so that there is a clear gain, which may be expended in flight until the speed is so much reduced that it is necessary to increase it by another ascent.

The reason is clear why, when following a steamer against the wind, the birds are compelled to repeat at short intervals the movements by which speed through the air is gained, and why, when the wind is favourable, the intervals are longer. The great length of these intervals just mentioned seems to me the difficult point in the theory. It cannot be proved that the speed acquired by the movements described is sufficient for such long flights between the ascents as are sometimes seen, under conditions where there is no suspicion of upward currents near the surface. Our knowledge of the resistance of the air to such complex surfaces as the wings of birds is, I believe, almost nothing; and even if we knew, without doubt, the true explanation of the manner in which the energy lost by resistance is renewed, we might find it hard to apply it to all cases, unless we had some real knowledge of the supporting power of the air, and the horizontal resistance at different velocities.

I have sometimes seen a number of albatrosses sailing in a peculiar manner, the wind being at right angles to the course of the steamer. They ascend against and descend with the wind, turning alternately right and left, so as to describe an undulating line, not far behind the stern. A number of them sometimes do this for hours, while others are sailing in various directions farther off. It is curious that the common sea-gulls of New Zealand (*Larus dominicanus*), which have become to a great extent land-birds since the country was colonized, may be sometimes seen making their way over flat country in the same manner as the albatrosses just described—that is, at right angles to the wind, and turning alternately right and left, and nearly touching the ground at each descent. Their success in doing the sailing flight is, however, imperfect, as they seem compelled to flap their wings a few times during the second half of each ascent. Perhaps future generations of gulls may improve. No doubt some muscular exertion is saved by this mode of progression. The foregoing theory of the sailing flight of the albatross shows, I think, the action of a *vera causa*, which, as far as I know, has not been noticed before. A. C. BAINES.

Christchurch, New Zealand.

Note on *Ragadia crisia*.

MR. W. L. DISTANT, in his admirable "Rhopalocera Malayana," calls attention to the recent appearance of *Ragadia crisia* in the Malay peninsula. As I have had opportunities of studying the habits of the species in what seems to be one of its head-quarters, it may be desirable to record the facts.

Mr. Distant writes as follows:—"One of the most peculiar facts in relation to this butterfly seems to be its almost recent appearance in the Malay peninsula, or at all events its first capture there by collectors. I did not meet with it myself when collecting at Province Wellesley, nor did I subsequently receive it in numerous collections derived from the peninsula. In 1883, however, the species seems to have been common from Penang to Singapore. I first received two specimens captured on Penang Hill, and sent to me as a new species; others shortly followed from Province Wellesley, with the remark of an experienced collector that the species was quite new to the locality; and almost simultaneously the Indian mail brought me more examples from Sungei Ujong, Malacca, and Singapore. My friend Mr. Logan also sent me an example with the comment, 'a very rare butterfly not known to collectors here.'

"Capt. Godfrey, who also captured the species at Sungei Ujong, described it as being found in low undergrowth in the forest, where, especially in the early morning, I several times met with it. Its flight is weak and feeble, but it cleverly eludes pursuit by threading its way through tangled brushwood" ("Rhop. Mal.," p. 421).

Mr. W. B. Pryer, in his joint paper with Mr. Distant ("Rhop. Northern Borneo," *Ann. Mag. Nat. Hist.*, January 1887, p. 49), describes it as "rare, under almost thick forest shade." In ten years' collecting in North Borneo he has only met with a few specimens, not more than a dozen, nearly all of which he saw at Silam, on the coast of the district I write about.

I find it to be the very commonest butterfly in the dense forest of the centre of Darvel Bay Peninsula, on the east coast of British North Borneo.

I first saw the insect in the deep forest between Lamag on the River Kinabatangan and Itok Batu on the River Segama, about 120 miles inland. It was not common, but I always saw one or two daily. Since then Mr. Pryer has taken it higher up the River Kinabatangan, some 250 miles up stream. The butterfly is still quite rare on the coast, the only specimens having fallen to Mr. Pryer's net.

Last year I made an exploration through the forest from the River Tinkyo in Darvel Bay to the head-waters of the River Segama. Within four miles of the coast, in the alluvial flat of the River Tinkyo, the species was seen daily, but was far from common. As soon as we touched the mountain country it began to grow common, and from 600 feet to 2500 feet above sea-level it was the commonest butterfly in the deep forest.

Capt. Godfrey's description of its habits agrees with mine, except in one particular. He found it most plentiful in early morning. I was always in the jungle from soon after dawn to near dusk, and found it appeared about 9 o'clock in the morning, and was on the wing till about 4 o'clock in the afternoon.

