otoconia in the former case and single spherical *otoliths* in the latter. The dental organs themselves usually consist of a basal plate of attachment, with which the dental tubercles or fangs, which always point backwards, are connected. They are subject to depreciation or suppression, and further development or increase, both wholly or as to their component parts, which has, no doubt, given rise to all the diversity of character which we observe in the different families of Gasteropoda. Thus we often find the dental processes so large as to quite absorb the basal plates, while in other cases the basal plate alone remains, as it were preparatory to its complete extinction. It will be seen therefore that if the pleura on each side gradually undergoes suppression, a typical pavement will be made to assume a more or less strap-like appearance; and this character will be made more deceptive by the coincident development of the rhachis*. On the other hand, if the rhachidian series is suppressed, the dentition will, of course, be divided into two lateral portions and thus become more or less decidedly double, the effect being enhanced by the greater development of the central part of each pleura. Illustrations of these conditions are to be found in all the principal sections of the Gasteropoda. I have only to regret at present that my time will not permit me to make this subject clearer by special reference to examples; but I hope to do so at some future period as an introduction to the second part of this paper, taking up the classification of the Gasteropoda Diœcia.

Observations on Ants, Bees, and Wasps; with a Description of a new Species of Honey-Ant.—Part VII. Ants.

By Sir John Lubbock, Bart., M.P., F.R.S., F.L.S., D.C.L., LL.D., Vice-Chancellor of the University of London.

[Read June 17, 1880.]

(Plate VIII.)

Power of Communication by something approaching to Language.

In my previous papers many experiments have been recorded, in which I have endeavoured to throw some light on the power of

* For example, in the Eolidæ and neighbouring genera, the affinity of which cannot be doubted, the gradual reduction from a typical pavemental dentition to the pseudo strap-like form may be easily observed.

communication possessed by ants. It is unquestionable that if an ant or a bee discovers a store of food her comrades soon flock to the treasures, although, as I have shown, this is by no means always the case. But it may be argued that this fact taken alone does not prove any power of communication at all. An ant observing a friend bringing food home, might infer, without being told, that by accompanying the friend on the return journey she might also participate in the good things. I have endeavoured to meet this argument in my third paper (Linn. Journ. vol. xii. p. 466) by showing that there was a marked difference in the result, if on experimenting with two ants one had access to a large treasure, the other only to a small one.

It also occurred to me that some light would be thrown on the question by compelling the ant who found the treasure to return empty-handed. If she took nothing home and yet others returned with her, this must be by some communication having passed. It would be a case in which precept was better than example.

I selected therefore a specimen of *Atta testaceo-pilosa*, belonging to a nest which I had brought back with me from Algeria. She was out hunting about six feet from home, and I placed before her a large dead bluebottle fly, which she at once began to drag to the nest. I then pinned the fly to a piece of cork, in a small box, so that no ant could see the fly until she had climbed up the side of the box. The ant struggled, of course in vain, to move the fly. She pulled first in one direction and then in another, but, finding her efforts fruitless, she at length started off back to the nest empty-handed. At this time there were no ants coming out of the nest. Probably there were some few others out hunting, but for at least a quarter of an hour no ant had left the nest. My ant entered the nest, but did not remain there; in less than a minute she emerged accompanied by seven friends. I never saw so many come out of that nest together before. In her excitement the first ant soon distanced her companions, who took the matter with much *sang froid*, and had all the appearance of having come out reluctantly, or as if they had been asleep and were only half awake. The first ant ran on ahead, going straight to the fly. The others followed slowly and with many meanderings; so slowly, indeed, that for twenty minutes the first ant was alone at the fly, trying in every way to move it. Finding this still impossible, she again returned to the nest, not chancing to meet

any of her friends by the way. Again she emerged in less than a minute with eight friends, and hurried on to the fly. They were even less energetic than the first party; and when they found they had lost sight of their guide, they one and all returned to the nest. In the meantime several of the first detachment had found the fly, and one of them succeeded in detaching a leg with which she returned in triumph to the nest, coming out again directly with four or five companions. These latter, with one exception, soon gave up the chase and returned to the nest. I do not think so much of this last case, because as the ant carried in a substantial piece of booty in the shape of the fly's leg, it is not surprising that her friends should some of them accompany her on her return; but surely the other two cases indicate a distinct power of communication.

