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XXXIX. *Radiation Pressure.*

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A HUNDRED years ago, when the corpuscular theory held almost universal sway, it would have been much more easy to account for and explain the pressure of light than it is today, when we are all certain that light is a form of wave-motion. Indeed, on the corpuscular theory it was so natural to expect a pressure that numerous attempts were made † in the eighteenth century to detect it. But the early experimenters had a greatly exaggerated idea of the force they looked for. Even on their own theory it would only have double the value which we now know it to possess, and their methods of experiment were utterly inadequate to show so small a quantity. But had these eighteenth-century philosophers been able to command the more refined methods of today, and been able to carry out the great experiments of Lebedew and of Nichols and Hull, and had they further known of the emission of corpuscles revealed to us by the cathode stream and by radioactive bodies, there can be little doubt that Young and Fresnel would have had much greater difficulty in dethroning the corpuscular theory and setting up the wave theory in its place.

The existence of pressure due to waves, though held by

* Communicated by the Physical Society; being the Presidential Address, delivered at the Annual General Meeting, February 10, 1905.

† Some account of these methods is given by Nichols and Hull in "The Pressure due to Radiation," Proc. Am. Ac. xxxviii. No. 20, p. 559. See also Priestley, "On Vision," p. 385.

Euler and used by him 160 years ago to explain the formation of comets' tails by repulsion, seems to have dropped out of sight, till Maxwell, in 1872, predicted its existence as a consequence of his Electromagnetic Theory of Light. It is remarkable that it should have been brought to the front through the investigation of such a special type, such an abstruse case, of wave-motion, and that it was not seen that it must follow as a consequence of any wave-motion, whatever the type of wave we suppose to constitute Light. I believe that the first suggestion that it is a general property of waves is due to Mr. S. Tolver Preston, who in 1876 * pointed out the analogy of the energy-carrying power of a beam of light with the mechanical carriage by belting, and calculated the pressure on the surface of the Sun by the issuing radiation, obtaining a value equal to the energy-density in the issuing stream, without assumption as to the nature of the waves. But though the analogy is valuable, I confess that Mr. Preston's reasoning does not appear to me conclusive, and I think it still remains an analogy. There is, I suspect, some general theorem yet to be discovered, which shall relate directly the energy and the momentum issuing from a radiating source. It seems possible that in all cases of energy transfer, momentum in the direction of transfer is also passed on, and therefore there is a back pressure on the source. Such pressure certainly exists in material transfer, as in the corpuscular theory. It exists too, as we now know, in all wave transfer. From the investigation below (p. 397) it appears to exist when energy is transferred along a revolving twisted shaft. In heat-conduction in gases, the kinetic theory requires a carriage of momentum from hotter to colder parts ; so that there is some ground for supposing the pressure to exist in all cases.

Though we have not yet a general and direct dynamical theorem accounting for radiation pressure, Professor Larmor † has given us a simple and most excellent indirect mode of proving the existence of the pressure, which applies to all waves in which the average energy-density for a given amplitude is inversely as the square of the wave-length. Let us suppose that a train of waves is incident normally on a perfectly reflecting surface. Then, whether the reflecting surface is at rest, or is moving to or from the source, the perfect reflexion requires that the disturbance at its surface shall be annulled by the superposition of the direct and reflected trains. The two trains must therefore have

* Engineering, 1876, vol. xxi. p. 83.
† *Encyc. Brit.* xxxii. Radiation, p. 121.

equal amplitudes. Suppose now that the reflector is moving forwards towards the source. By Doppler's principle, the waves of the reflected train are shortened, and so contain more energy than those of the incident train. This extra energy can only be accounted for by supposing that there is a pressure against the reflector, that work has to be done in pushing it forward. When the velocity of the reflector is small, the pressure is easily found to be equal to $E\left(1+\frac{2u}{U}\right)$, where $\frac{E}{2}$ is the energy-density just outside the reflector in the incident train, U is the wave-velocity, and u the velocity of the reflector. If $u=0$, the pressure is E; but it is altered by the fraction $\frac{2u}{U}$ when the reflector is moving, and the alteration changes sign with u. A similar train of reasoning gives us a pressure on the source, increased when the source is moving forward, decreased when it is receding.