It has the feeblest (and wickedest) flight of any butterfly known to me. I never saw it rise six feet above the ground, and it flaps slowly along, apparently with effort, its wings not stiff but bending with each stroke. It *looks* a certain capture; but this, as Capt. Godfrey found, is delusive and elusive. It keeps just above the low bushes from two to three feet high, and sneaks in among them most exasperatingly. It seems to do this deliberately, and will rather circuit round an opening made by a fallen tree than cross the small opening. It is often seen flying in rain.

As a rule it is quite solitary, it being rare to see two at once, and it is not at all bold or pugnacious.

Its wings are so soft that it often crumples in the wet, and it is almost impossible to set it during the rainy season, the wings relaxing in a few hours, though it may have been a week on the setting-board.

From February to October it was equally common. I then came out of the forest. It probably flies all the year round.

It is one of the few true forest species, and avoids the sunshine.

I do not know whether the insect is common elsewhere, and can offer no suggestion as to why it should be spreading from this part, though it is undoubtedly creeping coastwards. The eastern part of North Borneo is untouched primæval forest, the only clearings being on the coast and river-banks, and these are small. The country where *Ragadia* abounds is quite uninhabited, and it is difficult to see how the food-plant (unknown) could have been taken thence to the Straits Settlements. Then too, in Borneo at any rate, it would avoid clearings.

Leaving this question for future observers to solve, we now know that in one part of the interior of North Borneo *Ragadia crisia* is very common, and it is extending its area.

SYDNEY B. J. SKERTCHLY.

Spherical Eggs.

PROF. ALDIS will find references to the history of this ancient question in an article by Mr. W. Walton in the *Quarterly Journal of Mathematics*, vol. ix. p. 79, where it appears as Leslie Ellis's problem of the thirsty crow. "A thirsty crow flew to a pitcher and found there was water in it, but so near the bottom he could not reach it. Seeing, however, plenty of small, equal spherical pebbles near the place, he cast them one by one into the pitcher, and thus by degrees raised the water up to the very brim and satisfied his thirst. Prove that the volume of water must have been to that of the pitcher in a ratio not less than $3\sqrt{2} - \pi : 3\sqrt{2}$." References are supplied in the article by the Rev. Dr. Luard to Pliny's "Natural History," book x. chapter 43, "De Corvorum Intelligentiâ"; Plutarch, "De Solertiâ Animalium; and Ælian, "De Naturâ Animalium." Consult also Tait's "Properties of Matter."

Thus in an aggregation of closely-packed equal spherules the unoccupied space is $1 - \frac{1}{6}\pi\sqrt{2}$ of the whole volume.

We may verify this experimentally by comparing the weight of a given volume of small lead shot with the weight of an equal volume of lead; theoretically the densities should be as π to $3\sqrt{2}$.

On a larger scale, the question may be studied in the piles of spherical shot formerly to be seen in our forts and arsenals. Whether we begin piling the shot in horizontal layers, in triangular order, or in square order, the internal molecular arrangement of the spheres is the same; but the square order in the horizontal layers is preferred, as it is then possible to build the pile on a rectangular base, finishing off at the top with a ridge

of balls in single file, the sloping faces all showing the spheres in triangular order.

Suppose a bag, impermeable to water, is filled with lead shot, placed in an hydraulic press, and subjected to great pressure. The lead spheres will be flattened against each other in regular cell structure into a solid mass, each sphere being changed into a *rhombic dodecahedron*; and in this manner the form of the cell of the bee has been considered as arising in a natural manner by Mrs. Bryant, D.Sc., in a paper read before the London Mathematical Society, vol. xvi., "On the Ideal Geometrical Form of Natural Cell-Structure." The plane surfaces of separation also form a possible arrangement of the films of a mass of soap-bubbles; but the instability of the corners where six edges meet modifies the soap-bubble arrangement to the form investigated by Sir W. Thomson in the *Acta Mathematica*.

April 27. A. G. GREENHILL.

Name for Unit of Self-Induction.

A NAME for the unit coefficient of self-induction is much wanted. No one is satisfied with secohm, and yet it seems making its way; by reason, no doubt, of Ayrton and Perry's ingenious commutating arrangement for helping to measure it. It is an unpleasing name, and it is too big a unit. The name quad, which I formerly suggested, is on further consideration still less satisfactory for permanent use, because it emphasizes unduly the accident that *in electro-magnetic measure* self-induction happens to be a length. One looks forward to the time when all distinction between electrostatic and electro-magnetic measures shall vanish by both ceasing to be; and at that not far-distant time, names emphasizing the present arbitrary state of things will be anachronisms, as well as stumbling-blocks to beginners. I beg to suggest that a milli-secohm shall be called a *vo*. It is a short and harmless unmeaning syllable not yet appropriated. It should be its own plural. The unit of conductivity is already styled a *mo*; and 8 vo will look well alongside 12 mo. "Vometer" is short and satisfactory. A unit of magnetic induction will then be the vo-ampere; and this, being of a size convenient for dynamo makers, may be hoped to replace their abominable mongrel unit "Kapp-lines."