Lest, however, it should be supposed that the result was accidental, I determined to try it again. Accordingly on the following day I put another large dead fly before an ant belonging to the same nest, pinning it to a piece of cork as before. After trying in vain for ten minutes to move the fly, my ant started off home. At that time I could only see two other ants of that species outside the nest. Yet in a few seconds, considerably less than a minute, she emerged with no less than twelve friends. As in the previous case, she ran on ahead, and they followed very slowly and by no means directly, taking, in fact, nearly half an hour to reach the fly. The first ant, after vainly labouring for about a quarter of an hour to move the fly, started off again to the nest. Meeting one of her friends on the way she talked with her a little, then continued towards the nest, but after going about a foot, changed her mind, and returned with her friend to the fly. After some minutes, during which two or three other ants came up, one of them detached a leg, which she carried off to the nest, coming out again almost immediately with six friends, one of whom, curiously enough, seemed to lead the way, tracing it, I presume, by scent. I then removed the pin, and they carried off the fly in triumph.

Again, on the 15th June, another ant belonging to the same nest had found a dead spider, about the same distance from the nest. I pinned down the spider as before. The ant did all in her power to move it; but after trying for twelve minutes, she went off to the nest. For a quarter of an hour no other ant had come out, but in some seconds she came out again with ten companions.

As in the preceding case, they followed very leisurely. She ran on ahead and worked at the spider for ten minutes; when, as none of her friends had arrived to her assistance, though they were wandering about evidently in search of something, she started back home again. In three quarters of a minute after entering the nest she reappeared, this time with fifteen friends, who came on somewhat more rapidly than the preceding batch, though still but slowly. By degrees, however, they all came up, and after most persevering efforts carried off the spider piecemeal. On the 7th July I tried the same experiment with a soldier of *Pheidole megacephala*. She pulled at the fly for no less than fifty minutes, after which she went to the nest and brought five friends exactly as the *Atta* had done.

In the same way, one afternoon at 6.20 I presented a slave of *Polyergus* with a dead fly pinned down. The result was quite different. My ant pulled at the fly for twenty-five minutes, when, as in the previous cases, she returned to the nest. There she remained four or or five minutes, and then came out again alone, returned to the fly, and again tried to carry it off. After working fruitlessly for between twenty and twenty-five minutes, she again went back to the nest, staying there four or five minutes, and then returning by herself to the fly once more. I then went away for an hour, but on my return found her still tugging at the fly by herself. One hour later again I looked, with the same result. Shortly afterwards another ant wandering about found the fly, but obviously, as it seemed to me, by accident.

Aug. 2. At 3 o'clock I put a dead fly pinned on to a bit of cork before a *Formica fusca*, which was out hunting. She tried in vain to carry it off, ran round and round, tugged in every direction, and at length at ten minutes to four she returned to the nest; very soon after she reappeared preceded by one and followed by two friends; these, however, failed to discover the fly, and after wandering about a little returned to the nest. She then set again to work alone, and in about forty minutes succeeded in cutting off the head of the fly, which she at once carried into the nest. In a little while she came out again, this time accompanied by five friends, which all found their way to the fly; one of these, having cut off the abdomen of the fly, took it into the nest, leaving three of her companions to bring in the remainder of their prey.

These experiments certainly seem to indicate the possession by ants of something approaching to language. It is impossible to

doubt that the friends were brought out by the first ant; and as she returned empty-handed to the nest, the others cannot have been induced to follow her merely by observing her proceedings. I conclude, therefore, that they possess the power of requesting their friends to come and help them.

Recognition of Relations.

