subsist, because it is dynamically unstable. It does not, then, seem worth while to consider the remarks made on that passage.

With regard to the first proposition that, if the moon separated from the earth near the present earth's surface, it can only have subsisted as a flock of meteorites, my own words may be quoted as follows :—

"The planet then separates into two masses, the larger being the earth and the smaller the moon. I do not attempt to define the mode of separation, or to say whether the moon was initially more or less annular. At any rate it must be assumed that the smaller mass became more or less conglomerated, and finally fused into a spheroid, perhaps in consequence of impacts between its constituent meteorites, which were once parts of the primæval planet. Up to this point the history is largely speculative, for, although the limiting ellipticity of form of a rotating mass of fluid is known, yet the conditions of its stability, and *a fortiori* of its rupture, have not as yet been investigated. . . . At some early stage in the history of the system the moon has conglomerated into a spheroidal form."[1]

When, however, Mr. Nolan goes on to his second proposition, and states that this amounts to saying that the moon must have been a ring of fragments revolving in the plane of the equator, and that such a ring must be uniformly distributed and therefore incompetent to raise frictional tides, it is not easy to follow him. Is there any objection to the existence of a flock of meteorites? And would not such a flock raise tides in the planet which, if subject to friction, would introduce forces tending to make the meteorites recede? It seems that there is no such objection, and that the flock of meteorites would follow the same fate as the satellite when conglomerated in a single mass.

The difficulties which are raised by the author in the conception of the conglomeration are such as meet us in all evolutionary theories, and whether or not it is possible as yet to see our way mentally through the changes which may have taken place, yet it is generally admitted that conglomeration took place in some way.

He then points out that no other satellite is traceable up to the surface of its planet, and concludes that it is a coincidence that the masses and periods of the moon and earth are apparently such as fit into the theory. No one has pointed out the non-existence of such a satellite more clearly than I,[2] but the absence of reference to my work seems to show that Mr. Nolan has not looked at it. It is not then surprising to read: "Is it not very illogical to suppose that the moon originated in a way which cannot have been the way of origin for other planets and satellites?" And the reader of this sentence would hardly think that my position is that there is a probability that a cause which was subordinate in the history of the other planets was predominant in the case of the moon and earth, and that it is proved numerically that in the terrestrial sub-system the actual distribution of masses and momenta (the factors governing tidal friction) differs at least as much from the corresponding factors in the other planetary sub-systems as the supposed modes of evolution.

On p. 13 we read:—

"There is a law, according to which two heavenly bodies cannot revolve about their centre of gravity with their surfaces nearly in contact, unless one be smaller than the other by a certain amount, and, further, that the small one be denser than its companion by a certain value."

I do not know where to find the proof of such a law, and at the present moment am disposed to doubt its correctness.

Next, on p. 14, we find:—

"Rapid rotation would never cause a quantity of the matter of a body to become piled up at one particular place, and form into a separate single body there of any appreciable size."

Now very recently M. Poincaré has rigorously proved in a very remarkable paper[1] the possibility and even the dynamical stability of such a "piling up," and has given a sketch of the mode of separation of a portion of the mass of rotating fluid. In a paper of my own, now nearly finished, the same problem is treated, but from a different point of view.

It will be perceived from the quotations that the pamphlet is true to its title, and refers almost entirely to the genesis of the moon. This affords some proof that my speculative remarks hazarded as to the mode of origin of the moon, were not so guarded as was intended. The justice of the third of Mr. Nolan's propositions may, however, be denied, and certainly the theory cannot be held to depend on the genesis of the moon at the *present* surface of the earth.

The present opportunity will be convenient for a short reiteration of my point of view with regard to the whole subject.