The vo in electro-magnetic measure is 10 kilometres, and hence a vo ampere per square decimetre is a magnetic field of a thousand C.G.S. units, and might be called a "Gauss." For lightning-conductor work the natural unit of self-induction will be a milli-vo, or 10 metres of electro-magnetic measure.

Grasmere, April 16. OLIVER J. LODGE.

Hertz's Equations.

PERMIT me to add a line of explanation of my letter on this subject, printed in NATURE, vol. xxxix. p. 558. I intended no criticism of Hertz's general result, but merely to draw attention to the necessity of rejecting all solutions of the equation in Π which made the force (Z) infinite for points on the vibrator.

Berkswell, April 24. H. W. WATSON.

A NEW PEST OF FARM CROPS.

DURING the past three or four years, in the examination of plants affected by various injurious worms and Arthropods, and of the soils in which such plants grew, I have from time to time been led to suspect that certain small species of *Oligochæta* were concerned in damaging, if not ultimately destroying, several species of cultivated plants. With a view to converting suspicion into proof, experiments on isolated growing pot-plants have been carried on.

Within the past few weeks I have received, through the kindness of Miss E. A. Ormerod, additional evidence of a striking character, which induces me to place the main facts on record.

In the spring of 1885, Miss Ormerod forwarded to me for inspection two small white *Oligochæta*, 1½ inch long, received by her in soils from the roots of plants. In reporting on them I replied that it did not seem very probable that they could seriously injure the plants.

In April 1888, an inquiry reached me as to the nature and means of prevention of a serious attack of "small white worms" destructive to pot and green-house plants. On being placed in communication with the observer, the Rev. William Lockett, Rector of Littledean, I received from him a box of soil taken from his affected flower-pots, and much valuable information in answer to a series of questions put by me. The soil itself contained some hundreds of the white worms described; and the detailed information all pointed to these worms as the cause of many serious losses which had been sustained.

The worms were Enchytræidæ, of the genus *Enchytræus*, apparently near to *E. Buchholzii*, Vejd. I took three plants, a sunflower, a geranium, and a tradescantia, and had them re-potted in carefully examined sifted earth; when they were well established, I put fifteen of the worms into each pot, and left them to be tended by the gardener. I kept a number of the worms in soil which was alternately wetted and dried at regular intervals. They all kept alive and vigorous; when wet to complete immersion they were most active, when dried they remained quiescent, apparently dried up, and difficult to discover.

After two months, the sunflower drooped and bent over, and examination showed the roots and rootlets dead and the stem rotting. Within the decaying stem some of the Enchytræidæ were found alive and active. The other two plants are still living, but it will be shown that the number of worms supplied them was too small. Mr. Lockett lost spiræas, vegetable marrows, fuchsias, gloxinias, and many other plants, and the dead roots often contained in and around them many hundreds of worms to each plant. Both in his garden and a neighbouring ash-heap he found an abundance of them.

I was on the point of repeating my experiments this spring with various seedlings, when I received by the kindness of Dr. Gilbert, of Rothamsted (at the suggestion of Miss Ormerod), a quill with two or three specimens of worms of the same genus. Mr. John J. Willis, the superintendent of the field experiments at Rothamsted, in sending them described them as obtained from a field of clover "with a good plant except across one portion of the field, where all the plants were dying off," the small worms occurring at the roots of the clover along with larvæ of *Sitones* and wire-worm. "There is scarcely a plant that has not one or more of these creatures attached." Mr. Willis has been good enough to send me several communications on the subject, and a supply of the worms, living and in spirit. Much of his information is interesting, as that the more decayed the root, the larger the number of worms; that even healthy plants harbour a few specimens; that the worms seem sometimes to enwrap the rootlets with their coiled body. He hears of other fields of clover in a similar condition apparently to those at Rothamsted. I have a quantity of detailed information, but to summarize it, there appears to be but little room for doubt that these small *Oligochæta* are one cause of the decay of the clover at Rothamsted, as they were of the many varieties of garden plants at Littledean.

The Enchytræidæ have not hitherto, so far as I can learn, been accused of causing serious injury to plants. Vejdovsky, in his "Monographie der Enchytræiden," says, "Die Enchytræiden bewohnen trockene und feuchte Erde, süsses und salziges Wasser, Sümpfe und morsches Holz." In what manner they directly injure the plant remains to be observed—probably by sucking the fine root-hairs. Under observation the pharynx is rapidly everted and withdrawn in the act of feeding. I have so far recognized two species. If, as seems not improbable, further corroboration should be forthcoming, we may find that we have to add to the list of enemies of the clover plant from which it so mysteriously suffers, these unsuspected Oligochætes. The discovery, though fraught