In my last paper (Linn. Journ. vol. xiv. p. 611) I recorded some experiments made with pupæ, in order if possible to determine how ants recognized their nest companions. The general result was that pupæ tended by strangers of the same species, and then after they had arrived at maturity put into the nest from which these strangers had been taken, were invariably treated as interlopers and attacked. On the other hand, if they were tended by ants from their own nest, and then after arriving at maturity put back in their own nest, they were invariably recognized as friends; and, lastly, if as pupæ they were tended by strangers, but then after arriving at maturity put back in their own nest, they were generally received as friends. In all these experiments, however, the ants were taken from the nest as pupæ, and though I did not think the fact that they had passed their larval existence in the nest could affect the problem, still it might do so. I determined therefore to separate a nest before the young were born, or even the eggs laid, and then ascertain the result. Accordingly I took one of my nests, which I began watching on the 13th Sept., 1878, and which contained two queens, and on the 8th Feb., 1879, divided it into halves, which I will call A and B, so that there were approximately the same number of ants with a queen in each division. At this season, of course, the nest contained neither young nor even eggs. During April both queens began to lay eggs. On the 20th July I took a number of pupæ from each division and placed each lot in a separate glass, with two ants from the same division. On the 30th August I took four ants from the pupæ bred in B, and one from those in A (which were not quite so forward), and after marking them as usual with paint, put the B ants into nest A, and the A ant into nest B. They were received amicably and soon cleaned. Two, indeed, were once attacked for a few moments, but soon released. On the other hand, I put two strangers into nest A, but they were at once killed. For facility of observation I placed each nest in a closed box. On the 31st I carefully examined the nests and also the

boxes in which I had placed them. I could only distinguish one of the marked ants, but there were no dead ants either in the nests or boxes, except the two strangers.

I carefully examined the box in the same way for several successive mornings, but there was no dead ant. If there had been I must have found the body, and I am sure therefore that these ants were not attacked.

Again, on the 31st Aug. I put two more of the ants which had emerged from the pupæ taken out of nest B, and nursed by ants from that nest, into nest A at 10 A.M. At 10.30 they were quite comfortable amongst the others. At 11 I looked again and they seemed quite at home, as also at 11.30, after which I looked every hour. The next morning I found them evidently quite at home in the nest.

On the 15th September I put three of the ants which had emerged from the pupæ taken out of nest A, and nursed by ants from that nest, and put them into nest B at 1.30. They seemed to make themselves quite at home. I looked again at 2.30, with the same result. At 3.30 I could only find two, the third having no doubt been cleaned, but no ant was being attacked. At 5.30 they were no longer distinguishable, but if any one was being attacked we must have seen it. The next morning they all seemed quite peaceful, and there was no dead ant in the box. I looked again on the 17th and 19th, but could not distinguish them. As, however, there was no dead ant, they certainly had not been killed. I then put in a stranger; she was soon attacked and killed—showing that they would not tolerate an ant whom they did not recognize as in some way belonging to the community.

These observations seem to me conclusive as far as they go, and they are very surprising. In my experiments of last year, though the results were similar, still the ants experimented with had been brought up in the nest, and were only removed after they had become pupæ. It might therefore be argued that the ants having nursed them as larvæ, recognized them when they came to maturity; and though this would certainly be in the highest degree improbable, it could not be said to be impossible. In the present case, however, the old ants had absolutely never seen the young ones until the moment when, some days after arriving at maturity, they were introduced into the nest; and yet in all ten cases they were undoubtedly recognized as belonging to the community.

It seems to me therefore to be established by these experiments that the recognition of ants is not personal and individual; that their harmony is not due to the fact that each ant is individually acquainted with every other member of the community.

At the same time, the fact that they recognize their friends even when intoxicated, and that they know the young born in their own nest even when they have been brought out of the chrysalis by strangers, seems to indicate that the recognition is not effected by means of any sign or password.

Mr. McCook states that ants more or less soaked in water are no longer recognized by their friends, but, on the contrary, are attacked. Describing the following observation, he says*:—"I was accidentally set upon the track of an interesting discovery. An ant fell into a box containing water placed at the foot of a tree. She remained in the liquid several moments and crept out. Immediately she was seized in a hostile manner, first by one, then another, then by a third: the two antennæ and one leg were thus held. A fourth ant assaulted the middle thorax and petiole. The poor little bather was thus dragged helplessly to and fro for a long time, and was evidently ordained to death. Presently I took up the struggling heap. Two of the assailants kept their hold; one finally dropped, the other I could not tear loose, and so put the pair back upon the tree, leaving the doomed immersionist to her hard fate."

After repeating one or two other similar observations, he adds†:—"The conclusion, therefore, seems warranted that the peculiar odour or condition by which the ants recognize each other was temporarily destroyed by the bath, and the individuals thus 'tainted' were held to be intruders, alien and enemy. This conclusion is certainly unfavourable to the theory that any thing like an intelligent social sentiment exists among the ants. The recognition of their fellows is reduced to a mere matter of physical sensation or 'smell.'"

