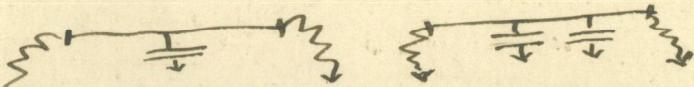


Paxton Devon 14/5
Aug 17-93.

My dear Fitzgerald, Altho I had no idea of troubling you to notice my Review of a Review except on the one question I directed your attention to at the end, I am glad to have your very interesting letter, & to reply on a few of the points.

Ferranti Effect. It is a resonance effect, viz. the reaction between ^{the inductive} Capacity of Cable and Inductance of "apparatus", altho the latter is not a very definite quantity ^{itself}. It therefore comes under Maxwell's theory of resonance, condenser & coil, given by him in a letter to Sir W. Grove, altho my knowledge of that is slight, only having seen it quoted. Now there is another kind of resonance, viz. in the cable itself, which is most marked when there is no terminal interference (short circuit or disconnection at ends brings it fully on), due to the to and fro reflection of waves. The frequency is much too small for the length for this resonance, which I described in 1886, to

be operative. So the Ferranti effect is the more simple resonance described by Maxwell - I was written to at the time the Ferranti came out, asking for information, and I warned my inquirers not to waste their time over elaborate formula relating to the one effect when it was pretty plain it could not be in it, & advised them to treat the cable as one condenser, or at most, as two,



and calculate by the condenser & coil theory applied to the experimental data. One of my inquirers wrote a paper about it. He brought in the more complicated formulae, but it was quite out of place, as I told him. But I don't say it did no harm.

By the way it has always struck me as very queer and unintelligible that Maxwell in his treatise did not say something about Sir W. T's condenser-coil theory, & his own extension thereof showing resonance. But not a word! I found both out for myself as well the true cable self-resonance.

S.P.T. I know he is trying to follow up my theory because he has more than once directed attention to it, & I have no reason

to suppose that he wd hesitate to act & to
knowle it. The field is open to him & others to find practical ways of
doing it. As for details, I have gone into
very considerable detail, and numerically,
in our paper, "On Telegraph & Telephone
Circuits" vol. 2 p. 339 to 354, besides in
other papers more generally. I don't care
to describe practical dodges for imitating
the effects which can on paper be got
to any extent by increasing the inductances uniformly.

^ Atlantic telephony is perhaps very remote,
though possible, but that great improve-
ments are practicable in Atlantic
telegraphy & in shorter cable telephony
I feel certain, & I might have done
it had it come in my way. But it
is out of my way to anything of no use
information about my papers though, ^{and} provided it does not entail laborious
calculations. ^{to be having "appointment"} ^{the idea of}

[Permanent magnet & Current.] This is what
I specially wanted to know about, but
I don't quite see where the difficulty referred
to it, or rather, precisely what it is. No
doubt a full theory of magnetism is very

difficult, with hysteresis etc; but evidently
it isn't that, that is the question. Let it be
a rudimentary magnet, constant μ and
constant density of intrinsic mag. Start
a current in it. Where does the energy come
from? From the battery of course! And from
the magnet as well, partly, it may be. I now
think I see the point of your reference to my
p. 460 vol 1, & perhaps it is the point
J. P. referred to. In fact, it was to explain
the similar difficulties that I remember I
altered Maxwell's reckoning of magnetic
energy, & magnetic stress ^{from mechanical force}. They went ^{went} work
into a consistent scheme at all, no how!
My system does work consistently &
comprehensively, whether right or wrong. [I
regard an intrinsic magnet as a store
of energy trying to work itself out in a
manner equivalent to an unperformed mag-
netic force b say. The intensity of intrinsic
mag. is μh ($\text{or } \frac{\mu h}{4\pi}$ in common
units). On this basis you can work out
details perfectly consistently, stress, force,
flux of energy, etc. As regards the
physical meaning of b or μh , I have
also tried to put that down, in mere

than one place, partly in the article
in vol. 2. p 39. The energy that \underline{h}
draws upon when it works is quite distinct
from the energy of the field; it comes from
the magnet, where \underline{h} is, when it works,
& is the energy of some structural alteration
in the material produced in the act of
conversion of "induced" to "intrinsic" mag-
netisation, which tries to set up an
external field, & does, according to the
distribution of \underline{h} and of \underline{B} . \underline{h} may
be set working by external means, as
its activity is $\underline{h} \cdot \underline{B}$ (your - Sh. 5)

It is a consistent theory, & I believe
there is something in it, with limitations
of application. \underline{h} may be regarded
as approximately constant in a hard
steel magnet provided \underline{B} does not change
greatly. I made some experiments to try
to settle this. Very crude though want
of means, but so far as they went, they
decidedly confirmed any idea of the
intrinsic force \underline{h} (or equivalent thereof)
trying to work as much as the magnetic
resistance would let it. But of course

\underline{h} must vary as well as \underline{p} in general, and
in any case I shd regard the theory as
rough, even if we knew the variations.

If you have a bar magnet, quite uniform
longitudinally magnetised intrinsically, and
also a solenoidal current round its surface
of such strength as to be equivalent to \underline{h}
in producing induction; then the two together
will double \underline{B} all over, and the extra
magnetic energy will come out of the latent
force in the magnet, provided the doubling
of the induction does not alter \underline{h} or \underline{p} too
much, when modified reckonings will be
required. But I would only desire to
stick to the fundamental idea concerned.
See the illustration I use on p. 457. vol. I.

[Illustration.] I will send you a proof
of my gravitational two articles when I
get a revise. I think it will interest you.
In the meantime I may say that what I
have done is this. [I assume gravitational
force is a property of the ether, and that
it is propagated at single speed v ,]
so that $v^2 \nabla^2 \underline{e} = \ddot{\underline{e}}$
if \underline{e} is density intensity of force, so that

$\rho \underline{v}$ is force on ρ (density of matter).)
also show that if \underline{u} is the velocity of
 ρ then $\rho \underline{u} - c \underline{e}$ (c a constant)
is a circuital flux. say therefore

$$\text{curl } \underline{h} = \rho \underline{u} - c \underline{e}$$

This leads to another circuital equation
viz. $\text{curl } \underline{e} = \mu \underline{h}$

where μ is such that $\mu c^2 = 1$. [So
we have a very curious variation of
the theory of convective currents &
electrification & their electromagnetic
effects.

$\rho \underline{u}$ is analogous to convection it
is the flux of matter.] Details in
article. [The remarkable thing is that
even when v is merely the speed of light
the perturbations from the Newtonian
law are only of the order $(10)^{-6}$ of the
full force for fast stars, and only
 $\frac{1}{360}$ of this for slow stars like the Sun.

If Sun at rest, then the Newtonian
law is unaffected.] I have also made
allowance for the effect of \underline{u} and \underline{h}
It is more complicated, but of the

same order of magnitude. Discussion
does not come in, so far as I can see.

Yours sincerely
Oliver Heaviside

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