It is essential, I think, to Larmor's proof that we should be able to move the reflecting surface forward without disturbing the medium except by reflecting the waves. In the case of light-waves it is easy to imagine such a reflector. We have to think of it as being, as it were, a semipermeable membrane, freely permeable to æther, but straining back and preventing the passage of the waves. In the case of sound-waves, or of transverse waves in an elastic solid, it is not so easy to picture a possible reflector. But for sound-waves I venture to suggest a reflector which shall freeze the air just in front of it, and so remove it, the frozen surface advancing with constant velocity u. Or perhaps we may imagine an absorbing surface which shall remove the air quietly by solution or chemical combination. In the case of an elastic solid, we may perhaps think of the solid as melted by the advancing reflector, the products of melting being passed through pores in the surface and coming out to solidify at the back.

Though Larmor's proof is quite convincing, it is, I think, more satisfying if we can realize the way in which the pressure is produced in the different types of wave-motion.

In the case of electromagnetic waves, Maxwell's original mode of treatment is the simplest, though it is not, I believe, entirely satisfactory. According to his theory, tubes of electric and of magnetic force alike, produce a tension lengthways and an equal pressure sideways, equal respectively to the electric and magnetic energy-densities in the tubes. We regard a train of waves as a system of electric and magnetic tubes transverse to the direction of propagation, each kind pressing out sideways—that is, in the direction of propagation. They press against the source from which they issue, against each other as they travel, and against any surface upon which they fall. Or we may take Professor J. J. Thomson's point of view *. "Let us suppose that the reflecting surface is metallic; then, when the light falls on the surface, the variation of the magnetic force induces currents in the metal, and these currents produce opposite effects to the incident light, so that the inductive force is screened off from the interior of the metal plate: thus the currents in the plate, and therefore the intensity of the light, rapidly diminish as we recede from the surface of the plate. The currents in the plate are accompanied by magnetic force at right angles to them; the corresponding mechanical force is at right angles both to the current and the magnetic force, and therefore parallel to the direction of propagation of the light." In fact, we have in the surface of the reflector a thin current-sheet in a transverse magnetic field, and the ordinary electrodynamic force on the conductor accounts for the pressure.

In sound-waves there is at a reflecting surface a node—a point of no motion, but of varying pressure. If the variation of pressure from the undisturbed value were exactly proportional to the displacement of a parallel layer near the surface, and if the displacement were exactly harmonic, then the average pressure would be equal to the normal undisturbed value. But consider a layer of air quite close to the surface. If it moves up a distance y towards the surface, the pressure is increased. If it moves an equal distance y away from the surface, the pressure is decreased, but by a slightly smaller quantity. To illustrate this, take an extreme case, and for simplicity suppose that Boyle's law holds. If the layer advances half way towards the reflecting surface, the pressure is doubled. If it moves an equal distance outwards from its original position, the pressure falls, but only by one-third of its original value; and if we could suppose the layer to be moving harmonically, it is obvious that the mean of the increased and diminished pressures would be largely in excess of the normal value. Though we are not entitled to assume the existence of harmonic vibrations when we take into account the second order of small quantities, yet this illustration gives the right idea. The excess of pressure in the compression half is greater than its defect during the extension half, and the net result is an average excess of pressure—a quantity itself of the second order—on the reflecting surface.

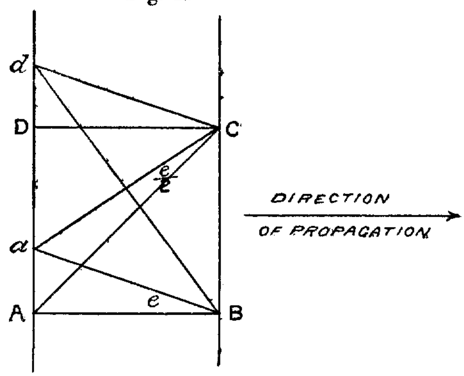
* Maxwell's 'Electricity and Magnetism,' 3rd edition, vol. ii. p. 441, footnote.