This conclusion does not, I confess, seem to me to be conclusively established.

Workers breeding.

In my last paper I brought forward some strong evidence tending to show that when workers laid eggs they always produced males. This is, however, a physiological fact of so much

* 'Mound-making Ants of the Alleghanies,' p. 280,

† Ibid. p. 281.

interest that I have carefully watched my nests this year also, to see what further light they would throw on the subject.

In six of those which contained no queen, eggs were produced, which of course must necessarily have been laid by workers.

The first of these, a nest of *Lasius niger*, which I have watched since July 1875, and which, therefore, is interesting from the great age of the workers, about ten larvæ were hatched, but only four reached the pupa state. Of these one disappeared; the other three I secured, and on examination they all proved to be males.

A second nest of *Lasius niger*, which has been under observation since November 1875, produced about ten pupæ. Of these I examined seven, all of which I found to be males. The others escaped me. I believe that, having died, they were brought out and thrown away.

A nest of *Formica cinerea*, captured at the same time, produced four larvæ, all of which perished before arriving at the pupa stage. They were certainly not workers.

In a nest of *Formica fusca* which I have had under observation since Aug. 1876, three pupæ were produced. They were all males.

Another nest of *Formica fusca* produced a single young one, which also was a male.

Lastly, my nest of *Polyergus rufescens*, which M. Forel was so good as to send me in the spring of 1876, and to which I have already frequently referred in these papers, produced twelve pupæ. Eleven of these turned out to be males. The other one I lost; but I have little doubt it was brought out and thrown away. At any rate it was not a worker. As regards the first three of these pupæ, I omitted to record whether they belonged to the *Polyergus* or to the slaves. The last eight were males of *Polyergus*.

Thus, then, this year again, in five of my queenless nests, males have been produced; and in not a single case has a worker laid eggs which have produced a female, either a queen or a worker. Perhaps I ought to add that workers are abundantly produced in those of my nests which possess a queen.

Again, as in previous years, so this season again, while great numbers of workers and males have come to maturity in my nests, not a single queen has been produced. We have, I think, therefore, strong reason for concluding that, as in the case of bees, so also in ants, some special food is required to develop the female embryo into a queen.

Longevity of Ants.

In my previous paper I have called attention to the considerable age attained by my ants, and I may perhaps be permitted to repeat here, *mutatis mutandis*, a paragraph from my last communication with reference to my most aged specimens, most of those mentioned last year being still alive. One of my nests of *Formica fusca* was brought from the woods in December 1874. It then contained two queens, both of which are now still alive*. I have little doubt that some of the workers now in the nest were among those originally captured, the mortality after the first few weeks having been but small. This, however, I cannot prove. The queens, however, are certainly six, and probably seven years old.

In the following nests—viz. another nest of *F. fusca*, which I brought in on the 6th June, 1875, one of *Lasius niger* on the 25th July, 1875, and of *Formica cinerea* on the 29th November, 1875—there were no queens; and, as already mentioned, no workers have been produced. Those now living are therefore the original ones, and they must be between five and six years old.

Though I lose many ants from accidents, especially in summer, in winter there are very few deaths.

The nest of *F. sanguinea*, which M. Forel kindly forwarded to me on the 12th Sept., 1875 (but which contained no queen), gradually diminished in numbers, until in Feb. 1879 it was reduced to two *F. sanguinea* and one slave. The latter died in Feb. 1880. One of the two mistresses died between the 10th and 16th May, 1880, and the other only survived her a few days, dying between the 16th and 20th. These two ants, therefore, must have been five years old at least. It is certainly curious that they should, after living so long, have died within ten days of one another. There was nothing, as far as I could see, in the state of the nest or the weather to account for this, and they were well supplied with food, yet I hardly venture to suggest that the survivor pined away for the loss of her companion.

Behaviour to strange Queens.

