

On the early Stages of Development of Orthopterous Insects. By
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Royal Horticultural Society.

[Read Nov. 20, 1862.]

I do not know why it should be so, but I am inclined to think that it is the fact, that although naturalists and scientific men are necessarily as liable to error as the rest of mankind, they are more reluctant to acknowledge their errors. Of course, in the abstract and general, they will acknowledge, as much as you please, they are all "miserable sinners"; but touch them on specific points, and, recalcitrant as all mankind are to such confessions, the naturalist, I think, kicks hardest. It may be that it costs him more to make them. His works are the children of his brain; he has travailed hard to bring them forth: they are the result of long thought and laborious investigation; and the more time and pains he has bestowed upon them, it is natural that the greater should be his reluctance to acknowledge them to be abortive. It is wrong, however, to speak of it as an acknowledgment. If he is once convinced that he is wrong, he is not less ready to acknowledge it than other men. But the difficulty is, to convince him. There are some men whom it is impossible to convince; they rest on the conclusion to which they have once come with all the unreasoning obstinacy of consistency; there are others who are only unconvinced after they have committed themselves to print. The majority, however, are only *difficult* to convince. For myself, I claim to be one of these reasonable men. When facts and arguments have all declared against me, and I have no longer a leg to stand upon, I will handsomely acknowledge myself to have been in the wrong; but until then, I find it very difficult "to see the matter in that light."

This is my present predicament. Some years ago I published a few observations on the metamorphoses of Orthopterous and Hemipterous insects, which received more attention than they deserved, from their concurring with and supporting what appeared to be the views of one of our *heroës scientiæ*, Professor Owen.

During the past summer I have made some further observations, which have shown me that I had certainly misread the facts which I saw; and they have also suggested to me the doubt that I may be wrong in the conclusions to which I had arrived. These conclusions did not, however, rest alone on the facts which I had misread, but on independent observations made by Professor Owen; and it may thus be that the views which I formerly pro-

pounded are still well founded, although some of the props on which I rested them have to be withdrawn. In these circumstances I think I shall best perform my duty by simply laying before the Society, at this the earliest opportunity, the observations which lead me to think that I have been mistaken.

To enable the Society to understand the point at issue, I must crave their endurance whilst I recapitulate a little.

Fellows are of course aware that *Orthopterous* and *Hemipterous* insects are characterized by what is called a semicomplete metamorphosis, that is, that they quit the egg, not in the shape of caterpillars, but nearly similar in form to the perfect insect, but with some parts only partially developed; that, after so appearing, they at no time go into a dormant chrysalis state, but merely cast their skin a certain number of times, after each of which the wings and other parts of the insect make an advance towards their perfect state. The first stage of these insects after their appearance out of the egg has been treated by entomologists as a peculiar form of the larva state, which Mr. Westwood has characterized as "homomorphous" or "monomorphous," from its resemblance to the perfect insect after its first moultings; and when the wings begin to appear, it was said to pass into the pupa state, and was called an active nymph or pupa. Professor Owen, however, in his lectures on invertebrate animals, pointed out that we ought not to look upon these "homomorphous" larvæ as true larvæ, but that the true larval condition was to be sought for in the egg. He stated that "these insects" (the *Orthopterous* and *Hemipterous*) "are at one stage of their development apodal and acephalous larvæ, like the maggot of the fly; but, instead of quitting the egg in this stage, they are quickly transformed into another, in which the head and rudimental thoracic feet are developed to the degree which characterizes the hexapod larvæ of the *Carabi* and *Petalocera*; the thorax is next defined, and the parts or appendages of the head are formed, at which stage of development the young *Orthopteran* corresponds with the hexapod antenniferous larva of the *Meloë*; but it differs from all *Coleopterous* larvæ in being inactive, and continuing in the egg almost until all the proportions and characters of the mature insect are acquired, save the wings."

This statement was published in 1855, in the edition of Owen's 'Invertebrate Animals,' when I happened to be engaged in writing an account of a living specimen of the Leaf-insect (*Phyllium Scythe*) which was reared in that year in the Botanic Garden of Edinburgh; and it suggested to me doubts as to the received mode of explaining the phenomena of the development of these insects.