This excess in the compression half of a wave-train is connected with the extra speed which exists in that half, and makes the crests of intense sound-waves gain on the troughs.

Lord Rayleigh*, using Boyle's Law, has shown that the average excess on a surface reflecting sound-waves should be equal to the average density of the energy just outside ; and I think the same result can be obtained by his method if we use the adiabatic law. But the subject is full of pitfalls, and I am by no means sure that the result is to be obtained so easily as it appears to be. It is perhaps worth while to note one of these pitfalls, of which I have been a victim. It is quite easy to obtain the pressure against a reflecting surface by supposing that the motion just outside it is harmonic. But the result comes out to $(\gamma+1)$ energy-density, where γ is the ratio of the specific heats. Lord Rayleigh kindly pulled me out of the pit into which I fell, pointing out that when we take into account second-order quantities the ordinary sound equation does not hold. In fact we cannot take the disturbance as harmonic, and the simple mode of treatment is illusory.

The pressure in transverse waves in an elastic solid is, I think, to be accounted for by the fact that when a square,

Fig. 1.



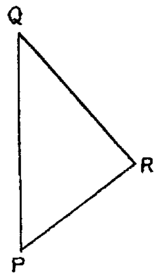
ABCD, is sheared into the position aBCd (fig. 1) through an angle e, the axes of the shear, aC and Bd, no longer make 45° with the planes of shear AD, BC. Since AC$a = \frac{e}{2}$, the pressure-line aC is inclined at $45° - \frac{e}{2}$ to the direction of propagation, and the tension-line at $45° + \frac{e}{2}$ to that line.

* Phil. Mag. iii. 1902, p. 338, "On the Pressure of Vibrations."

The result is a small pressure perpendicular to the planes of shear, that is, in the direction of propagation; and this small pressure is just equal to the energy-density of the waves.

For let PQR (fig. 2) be a small triangular wedge of the solid, PQ being a plane of shear perpendicular to the direction of propagation. Let this wedge have unit thickness perpendicular to the plane of the figure. Let PR be along a pressure-line and QR along a tension-line, and let pressure and tension each be P. Resolve the forces on PR and QR perpendicular to PQ. Then we have a force from right to left,

Fig. 2.



$$\mathrm{P}.\,\mathrm{QR}\cos\mathrm{PQR}-\mathrm{P}.\,\mathrm{PR}\cos\mathrm{QPR}$$

$$=\mathrm{P}.\,\mathrm{PQ}\left(\cos^2\left(45^\circ-\frac{e}{2}\right)-\cos^2\left(45^\circ+\frac{e}{2}\right)\right)$$

$$=\mathrm{P}.\,\mathrm{PQ}\,.\,e.$$

Thus, to prevent motion in the direction of propagation there must be a pressure on PQ equal to $\mathrm{P}e=ne^2$, where n is the rigidity modulus. But the strain-energy per unit volume is $\frac{ne^2}{2}$, and the kinetic energy is equal to it. The total energy-density is therefore ne^2, and the pressure is equal to this.

The pressure of elastic solid waves appears to be beyond experimental verification at present. But that of sound-waves has been demonstrated most successfully by Altberg*, working in Lebedew's laboratory at Moscow.

A small wood cylinder, 21 mm. diameter, was suspended at one end of a torsion arm, with its axis horizontal and transverse to the arm. One end of the cylinder occupied a circular hole in the middle of a board, there being just sufficient clearance to allow it to move; and the plane end was flush with the outer surface of the board. When very intense sound-waves 10 cm. in length, from a source 50 cm. distant, impinged on the board, the cylinder was pushed back, the pressure sometimes rising to as much as 0·24 dyne/sq. cm. The intensity of the sound was measured independently by the vibrations of a telephone-plate, in a manner devised by M. Wien; and through a large range it was found that the pressure on the cylinder was proportional to the intensity indicated by the telephone manometer.