In tidal friction we have a *vera causa* of modifications in the configuration of the earth and moon. If we adopt provisionally the hypothesis of an adequate lapse of time, we can trace the changes, and find that the obliquity of the ecliptic, the eccentricity of the lunar orbit, and its inclination to the ecliptic (all unmentioned by Mr. Nolan), the lunar periodic time, and that of the earth's rotation, are co-ordinated together by supposing that the moon first had a separate existence at no great distance from the present surface of the earth, and with small differential motion with respect thereto. Then it is maintained that this co-ordination is so remarkable as to give good reason for accepting the hypothesis as in accordance with truth. Concerning the earlier stage in which the moon may be supposed to have separated from the earth, nothing more than conjecture is possible, but undoubtedly the condition adduced by Mr. Nolan escaped my notice.

In examining the rest of the solar system, it is found that, amongst other things, the Martian satellites afford a striking confirmation of the influence of tidal friction, and that the system of the moon and earth presents features so distinct from those of the other planets, as to justify the belief that tidal friction, subordinate in its influence on the other systems, may have been predominant in our own. The theory is also found to throw light on the distribution of satellites in the solar system.

It is as yet too soon to say how far these views embody the truth, but even should they be found untenable, yet certainly the determination of the effects of tidal friction on a system of planets and satellites is a problem of physical astronomy which was well worthy of attack.

G. H. DARWIN

[1] *Phil. Trans.*, part 2, 1880, pp. 880-81.

[2] "On the Tidal Friction of a Planet attended by several Satellites" *Phil. Trans.*, part 2, 1881).

[1] *Acta Mathematica* (1885), 7, 3, 4, "Sur l'équilibre d'une masse fluide animée d'un mouvement de rotation."

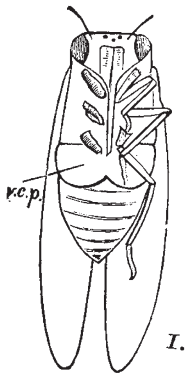
ON THE SOUND-PRODUCING APPARATUS OF THE CICADAS

HAPPENING to refer to Prof. Jeffrey Bell's "Comparative Anatomy and Physiology" on the question of the sounds produced by insects, I read, with reference to the Cicadas :—"The sound seems to be produced by the vibration of membranes, placed on either side of the stigmata of the metathorax, and set in motion by the respiratory air" (p. 389).

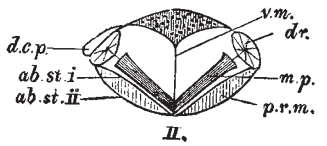
As this wind-instrument theory of Landois seems to be supplanting, in our text-books and popular natural histories (*e.g.* Cassell's), the drum theory advocated by Réaumur and the earlier writers, I think it permissible to draw attention to certain observations I made on this

subject some six years ago. They were published in the not readily accessible *Proceedings* of the South African Philosophical Society (1879-80, part iii. p. 161), and were not illustrated by any figure.

The Singerjie (*Platypleura capensis*) is a well-known and tolerably abundant insect at the Cape; and few visitors to the shores of Table Bay can have failed to notice, in the hotter months of the year, the sharp shrill metallic sound produced by the "little singer." It is soon found that the male Cicada alone possesses the power of singing, the female—recognised at once by the long ovipositor folded beneath the abdominal somites—being dumb. If the ventral surface of a male singerjie be examined (Fig. I.) two large ventral cover plates (*v.c.p.*) are seen, one on either side, meeting in the central line and extending backwards from the metathorax over the anterior abdominal somite. On turning the insect over and looking at the dorsal surface, two very much smaller dorsal cover plates are seen extending forward from either side of the first abdominal somite. If one of these plates be removed with fine pointed scissors, there is seen the wrinkled surface of a thickish chitinous membrane, the drum. Turning the insect over again, so as to examine more carefully the ventral aspect, and removing one of the ventral cover plates, two membranes are disclosed, separated by a transverse chitinous support. Of these the anterior is white, narrow, and opaque, the posterior (*p.r.m.*, Fig. II.) translucent, oval, and tightly stretched.



I.



II.