In a previous paper I have shown that, at least in the case of *Myrmica ruginodis*, the queen is capable of bringing larvæ to maturity, and consequently of founding a new nest by herself. Since, however, cases are on record in which communities are

* Aug. 3rd, 1880. They are still alive.

known to have existed for many years, it seems clear that fresh queens must be sometimes adopted. I have indeed recorded several experiments in which fertile queens introduced into queenless nests were ruthlessly killed, and subsequent experiments have always had the same result. Mr. Jenner Fust, however, suggested to me to introduce the queen into the nest, as is done with bees, in a wire cage, and leave her there for two or three days, so that the workers might, as it were, get accustomed to her. Accordingly I procured a queen of *F. fusca* and put her with some honey in a queenless nest, enclosed in a wire cage so that the ants could not get at her. After three days I let her out, but she was at once attacked. On the contrary, Mr. McCook reports the following case of the adoption of a fertile queen of *Cremastogaster lineolata* by a colony of the same species *:—"The queen," he says, "was taken in Fairmount Park, April 16th, and on May 14th following was introduced to workers of a nest taken the same day. The queen was alone within an artificial glass formicary, and several workers were introduced. One of these soon found the queen, exhibited much excitement but no hostility, and immediately ran to her sister workers, all of whom were presently clustered upon the queen. As other workers were gradually introduced they joined their comrades, until the body of the queen (who is much larger than the workers) was nearly covered with them. They appeared to be holding on by their mandibles to the delicate hairs upon the female's body, and continually moved their antennæ caressingly. This sort of attention continued until the queen, escorted by workers, disappeared in one of the galleries. She was entirely adopted, and thereafter was often seen moving freely, or attended by guards, about the nest, at times engaged in attending the larvæ and nymphs which had been introduced with the workers of the strange colony. The workers were fresh from their own natural home, and the queen had been in an artificial home for a month."

Possibly the reason for the difference may be that my ants had been long living in a republic, for, I am informed, that if bees have been long without a queen it is impossible to induce them to accept another.

Moreover, I have found that when I put a queen with a few

* Proc. Acad. Natural Sciences of Philadelphia, 1879. "Note on the Adoption of an Ant-Queen," by Mr. McCook, p. 139.

ants from a strange nest they did not attack her, and by adding others gradually, I succeeded in securing the throne for her.

Sense of Direction.

Having been much struck by the difficulty which ants appear under certain circumstances to experience in finding their way, as indicated, for instance, by some experiments which the Society has done me the honour to publish (Journ. Linn. Soc. vol. xiii. pp. 239–245), I have during the past year made some more experiments on this part of the subject.

I accustomed some ants (*Lasius niger*) to go to and fro to food over a wooden bridge (fig. 1).

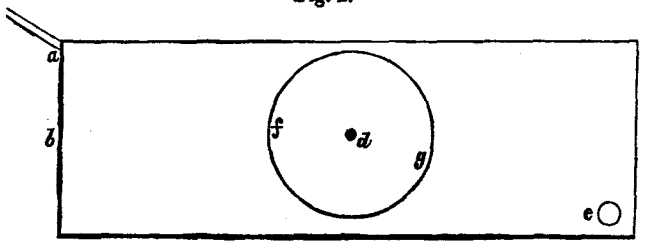
Fig. 1.

When they had got quite accustomed to the way, I watched when an ant was on the bridge and then turned it round, so that the end *b* was at *c*, and *c* at *b*. In most cases the ant immediately turned round also; but even, if she went on to *b* or *c*, as the case may be, as soon as she came to the end of the bridge she turned round.

I then modified the arrangement, placing between the nest and the food three similar pieces of wood. Then when the ant was on the middle piece, I transposed the other two. To my surprise this did not at all disconcert them.

I then tried the arrangement shown in fig. 2.

Fig. 2.



a is a paper bridge leading to the nest; *b* is a board about 22 inches long by 13 broad, on which is a disk of white paper fas-

tened at the centre by a pin *d*; *e* is some food. When the ants had come to know their way so that they passed straight over the paper disk on their way from *a* to *e*, I moved the disk round with an ant on it, so that *f* came to *g* and *g* to *f*. As before, the ants turned round with the paper.

As it might be possible that the ants turned round on account of the changed relative position of external objects, I next substituted a box 12 inches in diameter and 7 inches high (in fact a hat-box) for the flat paper, cutting two small holes at *f* and *g*, so that the ants passing from the nest to the food went through the box entering at *f* and coming out at *g*. The box was fixed at *d*, so that it might turn easily. I then, when they had got to know their way, turned the box round as soon as an ant had entered it, but in every case the ant turned round too, thus retaining her direction. I then varied the experiment as shown in figs. 3 and 4.

Fig. 3.