It appeared to me that if it were once determined that the larvæ of Orthopterous insects passed the early portion of their life in the egg, in the shape of maggots, something else must follow from this by way of corollary. It struck me that the necessary consequence of the early stage of the Orthopteran being a larva in the egg was, that it also passed the chrysalis state in the egg. I could quite understand that the perfect insect was eliminated out of the embryonic elements in the egg, in the same way that a chicken is hatched; but if the maggot were once hatched instead of the chicken, I know of no means, and no analogy, by which its vermiform character could be changed, except by passing through the dormant chrysalid state.

The solution which I proposed was that both the larval and chrysalid states were passed in the egg, and that what has been called the homomorphous larva or the active pupa, both by Westwood and Owen, was merely a phase of the perfect insect. Whether I am right in the former view is what I now doubt, but I retain as firm a conviction of the latter as ever.

The fact which I can testify from personal observation, and which is confirmed by various independent observers, that perfect-winged Hemiptera have been found coupling with incomplete, apterous (*quasi* larval) individuals, and not only so, (although as to this I do not speak from my own observation) but that two incomplete apterous individuals have been found coupling together, seems to me at least *primâ facie* evidence that the individuals exercising that function were not larvæ, but must have been perfect, although precocious insects.

The arguments by which I endeavoured to support the proposition that both the larval and chrysalis stages were passed in the egg were drawn from some observations upon the eggs of the Leaf-insect, which seemed consistent with the adoption of Professor Owen's observations that the insect passed its larval stage in the egg, as an apodal vermiform caterpillar; for it will be observed that the whole of my reasoning depended upon this. Without this basis, I had no ground to go upon. All that I said was, admit that the larva is once a maggot, and I defy you to transform it into a perfect insect in any other way than by passing it through the chrysalis state. I say so still; but if it is not a maggot in the egg, if it appears at once (that is so soon as it has left its embryonal condition and assumed a distinct character) with the limbs and parts of the so-called active pupa, then there is no puzzle; there is nothing abnormal: it has only to grow as any other creature grows.

The instances I adduced in support of the above view were

two, the first, as already mentioned, drawn from the egg of the Leaf-insect, and the second from that of the Cockroach, both however from dried specimens.

That drawn from the egg of the Leaf-insect was as follows. The outer case, or capsule, or egg as it is generally called, is about the size of a small pea, of a corticaceous honeycombed texture, lined within with a porcelain-like shell, and fitted with a little conical lid. Within it is an under membrane, which contains the yelk. This yelk is sometimes dried up and hardened, and may assume the form of a flasked-shaped transparent capsule. It had done so in my former specimen ; and its shape and appearance was so thoroughly that of a capsule, that it misled myself and every one who saw it, and I described it as such. I said that if we regarded the outer case or egg as the egg, then the inner membrane should be the chorion, and the flask-shaped capsule the yelk ; but as this capsule had a determinate form (which, indeed, is inconsistent with the liquid condition of a yelk), I thence inferred that, as it could not be the yelk, it might be the chrysalis.

When I made these remarks, I had very imperfect materials to generalize from. This I fully explained. I said, "I have only examined one addled egg, and it chanced to be one with what I suppose to be the chrysalis in it. Some more fortunate individual will, I hope, ere long have the opportunity of settling the question ; and if, on opening eggs at an early period, he find a maggot, and at later periods this capsule, I think I shall then be entitled to say that it has been settled in my favour." Last year I was desirous of introducing a supply of these interesting creatures as objects of attraction to the Royal Horticultural Society's Conservatory, and I procured a supply of eggs from my friend Professor Fayrer of Calcutta. Unfortunately these proved all addled, and I have thus had ample material for investigation (so far as dried specimens allow), and have perfectly satisfied myself that the egg does not differ in its parts from any other egg. The outer case is merely the shell, the inner membrane is the chorion, and what I took for a capsule with a determinate shape is merely the yelk hardened into a particular form. I have found it assuming more or less of this form according to circumstances—sometimes only a part of it, and at others nearly the whole being so shaped.