The transverse chitinous support (*ab.st.*i) is the sternum of the first abdominal somite; the membranes are intersternal membranes which would seem to be specially modified to act as resonators. The second ventral cover plate may now be removed, disclosing the anterior and posterior resonator membranes of that side; the anterior resonator membrane of each side may be cut through; and the abdominal portion of the insect may be separated from the thorax. When this is done there are seen, taking their origin from the mid-line of the first abdominal sternum (Fig. II., *ab.st.*i), two muscular pillars (*m.p.*), each of which, proceeding upwards and outwards, terminates in a chitinous plate, the upper surface of which is, in turn, connected by a fine ligament with the drum (*dr.*). Under a low magnifying power this drum is seen to be strengthened with brownish ribs, which, together with its general elasticity, cause it to spring back after it has been drawn forward by the action of the muscular pillars, the fibres of which are beautifully striated. Each time the drum is drawn forward and springs back, by the alternate contraction and relaxation of the muscular pillars, a sharp click is heard, as may readily be proved experimentally on the dead insect. That the well-known metallic sound is produced by a rapid succession of such clicks is put beyond question by the fact that, by irritating the muscular pillars in a freshly killed insect, the singing noise may be set agoing, and will then continue for several seconds or even minutes. This I had the pleasure of demonstrating to Mr. Roland Trimen, F.R.S., curator of the Cape Town Museum, the abdomen singing merrily when the head and thorax had been pitched out of window or destroyed. A weak current of electricity would also cause the singing to commence. The sound generally ceased after a while in a few isolated clicks, and at that time the waves of contraction in the muscular pillars were plainly visible. The singing noise was less sharp and clear than in the living insect, owing probably to the disruption of some of the resonator membranes; and I have noticed that rhythmical motions of the abdomen in the live insect produce rhythmical alterations in the intensity of the sound. The cover plates are doubtless mainly for protection; but the fact that on their removal the sound is less full and intense shows that they also may play the part of sounding-boards.

Fig. II. is a slightly diagrammatic view of the severed abdomen as seen from the anterior end. *ab.st.*i and ii are the sterna of the first and second abdominal somites; *p.r.m.* the posterior resonator membrane stretched between them; *v.m.* a vertical membrane; *m.p.* the muscular pillars; *dr.* the drum; and *d.c.p.* the dorsal cover plate of the right side, that of the left side being removed.

I may mention, by way of appendix, that in this species the rostrum contains only three stylets: two lateral, toothed on their external edges; one central and smooth. Although this central style shows, in some cases, indications of its having arisen by coalescence of two lateral styles, it is distinctly one and indivisible.

C. LLOYD MORGAN

University College, Bristol

RADIANT LIGHT AND HEAT[1]

V.

Evidence afforded by the Spectroscope as to the Nature of the Elements

THE point of greatest speculative interest connected with our subject is perhaps that regarding the constitution of the so-called elements.

What light, it may be asked, does spectrum analysis throw on this vexed question? Does it lead us to imagine that these bodies are truly elementary? Or to believe that they are in reality compounds which might be broken up if we had sufficiently powerful instruments for this purpose at our disposal?

I shall begin my remarks on this subject by taking it for granted that the constitution of matter is atomic and molecular.

When, therefore, two chemically different substances combine together we have the union of two heterogeneous atoms, forming a molecule of the compound substance. Thus, when an atom of chlorine and one of sodium combine together we have, as a result of the combination, chloride of sodium or common salt, and an ultimate molecule or compound atom of chloride of sodium may be described as the smallest portion of that substance which possesses all the properties of common salt, and which, if divided further, would be split up into one atom of sodium and one of chlorine.

The molecules of bodies are very frequently so placed as to have an attraction for each other, under the influence of which the body assumes a solid, or, it may be, a liquid state.

Sometimes, however, we have the body in the state of gas, in which the various molecules are so far apart as to have no perceptible attraction for each other. It is by means of such a gas that we can best study the properties of molecules as far as radiant light and heat are concerned. Now, spectrum analysis unquestionably tells us that at comparatively low temperatures and great nearness of particles we have a comparatively complicated

[1] Continued from p. 254.