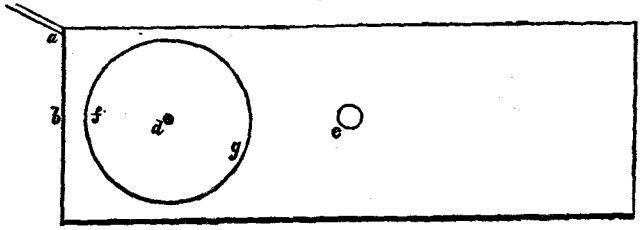
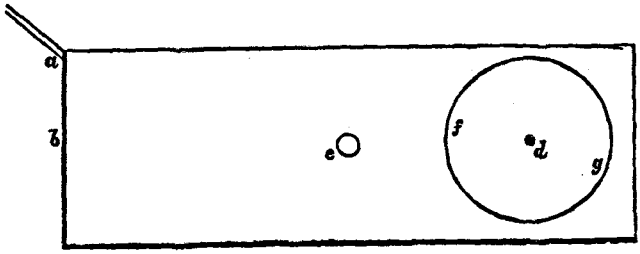


Fig. 4.



I replaced the white disk of paper, but put the food *e* at the middle of the board. When the ant had got used to this arrangement I waited till one was on the disk (fig. 3) and then gently drew it to the other side of *e*, as shown in fig. 4. In this case, however, the ant did not turn round, but went on to *g*, when she seemed a good deal surprised at finding where she was.

As to Hearing and Experiments with Telephone.

In order to ascertain if possible whether ants made any sounds which were audible to one another, I thought I would try the telephone. Accordingly I looked for two ants' nests (*Lasius niger*) not far from one another, and then, after disturbing one of them, had a telephone held just over it. I then held the second telephone close over the other nest, each telephone being perhaps one to two inches above the ground. If the disturbed ants made any sound which was transmitted by the telephone, the ants in the other nest ought have been thrown into confusion. I could not, however, perceive that it made the slightest difference to them. I tried the experiment three or four times, always with the same result.

I then put some syrup near a nest of *L. niger*, and when several hundred ants were feeding on the syrup, I blew on the nest, which always disturbs them very much. They came out in large numbers and ran about in great excitement. I then held one end of the telephone over the nest, the other over the feeding ants, who, however, took not the slightest notice.

I cannot, however, look on these experiments as at all conclusive, because it may well be that the plate of the telephone is too stiff to be set in vibration by any sounds which ants could produce.

On the Sting of Formica.

M. Dewitz, in an interesting paper published in the Zeitschr. für wiss. Zool. vol. xxviii., has given an account of the structure and development of the sting in ants*. *Formica rufa*, and other so-called "stingless" ants, do really possess a sting, although it is but rudimentary, and, indeed, serves only as a support for the duct of the poison-gland. Now under these circumstances a sting might either be rudimentary in the sense of undeveloped, and the sting might represent a rudimentary and archaic structure from which the more perfect organ of the other ants, as, for instance, of the Myrmicidæ, had developed itself; or, secondly, it

* "Das der Formicidenstachel kein verkümmertes Organ ist, sondern ein auf der niedrigsten Stufe der Entwickelung stehen gebliebenes, aus dem der ausgebildete Stachel hervorging, wir es also nicht mit einem *Rückschritt* sondern mit einem primitivem Organe zu thun haben" (Zeitschrift für wissenschaftliche Zoologie, vol. xxviii. p. 551).

might be an organ which, having fallen out of use, had become atrophied. M. Dewitz adopts the former view. He concludes that the rudimentary sting of the Formicidæ is not a stunted and evanescent organ, but one which has remained in the lowest stage of development, from which the more perfect sting has originated—that we have to do not with a reduced, but with a primitive organ.

Any opinion expressed by M. Dewitz on such a subject is, of course, entitled to much weight; nevertheless there are some general considerations which seem to me conclusive against his view. If the sting of *Formica* represents a hitherto undeveloped organ, then the original ant was stingless, and the present stings of the aculeate ants have an origin independent of that belonging to the other aculeate Hymenoptera, such as bees and wasps. These organs, however, are so complex, and at the same time so similarly constituted, that they must surely have a common origin. Whether the present sting is derived from a leaf-cutting instrument, such as that from which the sawfly takes its name, I will at present express no opinion. M. Dewitz would surely not regard the rudimentary traces of wings in the larvæ of ants as undeveloped organs; why, then, should he adopt this view with reference to the rudimentary sting? On the whole I must regard the ancestral ant as having been aculeate, and consider that the rudimentary condition of the sting of *Formica* is due to atrophy, perhaps through disuse.