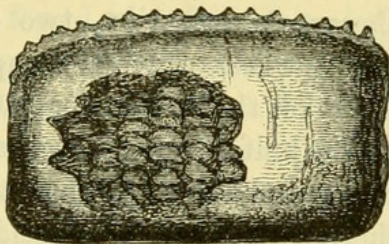
The other instance which I brought forward was drawn from the egg of the *Blatta*, and was published in 1856. This was the insect from the study of which Professor Owen arrived at his conclusions. He says in his 'Lectures on Invertebrate Animals' (p. 437, edition 1855), "Metropolitan duties shut out much of the field of nature ;

but still she may be found and studied everywhere. I first learned to appreciate the true nature and relations of the nominally various and distinct metamorphoses of insects, by watching and pondering over the development of a Cockroach, which quits the egg as a Crustacean. I saw that it passed through stages answering to those at which other insects were arrested: there was a period when its jointed legs were simple, short, unarticulated buds, when its thirteen segments were distinct and equal—when it was apodal, when it was acephalous." Predisposed by this, I viewed a specimen of the egg of the *Blatta*, which shortly afterwards came into my hands, in a different light from what I should perhaps otherwise have done.

I received from my correspondents in Old Calabar a rude idol-like figure, in the corners of the eyes and behind the ears of which three or four Cockroaches' eggs had been laid. The eggs of this insect are compound eggs, formed somewhat like a broad pea-pod, in which spaces are partitioned off transversely for the young insects, which are arranged like peas in the pod. When the insect had laid them on this figure, it had plastered them all over with a cemented coating of chopped wood or straw, like a cocoon; and after this had been done, the whole figure had been rudely painted by the natives, and these cases were covered with the paint. No insect could have emerged without breaking through the case and the paint, which were both uninjured. I presume, at a certain stage on their passage homewards, the cold had killed them; for I found the contents preserved as they no doubt existed at the time when this event took place.

In one I found the mummied remains of what, in describing them, I called a row of unmistakeable grubs, packed closely, filling these partitions. Fig. 1 is a sketch of this; and from it it may be judged whether my description was not fairly warranted.

Fig. 1.



The next egg which I opened showed no grubs. It was undoubtedly further advanced. In it I found only two insects, but they had no longer the grub form; they were small, wingless *Blattæ*. One was perfect and fresh, the other somewhat injured; they were probably the last survivors of their brothers and sisters, whom they had no doubt devoured; for the egg, although ready to open, had never given egress to any of its inmates. One of these has been lost; but the other may be seen in the egg, as I found it.

From these materials, and from those drawn from the Leaf-insect, I fancied that I had demonstrated—

1. A grub in the egg. (The first egg of the *Blatta*.)
2. A cocoon in the egg, containing the unwinged, imperfectly developed insect. (The Leaf-insect.)
3. An unwinged, imperfectly developed insect in the egg, free from the cocoon, and ready to emerge. (The second egg of the *Blatta*.)

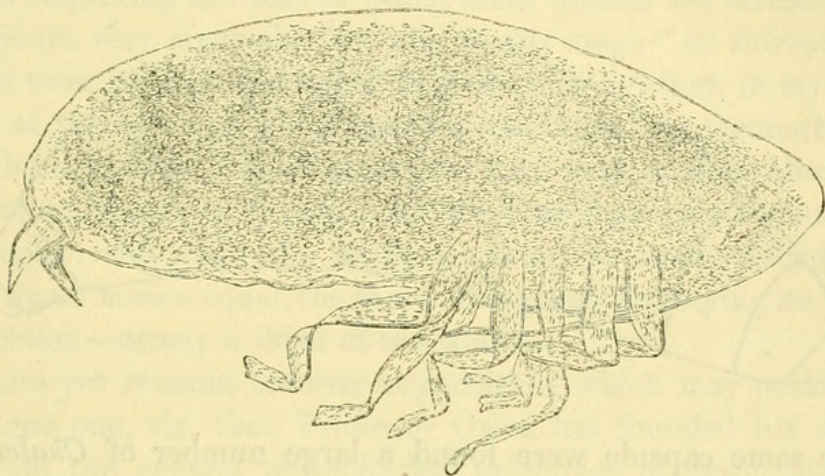
I have now to acknowledge and to show that, as regards the first, I was in error; the creature whose back looks so like one in the egg is not a grub. As regards the second, I have already shown that this was merely the yelk hardened into a cocoon-like shape. And as regards the third, it is quite true, but, without the first and second, proves nothing of what either Professor Owen or myself thought to exist.

