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Vents single, one at the end of each conical lobe, each provided with a peristome, and all leading to a dilated central cavity or cloaca, whose holes are variable in size and distance apart, corresponding to the breadth of the skeletal layer of this cavity between them; subcircular, and presenting *within* respectively from one to three or more openings which belong to the wall-structure. Structure of the wall, which is about 1-23rd in. thick, like that of the foregoing species, but with the sagittal radiates still larger. Spicules of two kinds, viz. acerate and triradiate; no quadriradiates:—1, acerates of two forms, viz. one long, thin, straight, cylindrical, and the other minute, short, and also straight, averaging about 14 by $\frac{1}{3}$ -6000th in.; 2, triradiates, of different sizes and different degrees of irregularity, sagittal and otherwise, the largest averaging 225 by 27-6000ths in., with arms respectively about 150 by 8-6000ths in. No. 1 is confined to the peristome in its long thin form, and in its short minute one sparingly to the cribriform sarcodæ, where it constitutes the mortar-spicule; no. 2, viz. the triradiate, in its smaller form, which is still comparatively large, is confined to the structure of the surface and that of the cloaca, where, in the former, one ray often projects in such a manner that, if not carefully examined, it may be mistaken for a large acerate directed towards the mouth, and the other form, which is much more sagittal, to the wall, where its shafts stretch across this part from opposite sides, and thus overlap each other, while their arms support the skeletal structures of the surface and cloaca. Size of specimen about $\frac{1}{2}$ inch in its widest diameter.

Obs. The chief characters of this specimen are its large triradiates, whose projecting arms on the surface seem to replace the large curved acerates usually found there; also the absence of quadriradiates, and therefore of echinating spines, on the surface of the cloaca.

[To be continued.]

VII.—*Professor E. Ray Lankester's Memoir "Limulus an Arachnid," and the Pretensions and Charges founded upon it.* By Professor CARL CLAUS.

IN a recently published article, in the April number of this Journal, entitled "Professor Claus and the Classification of the Arthropoda," Prof. E. Ray Lankester has taken upon himself to bring a series of heavy accusations against me, and asserts that I have borrowed from his *Limulus*-memoir of the year 1881 the views expressed by me upon the classification of the Arthropoda, on the occasion of a communication upon the heart of the Acarina, which appeared in the 'Anzeiger

der kais. Akad. der Wiss. in Wien,' for 17th December, 1885, and in the number of this Journal for February 1886. I venture to reply as follows to these charges:—

1. The communication published in the 'Anzeiger' upon the relations of the Gigantostraca to the Arachnoidea, on the unnatural character of the division into Branchiata and Tracheata, and on the classification of the Arthropoda, is essentially nothing more than a repetition of my opinion as already published years ago. Even in the work entitled 'Untersuchungen über die genealogische Grundlage des Crustaceensystems' (Vienna, 1876) I adhered to the views of those who, like Straus-Dürckheim, regard *Limulus* and branchiate Gigantostraca as allied to the air-breathing Arachnoidea, and the latter as having proceeded from the former, although, having regard to the possibility of a still undemonstrated Nauplius-stage, I considered it probable that the common origin with the true Crustacea was rather after than before the Nauplius-period of the Stem-Crustacean. In the case of *Limulus* and the Scorpions I also asserted the homology both of the six pairs of limbs of the cephalothorax and, with reference to the developmental history, of the six pairs of limbs of the præabdomen, of which the second pair represent the comb-like organ of the Scorpions, while the following four pairs immediately undergo retrogression (p. 110). In the 'Grundzüge der Zoologie' of the year 1880 I went so much further as to divide the Branchiata, or Crustacea *sensu latiori*, into EUCRUSTACEA (with the Entomostraca and Malacostraca) and GIGANTOSTRACA (with no certain traces of the Nauplius-stage), and accordingly I affirmed expressly of the Tracheata *that in opposition to the more ancient Branchiata they "were not referable to a unitary origin, since the Arachnoidea, which are derivable from the Gigantostraca, stand opposite to the Myriapoda and Insecta, which are united by a closer affinity"* (p. 515). This implied not only that the division of the Arthropoda into Branchiata and Tracheata is an artificial one, inasmuch as the branchiate Crustacea and the air-breathing Arachnoidea meet together in a common origin, but also the denial of the unitary origin of the tracheæ, and the contrast of two series of Tracheata, the Arachnoidea on the one hand, and the Myriapoda and Insecta on the other.