Just lately Professor Wood † has devised a strikingly

* *Ann. der Physik*, xi. 1903, p. 405.
† *Phys. Zeitschrift*, 1 Jan. 1905, p. 22.

simple experiment to illustrate sound-pressure. The sound-waves from strong induction-sparks are focussed by a concave mirror on a set of vanes like those of a radiometer, and when the focus is on the vanes as they face the waves the mill spins round.

Theory and experiment, then, justify the conclusion that when a source is pouring out waves, it is pouring out with them forward momentum as well as energy, the momentum being manifested in the reaction, the back pressure against the source, and in the forward pressure when the waves reach an opposing surface. The wave train may be regarded as a stream of momentum travelling through space. This view is most clearly brought home, perhaps, by considering a parallel train of waves which issues normally from a source for one second, travels for any length of time through space, and then falls normally on an absorbing surface for one second. During this last second, momentum is given up to the absorbing surface. During the first second, the same amount was given out by the source. If it is conserved in the meanwhile, we must regard it as travelling with the train.

Since the pressure is the momentum given out or received per second, and the pressure is equal to the energy-density in the train, the momentum-density is equal to the energy-density ÷ wave-velocity.

This idea of momentum in a wave train enables us to see at once what is the nature of the action of a beam of light on a surface where it is reflected, absorbed, or refracted, without any further appeal to the theory of the wave-motion of which we suppose the light to consist *.

It is convenient to consider the energy per linear centimetre in the beam, and the total pressure force, equal to this linear energy-density, so as to avoid any necessity for taking into account the cross section of the beam.

Thus, in total reflexion, let a beam AB (fig. 3) be reflected along BC, and let AB = BC represent the momentum in each in length V equal to the velocity of light.

Produce AB to D, making BD = AB.

Then DC represents the change in the momentum per second due to the reflexion—the force on the beam, if such language is permissible; and CD is the reaction, the total light-force on the surface.

If there is total absorption, let AB (fig. 4) represent the

* A discussion, on the electromagnetic theory, of the forces exerted by light is given by Goldhammer, *Ann. der Phys.* 1901, 4. p. 483.

momentum of the incident beam. Resolve AB into AE parallel and EB normal to the surface. Then, since the momentum AB disappears as light-momentum, there must

Fig. 3.

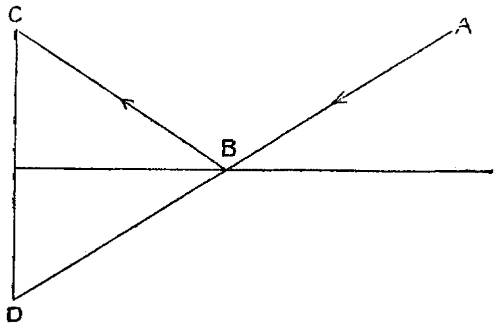


Fig. 4.

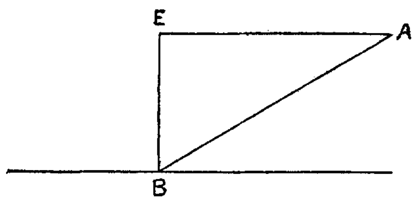
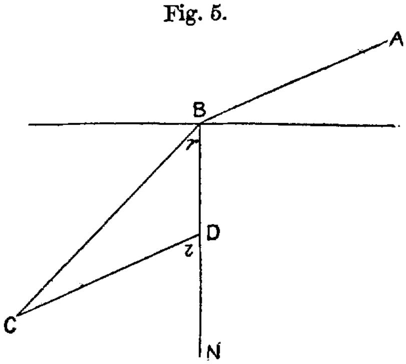


Fig. 5.



be a normal force EB on the surface and a tangential force AE parallel to the surface. I have lately* described an experiment which shows the existence of the tangential force AE.