Since the publication of these papers (now six years ago), I have always wished for the opportunity of investigating the matter further, but, until the present year, never enjoyed it. I have already told what I have found in this year's arrival of Leaf-insect eggs. I shall now explain what I have learned from the *Blatta*. In Scotland (my former residence) Cockroaches are only exceptionally abundant, and I never had the good luck to have any in my house. Since my removal to London, I cannot complain: I have the happiness to live in a house that is overrun with them, and I have friends through whom I have secured specimens from ships in the docks. Nowwithstanding this *embarras de richesses*, I have found surprisingly few eggs, and most of those which I did get were already opened or advanced to the stage of No. 3. On having recourse to the gravid female, however, I have obtained, by the Cæsarean operation, two or three half-developed eggs, a careful examination of which compels me to own that the interpretation I put on the African egg with the grubs is not borne out by facts.

The half-developed egg is comparatively much narrower, although not greatly shorter, than the full-grown egg. On opening one of these soft, pale, and flaccid egg-cases, I found what, to all appearance, was a row of glistening white grubs. Had I stopped there (and that was all the length the dried African specimen allowed me to go), I could have sworn it was a row of grubs; but, placed under the microscope, the scene changed. The first thing that attracted my attention was a pair of flag-like flaps at one end. This I soon found to be the anal projections at the tail of the perfect insect. The other part seemed rounded, but on a little pressure being given to the glass, out gushed from under the thorax, where it had been closely packed up, a whole quantity of sausage-like matter. Can these be the intestines? thought I. It was

the legs—long, lanky, ribbon-like legs, among which were mixed the long antennæ. Figure 2 will show how these appeared. No

Fig. 2.



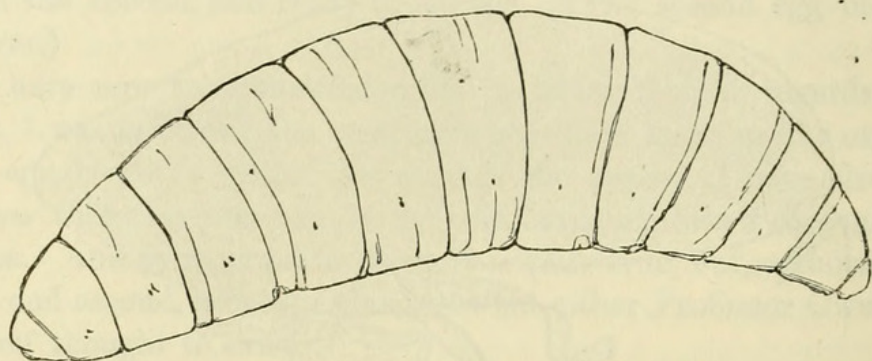
intestines or internal organs at all seemed yet to have been formed. The body was perfectly transparent and full of different-sized cells, which seemed a trifle more aggregated between the segments of the abdomen. In some there was no distinction between the head and thorax, or between the thorax and abdomen; in others this distinction began to appear (whether visible on account of position or not I could scarcely say, but probably it might be so). On the skin being ruptured, the cells poured out in quantity, but there was no appearance of any vessels or defined internal structure of any kind. There was, however, an indication of the eye externally.

I am therefore compelled to admit that, if my African specimens had been fresh and entire, I should have found a head and long legs curled up under the thorax, and not an apodous acephalous maggot.

So much by way of rectification of the mistake which I myself have committed. It remains to consider whether Professor Owen's statement, which led me wrong, is correct or not. If his observations bear the meaning which I attached to them, I certainly think he has fallen into a mistake. If he meant, as I supposed him to do, that the young *Blatta* ever was in the egg in the shape of a real acephalous and apodal larva, then I feel very confident that he is wrong; and he may have been misled in one of two ways. He may have fallen into the same mistake which I committed—mistaken the vermiform-looking back of the *Blatta* for a maggot. Or he may have fallen into an error which I have escaped. In opening one of these egg-capsules (not taken from the parent insect, but one which had arrived at maturity, and been deposited), I found a number of the larvæ in a disarranged, empty, sloppy state,

and in the midst of them a very comfortable, firm, plump, fat, true apodal and acephalous larva (fig. 3). It was obviously a parasite.