In his *Limulus*-article E. Ray Lankester has entirely ignored the contents of my work of the year 1876, and referring to the 'Grundzüge,' cited by him, but with the contents of which he was certainly unacquainted, he misrepresents my views by the incorrect statement: "of the relationships of the Gigantostraca to Arachnida Claus says nothing." Although I will not reproach Prof. Ray Lankester with being so ill-

informed as to my opinion when he prepared his *Limulus*-article, he certainly ought since then, and before publicly bringing such serious accusations against me, to have made himself better acquainted with my writings.

2. In the excess of his zeal it has quite escaped Prof. Ray Lankester that my conception is very different from his, and has nothing at all to do with the assertions and conclusions contained in the *Limulus*-article, so far as these are *peculiar to him*. Not only do I treat the derivation of the Scorpions from the Gigantotraca merely as a probable one, but I also in those words appeal, in the first place, only to the insufficient evidence of the Crustacean nature of the latter (Crustacean in the sense of the Eucrustacea), in order, in the next sentence, to seek the data for their relationship to the Arachnoidea in *developmental history*. Consequently, even without citing the *Limulus*-article, I exclude, as arguments, the supposed data derived from the perfect organism.

Or has Ray Lankester forgotten the criticism passed upon the contents of his *Limulus*-article by no other than Packard, the author of an important work on the development of *Limulus*? Has it passed from his memory that Packard has demonstrated his parallelizations, almost point by point, to be constructions of the imagination? (see S. F. Packard, "Is *Limulus* an Arachnid?" 'American Naturalist,' 1882). But even in this case he ought not to have overlooked the fact that I do not refer to the agreements deduced from the form and structure of the perfect organism, and from this he ought to have concluded at least that I have no great confidence in them.

Let us now look a little more closely into the contents of the celebrated *Limulus*-article and the other writings of Ray Lankester related to it, in order to judge of the value of the evidence for regarding *Limulus* as an Arachnid which they contain.

In opposition to Ray Lankester's assertion that *Limulus* and *Scorpio* agree, segment for segment, Packard has shown from the development that in *Limulus* there are not eighteen but only fourteen segments present, and consequently that four segments are added as "metaphysical inventions." "Our author," adds Packard, "sets out with the foregone conclusion that he 'must' find in the abdominal carapace of *Limulus* the representatives of the twelve abdominal segments of the Scorpion; and so, with a method of his own, he creates them out of his inner consciousness." No better judgment is passed upon the homologization of the six pairs of limbs of the abdomen with the triangular sternite, the pectinate appendages, and the four pairs of lung-sacs of the Scorpion.

Although in accordance with my own comparison (published in 1876) I cannot see why the pectinate appendages cannot represent the second pair of limbs, I nevertheless entirely agree with Packard in regarding the attempt to refer the lung-sacs of the Scorpion to the introverted branchial laminae of the last four pairs of limbs, as mere trifling with baseless assumptions. In point of fact this exceedingly remarkable speculation (which its author has, however, replaced by a new one) furnishes us with a not very edifying example of the ingenious hypotheses into which an unbridled imagination may lead the morphologist.

It fares no better with the assertions as to the agreement between the brain, nervous system, and eyes in the two types. Packard shows Lankester to be in error when he shifts the origin of the pair of nerves which run to the anterior extremities in *Scorpio*, from the brain, as in *Limulus*, to the oesophageal ring; and in the same way he disputes the interpretation adopted by Ray Lankester to enable him to homologize the scattered simple eyes of the Scorpion with the lateral faceted eyes of *Limulus*.

This, however, by no means exhausts the list of errors and fallacies. *Limulus*, like the Scorpion, possesses a supra- or circum-medullary artery, which issues from the aorta and embraces the oesophagus. No Crustacean, says Ray Lankester, has such a spinal vessel, consequently *Limulus* is an Arachnid. But is our author so imperfectly acquainted with the anatomy of the Crustacea as to have no knowledge of the vascular system of the Isopoda, in which there is a peri-oesophageal annular vessel, which issues from the aorta and receives blood from it? In my work upon the organs of circulation in the Schizopoda and Decapoda (Vienna, 1884) I have even attempted to show the probability that this condition was perhaps the original one in the ancestral forms of the Thoracostraca.