On the Arrangement of their Nests.

I have given the following figure (fig. 5), which represents a typical nest belonging to *Lasius niger*, because it seems to show some ideas of strategy. The nest is between two plates of glass, the outer border is a framework of wood, and the darker colour represents garden mould, which the ants have themselves excavated, as shown in the figure. For the narrow doorway (*a*), indeed, I am myself responsible. I generally made the doorways of my nests narrow, so as to check evaporation and keep the nests from becoming too dry. It will be observed, however, that behind the vestibule (*b*) the entrance contracts, still further protected by a pillar of earth, which leaves on either side a narrow passage which a single ant could easily guard, or which might be quickly blocked up. Behind this is an irregular vestibule (*c*), contracted again behind into a narrow passage, which is followed by another, this latter opening into the main chamber *d*. In this chamber

several pillars of earth are left, almost as if to support the roof. Behind the main chamber is an inner sanctum divided into three chambers, and to which access is obtained through narrow entrances (*f, f, f, f*). Most of the pillars in the main chamber are irregular in outline, but two of them (*g, g*) were regular ovals,

Fig. 5.

Ground-plan of a typical nest of *Lasius niger*, reduced. *a*, narrow doorway; *b*, widening beyond entrance; *c*, vestibule; *d*, main chamber; *e*, inner sanctum; *f, f, f, f*, narrow entrance passages to sanctum; *g, g*, special pillars.

and round each, for a distance about as long as the body of an ant, the glass had been most carefully cleaned. This was so marked, and the edge of the cleaned portion was so distinct that it is im-

possible not to suppose that the ants must have had some object in this proceeding, though I am unable to suggest any explanation of it.

On the treatment of Aphides.

Our countryman Gould, whose excellent little work on ants* has hardly received the attention it deserves, observes that "the queen ant [he is speaking of *Lasius flavus*] lays three different sorts of eggs, the slave, female, and neutral. The two first are deposited in the spring, the last in July and part of August; or, if the summer be extremely favourable, perhaps a little sooner. The female eggs are covered with a thin black membrane, are oblong, and about the sixteenth or seventeenth part of an inch in length. The male eggs are of a more brown complexion, and usually laid in March."

Here, however, our worthy countryman fell into an error, the eggs which he thus describes not being those of ants, but, as Huber correctly observed, of Aphides†. The error is the more pardonable, because the ants treat these eggs exactly as if they were their own, guarding and tending them with the utmost care. I first met with them in February 1876, and was much astonished, not being at that time aware of Huber's observations. I found, as Huber had done before me, that the ants took the greatest care of these eggs, carrying them off to the lower chambers with the utmost haste when the nest was disturbed. I brought some home with me and put them near one of my own nests, when the ants carried them inside. That year I was unable to carry my observations further. In 1877 I again procured some of the same eggs, and offered them to my ants, who carried them into the nest, and in the course of March I had the satisfaction of seeing them hatch into young Aphides. M. Huber, however, does not think these are mere ordinary eggs. On the contrary, he agrees with Bonnet, "that the insect, in a state nearly perfect, quits the body of its mother in that covering which shelters it from the cold in winter, and that it is not, as other germs are, in the egg surrounded by food by means of which it is developed and supported. It is nothing more than an asylum of which the Aphides born at another season have no need; it is on this account some are produced naked, others enveloped in a covering. The mothers

* An Account of English Ants. By the Rev. W. Gould, 1747, p. 36.

† My lamented friend Mr. Smith also observed these eggs (Entom. Annual, 1871). He did not, however, identify the species to which they belonged.

are not, then, truly oviparous, since their young are almost as perfect as they ever will be, in the asylum in which Nature has placed them at their birth"*.