* Phil. Mag. Jan. 1905, p. 169.

If there is total refraction, let AB (fig. 5) be refracted along BC. If E is the energy in unit length of AB, and if E′ is the energy in unit length of BC, the equality of energy in the two beams is expressed by

$$VE = V'E'.$$

But if M is the stream of momentum passing per second along AB, and if M′ is that along BC,

$$M = E \quad \text{and} \quad M' = E'.$$

Whence $$VM = V'M'$$

and $$M' = \frac{V}{V'} M = \mu M.$$

Let AB = M, and BC along the refracted beam = M′ = μM = μAB.

Draw CD parallel to BA, meeting the normal BN in D. Then

$$CD = CB \sin r / \sin i = \frac{CB}{\mu} = AB = M.$$

Hence by the refraction, momentum DC has been changed to momentum BC, or momentum BD has been imparted to the light. There is therefore a reaction DB on the surface. The force DB may be regarded as a pull-out or a pressure from within, and it is along the normal *.

If the refraction is from a denser to a rarer medium, CB will now represent the incident stream and BA or CD the refracted stream. BD is the stream added to CB to change it to CD, and DB is the force on the surface, again a force outwards along the normal.

In any real refraction with ordinary light, there will be reflexion as well as refraction. The reflexion always produces a normal pressure, and the refraction a normal pull. But with unpolarized light, a calculation shows that the refraction pull, for glass at any rate, is always greater than the reflexion push, even at grazing incidence.

The following table has been calculated from Fresnel's formula for unpolarized light by Dr. Barlow :—

* It has been pointed out by J. J. Thomson, 'Electricity and Matter,' p. 67, "that even when the incidence of the light is oblique, the momentum communicated to the substance is normal to the refracting surface." The change of momentum of a beam of light is, it may be noted, the same on the wave and on the corpuscular theory.

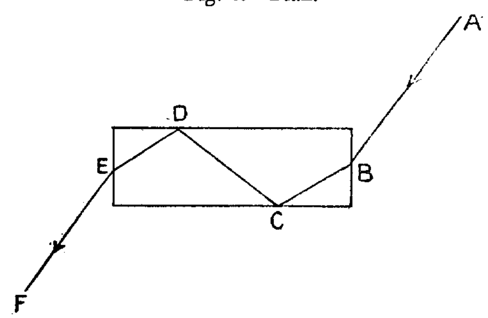
P = total pull on surface.
M = momentum per second in incident beam.
R = reflexion coefficient for angle i.
$\mu = 1·5$.

i.	R.	P/M.
0	·0400	·4000
20	·0402	·4240
40	·0458	·4925
50	·0572	·5310
60	·0893	·5720
65	·1205	·5771 Maximum.
70	·1710	·5683
75	·2531	·5329
80	·3878	·4521
89	·9044	·0738
$90-d\theta$	......	$2\mu^2 d\theta$
90	1·0000	·0000

If a ray of light passes obliquely through a parallel plate, there is a normal pull outwards at incidence and a normal pull outwards at emergence; and if the refraction were total, this would result in a couple. But since some of the light returns into the first medium, it is easy to see that the net result is a normal repulsion and a couple.

An experiment which I have lately made in conjunction with Dr. Barlow will serve as an illustration of the idea of a beam of light regarded as a stream of momentum. A rectangular block of glass, 3 cm. × 1 cm. × 1 cm., was suspended

Fig. 6.—Plan.