And now as to the supposed perfect agreement in the form and minute structure of the organs to which Ray Lankester appeals as an argument for *Limulus* being an Arachnid! And first of all the possession of reticulate sexual glands, which are said not to exist in the Crustacea. Does not Ray Lankester know the reticulate testes of the genus *Apus*, a genus which he made the subject of an extensive memoir? And is he so little able to judge of the morphological significance of a character as to estimate, from a classificational point of view, the external form of the sexual glands as a determinant factor in making *Limulus* an Arachnid? What have the comparisons of the leg-glands (the so-called *coxal glands*), which are quite arbitrarily interpreted as segmental organs, to do with the proof that *Limulus* is an Arachnid? or, lastly, the structure of the so-called *entochondrites* and inner skeletal structures in *Limu-*

lus, *Scorpio*, and *Mygale*, especially as perfectly similar endoskeletal structures occur also in the Crustacea?

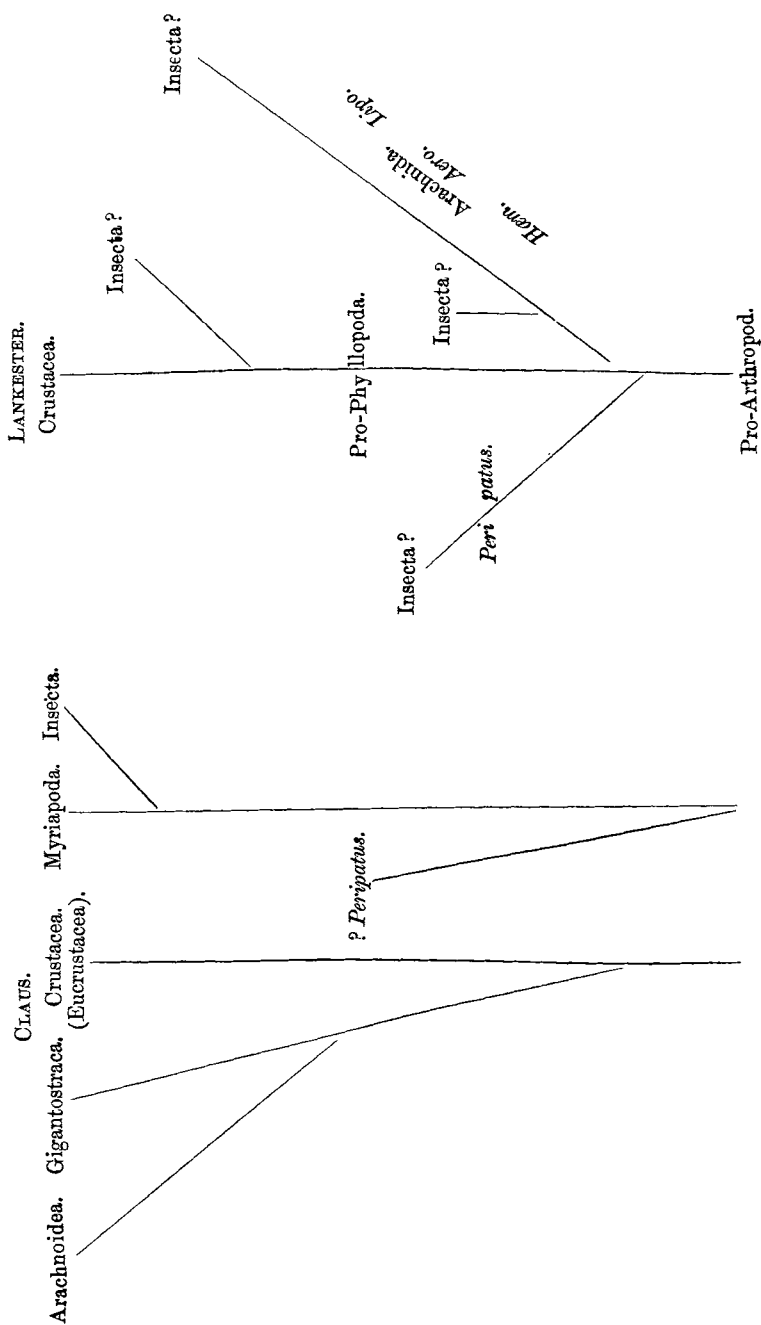
Under such circumstances it will hardly be a matter of wonder if I was unable to recognize in Ray Lankester's *Limulus*-article any advance towards a solution of a problem which has been extant for years, but rather felt compelled to regard it as a failure, so far as it went beyond what was known to his predecessors. Consequently if, in my short communication in the 'Anzeiger' of the Academy, I had been able to include any statements upon the literature of the subject, I should certainly have cited Ray Lankester's article only in the above sense, and to show how hasty speculations may shoot beyond the mark. I may, however, admit freely that in the preparation of my short note I had not the least thought of Ray Lankester's *Limulus*-memoir, especially as my conception of the relations of the Gigantostraca to the Arachnoidea dates much further back, and has nothing at all in common with all the speculations, assertions, and conclusions of the English author.

