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Saturday.

You eternal ass, Φ , what do you mean by objecting to my Electrometer? Here it is plainly and absolutely (for it is an absolute Electrometer, even in TT's sense of the term). -

A and B are two plates connected with the outer coating of a Leyden jar, each charged positively, therefore, and each at the very high potential Φ . On the upper surface of A and on the lower surface of B there is a layer of gutta percha, or any substance of very high specific inductive capacity; the thickness of each of these layers is very considerable, and in order to prevent a discharge through them from the plates A and B. C is an axis about which turn two aluminium pointers (very thin), Ca and Cb. At p the pointer Ca broadens out into a broad circular ~~plate~~^{equal} disc, p; and at q Cb broadens into the disc q. These discs are flush with two large plates parallel to A and B, through wh. the pointers Ca and Cb pass.

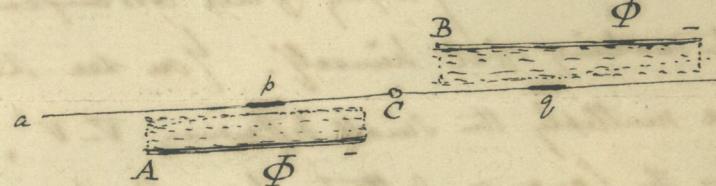
Before electification, let the plate A be moved by screw motion so that the disc p just rests against it; & let B just flush with q.

Connect p with the \oplus pole of a cell whose pot. is V' , and connect q with the other pole whose pot. = V' .

Let e = thickness of gutta percha layer, S' = surface of discs; and let a small weight, w, attached somewhere near a (at a distance x from C) restore equilibrium.

$$\frac{(\Phi - V')^2}{8\pi e r^2} S' = \frac{(\Phi - V)^2}{8\pi e^2} S' + \frac{wx}{l},$$

$$\therefore V - V' = \frac{4\pi e^2 wx}{l \Phi S}$$



The instrument can be used to measure Φ itself. For this purpose throw the plate B and the disc q out of account by discharging them, & let p and A be both at Φ .

(2)

There will be repulsion which can be corrected by weighting the pointer C_2 , as before.

The action would be greatly increased by placing plates above p and below q each at large potential Φ & each +ly electrified. We should thus get $\Phi + \bar{\Phi}$ in the den^r instead of Φ .

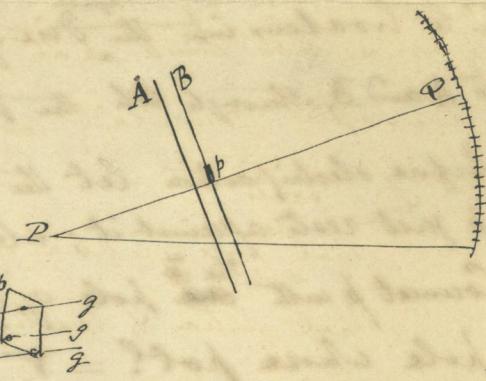
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Clark thinks very highly of this arrangement, & for a time he felt disposed to try the making of it himself. You see that by using a very large potential Φ we multiply the small difference $V - V'$, while the danger of a spark is obviated by the layer of gutta percha, wh. has the further advantage of multiplying the result by its high specific inductive capacity. True, we lose advantage in increasing e , but the loss, I think, is much more than compensated by the other two facts.

To explain the Construction of The Absolute Inclined Plane Electrometer.

Although the preceding instrument is all that can be desired, a curious and very effective arrangement may be here described.

Two parallel plates A and B are fixed ^{large} to an inclined plane PQ, whose incl. " can be varied. In the center of B is a small moveable plate of aluminum wh. is capable of sliding parallel to PQ on three little fixed glass rods passing through it parallel to PQ thus:



The distance between A and B can, by new arrangement, be made as small as we please. The plates & disc being uncharged, PQ is gradually lifted up until the disc p begins to slip. Then A is connected with a source at potential Φ , and B (with p) with a source at pot. $-V$. PQ is again raised and the new incl. " noted at wh. slipping begins noted. This will be very slightly $<$ or $>$ ~~the~~ previous value. If λ is the L of friction and $\Delta i = 0^{\circ}$ measure of difference of incl. "

$$\frac{(\Phi - V)^5}{\Phi \pi \ell^2} = w \sec \Delta i, \text{ where } w = \text{weight of disc.}$$

It would be necessary to observe that λ was always the same in every trial with uncharged plates.

I think that if K is the sp. induc. capacity of the gutta-percha, the equation would be

$$V - V' = \frac{4\pi e^2 \omega x}{\epsilon K^2 D^2},$$

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i.e., I believe that K^2 (and not K) would be in the den. This of itself would compensate the comparatively great value of e . [In fact K^2 would be in den. ∵ if we can put $\frac{e}{K}$, the "reduced thickness".]

I shall go to town this morning to consult with Elliott's manager about the construction of their Fleet & the Thermopile. I saw him a few days ago, and the possibility of accurate suspension of aluminium needles, & he said that it could certainly be done; but I did not then tell him the object I had in view. I have not yet studied your last letter. But why should it cease to be true that change of energy of system = work (internal + external) done by forces, if the mutual forces are not in lines joining particles?? Do you not think that Routh is simply an ass for enunciating the principle oracularly, as D'Alembert did? Is it now quite obvious from the two facts —

- 1°. That force of inertia of each particle = $\frac{\text{total}}{\text{resultant}}$ resultant force acting on it;
- 2°. That action & reaction take place in lines joining particles?

No. 2 will, I maintain, be open to doubt in certain critical states of matter. Copies of my book will be ready in a week or 10 days.

I do not yet know whether I can go over to Dublin before the end of a fortnight. If you were there, I should go; but I can employ myself in South Kensington very well till you return to town. Killaloe is rather out of the way, & doubt whether we could do much work there. You won't probably be tempted to stay too long there; & with much work ~~do~~ to do, we have only a short time to live and are occasionally full of misery [when splendid electrometers are pooh-poohed].

You never told me whether you considered my reference to Tellett
satisfactory or not.

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Binghamton, New York