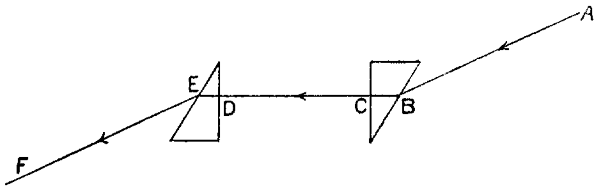
by a quartz fibre so that the long axis of the block was horizontal. It hung in a case with glass windows, which was exhausted to about 15 mm. of mercury. A horizontal beam of light from either a Nernst lamp or an arc was directed on to one end of the block so that it entered centrally as AB in fig. 6, and at an angle of incidence about 55°. After two internal reflexions it emerged centrally as EF from the other end. Thus a stream of momentum AB was shifted parallel to itself into the line EF, or a counter-clockwise couple acted on the beam. The reaction was a clockwise couple on the block. Using mirror, telescope, and a millimetre-scale about 184 cm. distant, a very small deflexion could just be detected with the strongest light and in the right direction. But the quartz fibre was rather coarse, indeed needlessly strong; and as the time of vibration was only 39 seconds, the deflexion was very minute. To render the effect more evident we used intermittent passage of the beam, sending it in during the half-period of vibration while B was moving from A, and shutting it off while B was moving towards A. The swings then always increased. When the beam was sent in during the approaching half and shut off during the receding half, the swings always decreased, and always rather more rapidly than they increased during the first half. For in the first case the natural damping acted against the light couple, and in the second with it. In one experiment the average increase was ·55 scale-division and the average decrease ·61 per period, and was fairly regular in each case. The mean was ·58. The steady deflexion is half this, or 0·29 division, giving a couple 11×10^{-6} cm.-dyne. We made a measurement of the energy in the beam by means of the rate of rise of a blackened silver disk; but it was necessarily very inexact, as we had no means of securing constancy in the arc used in this experiment. This energy measurement gave as the value of the couple 6×10^{-6}, and the agreement is sufficient to show that the order of the result is right.

An analysis of this experiment shows that the couple was really due to the pressures at the two internal reflexions; for, as we have seen, the forces at incidence at A and emergence at E are normal and produce no twist.

Another experiment which we have made is, I think, more interesting, in that it brings into prominence the pull outwards or push from within occurring on refraction. Two glass prisms, each with refracting angle 34°, another angle being a right angle, and with refracting edge 1·6 cm. long, were arranged as in fig. 7 (which shows the plan) at the ends of a thin brass torsion-arm suspended at its middle point from a quartz fibre in the same case as that used in the last experiment. The two inner faces were 3 cm. apart, and their width was 1·85 cm. A mirror gave the reflexion of a millimetre-scale 171·4 cm. distant. The moment of inertia of the system was 48 gm.-cm.^{2}, and the time of vibration was 317 seconds.

The air-pressure was reduced as before. When a beam of light from a Nernst lamp was sent through the system, as shown in the figure, it was shifted parallel to itself through a distance about 1·64 cm. The torsion-arm moved round clockwise by

Fig. 7.—Plan.



an easily measurable amount. In one experiment the deflexion was 3·3 scale-divisions, indicating a couple $1{\cdot}84 \times 10^{-5}$ cm.-dyne. The same beam directed on to the blackened silver disk gave the linear energy-density as $9{\cdot}8 \times 10^{-6}$, which should have given a couple $1{\cdot}6 \times 10^{-5}$. Though the agreement is perhaps accidentally close, yet, as we could use a Nernst lamp, the measurements were much more trustworthy than in the last experiment.

The interesting point here is that the effect could only be produced by a force outwards at B and E. Whatever forces exist at C and D would be normal to the surfaces and would give no twist.

A very short experience in attempting to measure these light-forces is sufficient to make one realise their extreme minuteness—a minuteness which appears to put them beyond consideration in terrestrial affairs, though I have tried to show* that they may just come into comparison with radiometer-action on very small dust particles.

In the Solar system, however, where they have freer play and vast times to work in, their effects may mount up into importance. Yet not on the larger bodies ; for on the earth, assumed to be absorbing, the whole force of the light of the sun is only about a 50 million-millionth of his gravitation-pull. But since the ratio of radiation-pressure to gravitation-pull increases in the same proportion as the radius diminishes if the density is constant, the pressure will balance the pull on a spherical absorbing particle of the density of the earth if its radius is a 50 billionth that of the earth—a little over a hundred-thousandth of a centimetre, say, if its diameter is a hundred-thousandth of an inch.