Had Ray Lankester been able to treat the few words of my communication with quiet consideration it would have been quite impossible that the contrast of the two views should have escaped him so completely; with the acuteness peculiar to him he must at once have recognized that I assert something quite different from his conclusions when I arrange the Gigantostraca and the Arachnoidea as descending from them as *different classes in a genetic series*, while he himself would prove *Limulus* to be an Arachnid, and imagines that he has proved it. I regarded the relationship of the Xiphosura and Arachnoidea as a *more distant one*; and by placing the Gigantostraca and Arachnoidea in one of the three Arthropod series I by no means affirm the *Arachnoidal nature* of *Limulus* any more than I would maintain the Insect nature of *Peripatus*, which, as a representative of the Onychophora, I placed, with the Myriapoda and Insecta, in the other series.

3. It must appear quite unintelligible that Ray Lankester was not aware of the great differences which exist between him and myself as to the mode of derivation of the classes of Arthropoda, as also of the contradiction in the interpretation of the antennæ, so that he could do my views the honour of regarding them as almost, point by point, adopted from his own. When I asserted in my communication: "Hitherto, evidently, far too much stress has been laid upon this latter agreement [respiration by tracheæ] in the unfortunate division of the Arthropoda into Branchiata and Tracheata" (and the same thing was previously said in the 'Grundzüge'), "without considering that the breathing by air-spaces may have been developed in different ways," &c., this of course,

according to Lankester, is "simply repeating a statement by me" &c.; and when I remark, "Accordingly the roots of the old Gigantostroma and Xiphosura may meet in a common origin" &c., and further, "Besides these two series of Arthropoda, probably united at the base, we have then to distinguish, as a third series of forms, that of the Insecta and Myriapoda, for the derivation of which the remarkable Annelid-like Onychophora (*Peripatus*) appear to be possibly of great significance,"—opinions which, as shown above, are already contained in the 'Grundzüge,'—our author does not hesitate to comment as follows upon these statements:—"Proceeding to formulate the conclusions *which he has taken bodily from me* as to the probable genealogy of the chief groups of the Arthropoda, Prof. Claus states that the stem of the Crustacea and that of the Arachnida are united at the base, whilst the Insecta Hexapoda and Myriapoda form a third series, 'for the derivation of which the remarkable Annelid-like Onychophora (*Peripatus*) appear to be so significant.'" I will here take no notice of the misrepresentation which my expression with reference to the Onychophora has undergone in the English translation cited by Lankester by the omission of the word "possibly," by which I wished to indicate that the Onychophora-question is still an open one*, and will confine myself to the demonstration of the difference of this derivation from the genealogical tree which Ray Lankester has sketched in his *Limulus*-article. He says, indeed, "This is a simple and direct description in words of the genealogical tree of the Arthropoda at the end of my article '*Limulus* an Arachnid,'" but unconsciously gives up this opinion in the following phrase, which runs:—"with this difference, that whilst I have represented the Crustacea and the Arachnida as two main stems with a common base, and *Peripatus* as a third and independent stem, I have indicated a hesitation to decide on referring the Insecta Hexapoda and Myriapoda to the stem of *Peripatus* absolutely, and have considered the possibility of their derivation from either the Arthrostracous Crustacea or the tracheate Arachnida." If I wished to embody the genealogical affinities of the three established Arthropod series in the form of a genealogical tree I should have to choose some such scheme as the following, which, as will be seen from the copy of Ray Lankester's genealogical tree of the Arthropoda placed beside it, presents a somewhat different appearance:—

* [The quotation in Prof. Lankester's paper was taken from the abridgment of Prof. Claus's note in this Journal, so that the omission of the word "possibly" is hardly to be charged upon Prof. Lankester. To us the "möglicherweise" seemed quite unnecessary, and indeed redundant, in the case of a group which only "appeared" to be of great significance; it certainly did not convey the idea above ascribed to it.—Eds.]



