

I have, within the last three days, got very decent currents indeed by phosphorescence, but I have not made enough observations to get at any law or regularity. I have abandoned Balmann's paint, & made emulsions of powdered sulphide of calcium and cold liquid gelatine which are poured over platinum plates, ^{and} immersed in distilled water. Magnesium light has given as big a deflection as 5 inches on the scale.

Clark has joined me in working at the very curious currents by the action of light (sunlight) on tin plates immersed in distilled water. These currents are very strong and constant, ceasing instantly with the light. We ~~are~~ are of opinion that the light decomposes the water (!! in presence of tin, either wholly or partially overcoming the affinity between the oxygen and hydrogen, and to some extent aiding the affinity of tin and oxygen. We have almost proved that the result is not due to free oxygen in solution, but to a real (total or partial) decomposition of the water molecule; and the fact that the plate returns almost instantly to its primitive condition on the withdrawal of the light points to a partial overcoming of the affinity of O and H, which affinity ^{re-}establishes itself on withdrawal of light. There is thus probably a state of surface strain in the tin.

Our next experiment will be with the tin plates in H_2O_2 , so that a real chemical and complete action of O on Sn will probably take place; for we expect that the affinity of the surplus O for the H will be more easily overcome.

[A red hot coal in front of the phosphorescent plate above described gives no result.]

10/9

Feb. 6.

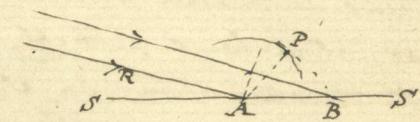
Φ,

In prospect of the time of Easter, when you usually come over here, I write to express a hope that you will soon get ready to pack up a few things and pay me a visit.

I am again working at the fluorescent & phosphorescent experiments, and the consideration of what takes place has led me back to the very foundations of the theory of atoms and molecules, and in particular to the elements of the Wave Theory of Light.

It strikes me that even the proof of the laws of reflection and refraction given by Lloyd is very far from being satisfactory.

Take a wave incident on the surface, S'S', of a body. Lloyd



begins by assuming that the point A of incidence of a particular ray "becomes a centre of disturbance" and that a spherical wave will disperse from A in both media. Now what is the precise way in which A becomes "a centre of disturbance"?

Does the ether atom at A disturbed by the ray R impinge on an immovable atom or molecule

of the hard reflecting surface at A, and thereby get knocked out of the simple harmonic vibration which it would have taken up and continued, had all gone well - i.e., had it not unfortunately knocked its head against the body at A?

Or does it actually displace the atom (or molecule?) of the surface at A?

In the latter case, it seems to me that the result will be complicated; for the displaced atom of the surface will probably perform vibrations which will not harmonise with the vibrations of the ether atom, and thus a new and, to some extent, independent disturbance will be communicated to the ether.

Indeed, this is likely to be the case in phosphorescence. There is here, I assume, a displacement of the atom of the body, and this last for some time after the incident ether disturbance has ceased - the body atom being probably massive as compared with the ether atom. In fluorescence the body atom is also displaced, but its disturbance ceases nearly with that of the ether.

But, then — why is that we have to look almost in the plane of the surface of a fluorescent body to see the fluorescence? Why should not the new reflected rays be visible in all directions? I thought that I saw some explanation of this yesterday, but I am now not

clear about it.

Possibly there is a portion of the energy of the incident ether

atom between the body atom and the ether atom in the body, so that the next time the body atom rises and hits the ether atom on the reflecting side of the surface it hits it with less velocity, & thus the body atom becomes a centre of disturbance of less velocity than that of the incident light. Thus the disturbance due to the motion of the body atom is one of lower refrangibility. But even this latter disturbance would travel (would it?) with the same wave-velocity in the ether at the reflecting side of the surface, and the fluorescent rays ought to come off at an angle equal to that of incidence. This assumes that all disturbances are propagated with the same wave-velocity in the same ether — which probably is not quite true.

Possibly, again, there is a real loss of kinetic energy in such a case, part of it passing into potential work inside the body molecule — viz., potential work of the forces of affinity of the body atoms in the body molecule.

The proofs of MacCullagh, Fresnel, and others, as well as I remember them, deal with ether and ether, the body atoms playing no part in the question. Witness what they call their assumption of "preservation of the vis viva" of the incident motion. I am strongly disposed to deny this assumption.

10/9

R....

11 Boddy

10 (a)

A few ideas on the expression and nature of the kinetic energy of a body.

We must begin by considering the simplest rigid body we know - viz., the atom.

If dm = mass of an atom, and v its velocity, its energy is $\frac{1}{2}v^2 dm$, or, with more refinement,

$\frac{1}{2}(v^2 + k\omega^2) dm$, if we consider it has having rotation.

Now if all the atoms of the system agree about their velocities, expressed by the forms

$$v_x = a + g^2 - ry, \quad v_y = b + ryc - px, \quad v_z = c + rye - py,$$

where a, b, c, p, q are constants throughout the system, the whole collection is a large rigid body and its only energy is

$$\frac{1}{2}(\bar{v}^2 + k^2\omega^2) M,$$

which is called energy of "visible" motion.

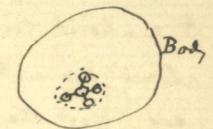
Possibly, however, the velocities of the atoms may consist of the above expression corrected by periodic functions of t (the time), and in this case two things may happen -

1°. the terms of correction may be such that all the atoms in one molecule move like a rigid body, no two molecules (possibly) behaving with regard to these terms as a rigid body;

2°. the terms of correction may be such that they do not express, even for the atoms inside the same molecule, the displacement of a rigid body.

In case 1°, the energy of the whole is increased by a term H , wh. is the energy of vibratory motion of molecules as rigid bodies. This is the energy of slow vibrations - Heat.

In case 2°, the additional energy is the energy of vibratory motions some of which (those of the smaller atoms) are very rapid - Light - and some of which (those of the



Body

larger atoms) are comparatively slow — Heat again. This is roughly true, but it requires a further consideration to make it quite right. The motions of the more massive atoms are not necessarily slow in virtue of the massiveness, for the rapidity of their vibrations depends on the forces (affinities) acting on them as well as on their masses. I should say that bodies (such as magnesium) wh. emit little heat and much light either have atoms of small mass or strong affinities in their molecules. Bodies which have small atoms coupled with strong forces of affinity ought to give nothing but the very rapid vibrations.

Clark has promised to look up numerical details in connection with this; but I need hardly say that any observations of yours will be most important.

Look at Lodge's lecture on "Heat? and Light" in last week's Nature. I heard him deliver it. He quoted you as an authority.

I think that the above way of putting the ordinary statement about energy gives ideas a definiteness & logical precision which the ordinary way of talking fails to give.

Tell me any College news going.

Keep this letter as it contains in writing for the first time some floating ideas of mine.

M.

Ball is coming here next Saturday.

Dr. Feddersen of Leipzig is publishing a biographical dictionary of scientific men, among whom he has included ME. No doubt he has also sent you a form to fill up.

I am sending Williamson my paper on Uniplanar Motion for the Royal Soc. in a few days, & you might glance at it. The planimeter is not quite finished.

I shall probably run over Bécat you at Racquet at Easter, & if I can get ready a decent collection of exp.^{ts} on phototele^t? we might perform them at one of your scientific meetings at the R.D.S.

Write, leisurely, thoughtfully, prayerfully, and adequately.

Are you Prof^v of Exp^t Physic yet?

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