* 'Nature,' Dec. 29, 1904, p. 200.

We may illustrate the possible effects of radiation pressure without proceeding to such fineness as this. Let us imagine a particle of the density of the earth, and a thousandth of an inch in diameter, going round the sun at the earth's distance. There are two effects due to the sun's radiation. In the first place, the radiation-push is $\frac{1}{100}$ of the gravitation-pull; and the result is the same as if the sun's mass were only 99/100 of the value which it has for larger bodies like the earth. Hence the year for such a particle would be longer by $\frac{1}{200}$, or about 367 instead of $365\frac{1}{4}$ days. In the second place, the radiation absorbed from the sun and given out again on all sides is crushed up in front as the particle moves forward and is opened out behind. There is thus a slightly greater pressure due to its own radiation on the advancing hemisphere than on the receding one, and this appears as a small resisting force in the direction of motion. Through this the particle tends to move in a decreasing orbit spiralling in towards the sun, and at first at the rate of about 800 miles per annum.

Further, if there be any variation in the sun's rate of emitting energy, there will be a corresponding variation in the increase of the year and the decrease of the solar distance, and the particle, if we could only observe it, would form a perfect actinometer.

Though, unfortunately, we cannot observe the motion of independent small particles circling round the sun at the distance of the earth, there is good reason to suppose that some comets at least are mere clouds of dust. If we are right in this supposition, they should show some of these effects. Encke's comet at once suggests itself as of this class; for, as everyone knows, it shortens its journey of $3\frac{1}{2}$ years round the sun on every successive return, and on the average by about $2\frac{1}{2}$ hours each revolution. Mr. H. C. Plummer * has lately been investigating this comet's motion; and he finds that if it were composed of dust particles, each of the earth's density and about $\frac{1}{50}$ mm. or rather less than a thousandth of an inch in diameter, the resisting force due to radiation pressure would account for its accelerating return. But the sun's effective mass would be reduced by about 1/80; and on certain suppositions he finds that the assumed mean distance as calculated from Kepler's law, without reference to radiation, is greater than the true mean distance by something of the order of 1 in 400, and he thinks such a large error is hardly possible. So

* Monthly Notices R.A.S., Jan. 1905, "On the Possible Effects of Radiation on the Motion of Comets, with special reference to Encke's Comet."

that radiation pressure has not yet succeeded in fully explaining the eccentricities of this comet. But comets are vague creatures. As Mr. Plummer suggests, we hardly know that we are looking at the same matter in the comet at its successive returns; and I still have some hope that the want of success is due to the uncertainty of the data.

There is one more effect of this radiation pressure which is worthy of note: its sorting action on dust particles. If the particles in a dust cloud circling round the sun are of different sizes or densities, the radiation accelerations on them will differ. The larger particles will be less affected than the smaller, will travel faster round a given orbit, and will draw more slowly in towards the sun. Thus a comet of particles of mixed sizes will gradually be degraded from a compact cloud into a diffused trail lengthening and broadening, the finer dust on the inner and the coarser on the outer edge.

Let us imagine, as an illustration of this sorting action, that a planet, while still radiating much energy on its own account, while still in fact a small sun, has somehow captured and attached to itself as satellite a cometary cloud of dust. Then, if the cloud consists of particles of different sizes, while all will tend to draw in to the primary, the larger particles will draw in more slowly. But if the larger particles are of different sizes among themselves, they will have different periods of revolution, and will gradually form a ring all round the planet on the outside. Meanwhile the finer particles will drift in, and again difference in size will correspond to difference in period and they too will spread all round, forming an inner fringe to the ring. If there are several grades of dust with gaps in the scale of size, the different grades will form different rings in course of time. Is it possible that here we have the origin of the rings of Saturn?

The Radiation Theory is only just starting on its journey. Its feet are not yet clogged by any certain data, and all directions are yet open to it. Any suggestion for its future course appears to be permissible, and it is only by trial that we shall find what ways are barred. At least we may be sure that it deals with real effects and that it must be taken into account.