The interpretation of the antennæ also I am said to have taken from Prof. Ray Lankester's writings! In his Cell-layer publication of the year 1873 our author has set up the beautiful hypothesis * of the change of position of the buccal aperture in the Arthropoda in order to explain a second supposition of his, according to which the prostomium of the Arthropoda is formed exclusively by the eye-segment. Ray Lankester consequently assumes that the antennal segments were originally placed metastomially, and only became prostomial by a subsequent shifting of position of the oral aperture. In what way, and induced by what causes, the formation of the new mouth took place we unfortunately do not learn; but we are told that this assumption is *fully warranted* by Kowalewsky's investigations upon *Amphioxus*, because, according to his observations, the mouth of *Amphioxus* is the first gill-slit or pharyngeal perforation on the left side, and has no relation to the primary larval mouth &c. (see footnote). Thus it is a completely false analogy which is supposed to furnish the foundation for the notion of the "adaptational shifting of the oral aperture," and justify the interpretation of the Arthropod antennæ as postoral limbs. And yet Ray Lankester ventures now to call this completely futile speculation a *fundamental theory*, from which I am supposed to have borrowed the interpretation of the second Crustacean antenna as a body-appendage! Subsequently, in the *Limulus*-article and that on *Apus* of the year 1881, the postoral nature of the antennæ is again affirmed, but only for the Crustacea;

* This fine passage runs as follows:—"Much more likely it seems is the explanation that the oral aperture shifts position, and that the ophthalmic segment alone in Arthropoda represents the prostomium, the antennary and antennular segments being aboriginally metastomial, and only prostomial by later adaptational shifting of the oral aperture." And further on (but upon this he has, perhaps wisely, said nothing): "The assumption of such a shifting of the oral aperture is fully warranted by what has been demonstrated in the case of Vertebrata through Kowalewsky's researches on *Amphioxus*. It is certain from those observations that the mouth of *Amphioxus* is the first gill-slit or pharyngeal perforation of the left side, and has no relation to a mouth such as that which appears at an earlier stage of development in the allied Ascidian larva, which latter mouth is that of Vermes generally. *Amphioxus* then and the Vertebrata have a new oral aperture, the old one being gradually suppressed. Comparative osteology and the embryology of higher Vertebrata have long made it clear that the vertebrate mouth belongs to the series of visceral clefts; but the significance of this in the comparison of Vertebrata and Invertebrata has yet to be fully appreciated. The identification of the neural and hæmal aspects of Vertebrata and Vermes in the light given by this demonstration of Kowalewsky's, as to the distinct character of the mouth in the two cases, must lead to most valuable results."

and it is admitted to be possible that the antennæ of *Peripatus*, as also of the Hexapoda and Myriapoda, are true appendages of the prostomium, as in the Chætopoda!

On the other hand, for my own part, even in my earlier writings, I have regarded the anterior antennæ of the Crustacea as prostomial appendages equivalent to the antennæ of Insects, Myriapoda, and of *Peripatus*, and subsequently, in agreement with Hatschek, as derived from the frontal tentacles of the Annelida, but have attached to the second pair of antennæ of the Crustacea the significance of a pair of body-appendages only secondarily shifted in front of the mouth; and this since I ascertained in many Entomostraca the origin of the nerves of the second antennæ far away from the cerebrum upon ganglia of the œsophageal ring, and at the same time took into consideration the paraoral position of these appendages in the Nauplius-larvæ. Not a change in the position of the mouth, as supposed by Ray Lankester, but an upward movement of the appendages performed in the course of development, with a corresponding displacement of the place of origin of the nerves belonging to them, was recognized as the argument for the preoral shifting of the second antenna and the origin of its nerve on the cerebrum.

When Ray Lankester states that he has not hitherto found this doctrine of an upward movement clearly formulated in my writings, this only proves once more that he does not know them very well. In the 'Grundzüge' indeed, in which the whole domain of zoology is treated in the most condensed form, no discussion of such a point is to be expected; but Ray Lankester might have expected to find something of the kind in the "Beiträge zur Kenntniss des feineren Baues der Daphniden &c.," Zeitsch. f. wiss. Zool. xxvii. (1876), and would have found it had he looked (pp. 377-379). Instead of this he comes forward at once with the charge:—"He has adopted my theory of 1873 in so far only as the second pair of antennæ are concerned;" nay, more, he does not shrink from the really enormous logical contradiction of characterizing my views as to the Arthropod antennæ ("as to the contrasted and totally distinct origin of the Crustacean antennæ") as adopted from his writings!