Fig. 3.



In the same capsule were found a large number of *Chalcidites*, some in chrysalis and some in the imago state. The apodous larva probably belonged to them. Professor Owen may have met with a similar parasite, and mistaken it for the real lord of the manor.

This is on the supposition that I have rightly interpreted Professor Owen's views. It may be that I have misunderstood him. In speaking of the insect passing its *larval* stage in the egg, he may have merely meant its *embryonic* stage. If this was what he meant, then probably we are at one in our meaning; for it has never occurred to me to question that, in its embryonic existence the *Blatta* went through the usual course of development observed in other animals in which it has been studied. I perfectly believe (although I have myself never traced its progress) that, as in the *Aphis*, whose development from the egg has been so clearly followed out and described by Professor Huxley—and, for that matter, as in the Vertebrata and in our own species—the first semblance of form which the germ of life assumes is a cord which may not inaptly be compared to something vermiform, and this shapeless shape or formless object gradually and imperceptibly assumes that shape which it is to bear when it leaves the egg. Consequently the long legs must at one time have been short ones, and still further back must have shown their first indications as buds.

This is embryonic development; and if it is this which Professor Owen means (and there are some passages in his work which now lead me to think that it may be so), then I misunderstood his meaning, and have been writing and thinking at cross purposes with him.

But if that is his meaning, then I must dissent from the relation which he appears to think exists between the development of these insects in the egg and the metamorphoses of other insects out of

it. I do not now think that we have any ground to call the embryos of the *Blattæ* "apodal and acephalous larvæ, like the maggot of the Fly," nor to hold that they "pass, in the egg, through stages answering to those at which other insects are arrested." I also doubt very much the fact that at any stage "its thirteen segments were distinct and equal." I can only say that, in my specimens at the epoch at which figure 2 was taken, the segments must have been at their earliest stage, for they were in some absolutely undistinguishable; and yet, in all, the long legs were by that time developed; and when the segments could be distinguished, they were by no means equal, the thoracic segment occupying its proper proportion—nearly a third of the whole.

There yet remains another supposition, which may perhaps be the true one, viz. that Professor Owen has founded his conclusions on the general physiological principles of development, as modified by erroneous observations, arising from either of the sources of error I have above indicated.

On the Species of *Fusidæ* which inhabit the Seas of Japan.

By ARTHUR ADAMS, F.L.S., &c.

[Read March 19, 1863.]

THE species of Fusoid Gasteropods are tolerably numerous in the seas of Japan. In the north I found many of the large *Neptunæ*, which in the south are represented by the group I have named *Siphonalia*. In the southern parts also linger a *Cominella* and a few *Euthriæ*. No species of *Fusus* proper was found by me further north than Tsuka Strait. *Hemifusus tuba*, used as a fog-horn by the fishermen, was observed from Tsu-Sima in the south to Hakodadi in the north.

The genus *Siphonalia*, A. Ad., will be found described, and twenty-one species enumerated, in the 'Annals and Magazine of Natural History' for March 1863.

1. Genus *HEMIFUSUS*, Swains.

1. *Hemifusus tuba*, Gmel. (*Murex tuba*, Gmel.—*Fusus tuba*, Enc. Méth.—*Pyrula tuba*, Lam.—*Cochlidium tuba*, Gray.)
Hab. Tatiyama; Hakodadi; Tsu-Sima; Nagasaki.

2. Genus *FUSUS*, Klein.

1. *Fusus crebriliratus*, Reeve, *Conch. Icon.* sp. 20.
Hab. Tatiyama; Okino-Sima.
 2. *Fusus Novæ-Hollandiæ*, Reeve, *Conch. Icon.* sp. 70.
Hab. Tatiyama; Hakodadi.



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