à cette occasion et ne m'engagea pas à la renouveler.

J'avais admis, comme chose possible, que la présence du pôle d'un fort aimant présenté à l'extrémité d'une hélice formée d'un fil de cuivre recouvert de soie pourrait développer dans cette hélice un courant électrique permanent.

Possédant un galvanomètre très sensible, je prolongeai d'environ cinquante mètres une des extrémités de son fil conducteur enveloppé de soie, que je terminai par une hélice à spires serrées, de quatre ou cinq centimètres de diamètre et longue de huit ou dix. J'empruntai au cabinet de physique un très fort aimant en fer à cheval, qui faisait partie de sa collection, pour approcher un de ses pôles de celui de l'hélice sur le prolongement de son axe.

Pour éviter toute influence possible de cet aimant sur le galvanomètre très sensible dont je me servais, j'avais porté ce galvanomètre dans une chambre éloignée de celle où j'opérais, je l'avais placé sous une cloche de verre et j'avais vérifié avec soin la position de l'index, après quoi je revins vers la spire et je rapprochai un des pôles du gros aimant de l'hélice, puis, sans me presser, je retournai vers le galvanomètre et je constatai que son index était exactement au même point qu'auparavant.

N'ayant aucun aide avec moi et ne soupçonnant pas que l'induction pût être un effet seulement instantané, dû au rapprochement ou à l'éloignement réciproque de l'hélice et de l'aimant, je ne pouvais mieux opérer. — Ce fut seulement six ans après que, les expériences de l'illustre Faraday étant connues, j'eus le regret d'apprendre que j'avais été bien près de découvrir, en 1825, un des faits les plus importants de la physique moderne et celui qui a donné naissance aux applications les plus précieuses au point de vue mécanique et industriel.

An account by J. D. Colladon of his experiments in 1825 when he almost discovered electromagnetic induction . Reproduced by kind permission of the Bibliothèque Nationale Suisse, Berne.

("Recherches et expériences sur l' électricité". 1825-1837. Reprinted Geneva 1893. Also in Ref. I.)

Entries in Faraday's Diary referring to unsuccessful attempts to detect electromagnetic or electrogalvanic induction (1824-1828).

1824. DECR. 28TH.

Expected that an electro magnetic current passing through a wire would be affected by the approach of a strong magnetic pole to the wire so as to indicate some effect of reaction in other parts of the wire—but could not perceive any effects of this kind. The power was from 2 to 30 pr. of 4 inch plates. The circuit was made long, short, of moderate copper wire—of very fine silver wire—the indicating needle was put into a galvanometer. The pole was put into a helix, etc. etc., but in no case did the magnet seem to affect the current so as to alter its intensity as shewn upon a magnetic needle placed under a distant part of it, although the M. Pole was so strong as to make the wire bend in its endeavours to pass round it.

1825. NOVR. 28TH.

ELECTRO MAGNETIC INDUCTION.

Experiments on induction by connecting wire of voltaic battery. A battery of 4 troughs, ten pair of plates each, arranged side by side.

Expt. I. The poles connected by a wire about 4 feet long, parallel to which was another similar wire separated from it only by 2 thicknesses of paper. The ends of the latter wire attached to a galvanometer exhibited no action.

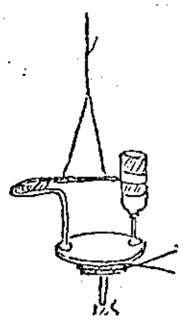
Expt. II. The battery poles connected by a silked helix—a straight wire passed through it and its ends connected with the galvanometer—no effect.

Expt. III. The battery poles connected by a straight wire over which was a helix, its ends being connected with the galvanometer —no effect.

Could not in any way render any induction evident from the connecting wire.

280 1825. DECR. 2ND.

ELECTRIC INDUCTION.

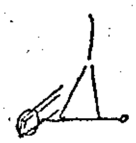


Expts. on Electric induction in imitation of Arago's experiment on rotation of magnets.

A Deluc's column dried and got into good action suspended horizontally by 5 feet of silk thread, then a plate of copper whirled beneath it—no action.

A small Leyden jar mounted with an external wire bent and arranged as in the sketch was suspended as before, and being charged the knobs or balls were one positive and the other negative. The copper plate being whirled beneath, no effect was produced except such as was due to the wind of the plate.

APRIL 22ND. 1828



Made a ring of clean copper wire, soldering the extremities; fixed it with thread to a piece of wire and suspended it as a balance of torsion (as in fig.); introduced the pole of a strong bar magnet through the ring, supposing it might exert an influence upon it; but upon bringing other magnets near the wire could observe no effect, whatever the position of them.

Brought the middle and other parts of a horse shoe magnet round the wire connecting the poles, but this closed circuit produced no effect.

Repeated the experiments with a copper ring not soldered but twisted together at the ends, but obtained no useful results. Repeated them also with a ring composed of many alternations of platina and silver but still obtained no direct results.