How is it possible, moreover, that, considering the contradiction in the interpretation of the Crustacean antennæ and the anterior limbs of the Gigantosthraca and Arachnoidea, Ray Lankester should be unable to comprehend that my explanation is quite different from his, and therefore even for that reason alone cannot be borrowed from him? While he interprets the falces (or so-called "jaw-antennæ") of the

Arachnida and the anterior limbs of *Limulus* as equivalent to the anterior antennæ of the Crustacea, I characterize the Arachnoidea and Gigantostaca by the absence of the anterior antennæ, which I correlate with the antennæ of Insects, Myriapods, and *Peripatus*. Formerly indeed, in my work upon the Crustacean system, I correlated the anterior members of *Limulus*, like the falces of the Arachnoidea, with the anterior antennæ of the Crustacea; but this interpretation was founded upon the erroneous notion, supported by the statements of authors, that the nerve belonging to them originates from the cerebrum. But since I became acquainted with the demonstration given by Alphonse Milne-Edwards, that this nerve, in opposition to the statements of Van der Hoeven, Owen, and Huxley, really originates from the œsophageal ring, I regard the interpretation of the anterior pair of members as belonging to the trunk as incontestable, while, on the other hand, I can see no obstacle to the homologization of the falces with the anterior limbs of *Limulus*, in the circumstance that the nerves passing to the falces of the Scorpions originate from the cerebrum, considering the other reasons in favour of the morphological relationship of the Gigantostaca and Arachnoidea. Just in the same way that the nerve of the second antenna of the Crustacea, originating from the œsophageal ring, becomes a cerebral nerve in the higher types of that class, a similar condition may also be developed in the second Arthropod series, and the nerve originating from the cerebrum in the higher type of the Arachnoidea may have belonged, in the Gigantostaca, as still in *Limulus*, to the œsophageal ring, and consequently to a trunk-ganglion; in other words, the nerve of the falces of the Arachnoidea has only secondarily become a cerebral nerve. From this mode of argumentation, which is quite different from Ray Lankester's, I have characterized the second Arthropod series by the reduction of the præoral region of the head and *the deficiency of the first pair of antennæ*, without the least reference to any opinions of Prof. Ray Lankester, with which mine have nothing in common. How complete this contradiction is, especially in the province of the Crustacea, Ray Lankester may ascertain from my investigations of recent date, which, indeed, appear to be equally unknown to him with the earlier ones. If Ray Lankester had only a remote presentiment of this contradiction, which is founded on the whole method of putting forward the question, on the mode of investigation and drawing conclusions, he would certainly have kept himself free from the apprehension that on the next favourable opportunity I might perhaps appropriate

from him the notion that the first Crustacean antenna is a postoral member. "I do not think it improbable," he does not hesitate to say, "that at some future date Prof. Claus may adopt the view which I have advocated as to the first, just as he has adopted it in regard to the second pair of Crustacean antennæ; and I am therefore anxious to take the present opportunity of insisting upon an important piece of evidence in its favour which has come to light through my researches on the relationship of *Limulus* to the Arachnida." Then follows a precious piece of argumentation, which furnishes a striking evidence as to the method of work adopted by our author, and by which the postoral interpretation of the anterior Crustacean antenna is to be proved. The "brick-red glands" of *Limulus* and the corresponding coxal glands of *Scorpio* and *Mygale* are segmental organs, and, indeed, according to Ray Lankester's latest investigations, the equivalents of the shell-glands of the Entomostraca, which, as is well known, open outwards on the second pair of maxillæ. Now, according to Gulland's and Kingsley's statements, the brick-red gland of the young *Limulus* opens in the basal joint of the fifth pair of appendages; consequently this pair of limbs corresponds to the second pair of maxillæ of the Entomostraca; and as this also represents the fifth appendage, the first pair of appendages of *Limulus* and the Arachnoidea represents the first pair of antennæ of the Crustacea, consequently this is the first postoral pair of appendages, *quod erat demonstrandum*! If the "brick-red gland" of *Limulus* were really homologous with the shell-gland of the Crustacea, the Arachnid theory of *Limulus* would be in a truly bad way!

In the preceding statement I have not only proved the falsity of the charges which Prof. Ray Lankester has brought against me, but I also believe that I have demonstrated the method which he has employed in order to make these charges seem plausible to the impartial reader who may not be thoroughly well informed upon the subjects. It is the same method which the honoured English author makes use of in his scientific works in order to build up the famous results of his remarkable deductions by means of the most extraordinary speculations without a sufficient foundation of facts. But while these must often serve to amuse the judicious reader, the grave charges against a colleague have a very serious side. Now that the proof of their absolute falsity has been given, the reproach of at any rate *frivolous suspicion* falls all the more heavily upon the originator of the accusations—a reproach from which a respectable man can only clear himself by simple and honourable revocation.