This is, I think, a mistake. This is not the opportunity to describe the anatomy of the Aphis; but I may observe that I have examined the female, and find these eggs to arise in the manner so well described by Huxley in our 'Transactions'†, and which I have also myself observed in other Aphides and in allied genera‡. Moreover, I have opened the eggs themselves, and have also examined sections, and have satisfied myself that they are true eggs containing ordinary yelk. If examined while still in the ovary the germ-vesicle presents the usual appearance, but in laid eggs I was unable to detect it. So far from the young insect being "nearly perfect," and merely enveloped in a protective membrane, no limbs or internal organs are present. These bodies are indeed real ova, or pseudova; and the young Aphis does not develop in them until shortly before they are hatched.

When my eggs hatched I naturally thought that the Aphides belonged to one of the species usually found on the roots of plants in the nests of *Lasius flavus*. To my surprise, however, the young creatures made the best of their way out of the nest, and, indeed, were sometimes brought out by the ants themselves. In vain I tried them with roots of grass &c.; they wandered uneasily about, and eventually died. Moreover, they did not in any way resemble the subterranean species. In 1878 I again attempted to rear these young Aphides; but though I hatched a great many eggs, I did not succeed. This year, however, I have been more fortunate. The eggs commenced to hatch the first week in March. Near one of my nests of *Lasius flavus*, in which I had placed some of the eggs in question, was a glass containing living specimens of several species of plant commonly found on or around ants' nests. To this some of the young Aphides were brought by the ants. Shortly afterwards I observed on a plant of daisy, in the axils of the leaves, some small Aphides, very much resembling those from my nest, though we had not actually traced them continuously. They seemed thriving, and remained stationary on the daisy. Moreover, whether they had sprung from the black eggs or not, the ants evidently valued them, for they built up a wall of earth round and over them. So things remained throughout the summer; but on the 9th Oct. I found that

* The Natural History of Ants. By M. P. Huber, 1820, p. 246.

† Trans. Linn. Soc. vol. xxii. (1859). ‡ Philosophical Transactions 1859.

the Aphides had laid some eggs exactly resembling those found in the ants' nests; and on examining daisy-plants from outside, I found on many of them similar Aphides, and more or less of the same eggs.

I confess these observations surprised me very much. The statements of Huber have not, indeed, attracted so much notice as many of the other interesting facts which he has recorded; because if Aphides are kept by ants in their nests, it seems only natural that their eggs should also occur. The above case, however, is much more remarkable. Here are Aphides, not living in the ants' nests, but outside, on the leaf-stalks of plants. The eggs are laid early in October on the food-plant of the insect. They are of no direct use to the ants, yet they are not left where they are laid, where they would be exposed to the severity of the weather and to innumerable dangers, but brought into their nests by the ants, and tended by them with the utmost care through the long winter months until the following March, when the young ones are brought out and again placed on the young shoots of the daisy. This seems to me a most remarkable case of prudence. Our ants may not perhaps lay up food for the winter, but they do more, for they keep during six months the eggs which will enable them to procure food during the following summer.

No doubt the fact that our European ants do not generally store up food in the usual way is greatly due to the nature of their food. They live, as we know, partly on insects and other small animals which cannot be kept fresh; and they have not learnt the art of building vessels for their honey, probably because their young are not kept in cells like those of the honey-bee, and their pupæ do not construct firm cocoons like those of the humble-bee.

Moreover, it is the less necessary for them to do so, because if they obtain access to any unusual store of honey, that which they swallow is only digested by degrees and as it is required; so that, as the camel does with water, they carry about with them in such cases a supply of food which may last them a considerable time. They have, moreover, as we know, the power of regurgitating this food at any time, and so supplying the larvæ or less fortunate friends. Even in our English ants the quantity of food which can be thus stored up is considerable in proportion to the size of the insect; and if we watch, for instance, the little brown garden-ant (*Lasius niger*) ascending a tree to milk their

Aphides, and compare them with those returning full of honey, we shall see a marked difference in size.

On a new Species of Honey-Ant, Camponotus inflatus.

I have, indeed, no reason to suppose that in our English ants any particular individuals are specially told off to serve as receptacles of food. M. Wesmael, however, has described* a remarkable genus (*Myrmecocystus mexicanus*), brought by M. de Normann from Mexico, in which certain individuals in each nest serve as animated honey-pots. To them the foragers bring their supplies, and their whole duty seems to be to receive the honey, retain it, and redistribute it when required. Their abdomen becomes enormously distended, the intersegmental membranes being so much extended that the chitinous segments which alone are visible externally in ordinary ants seem like small brown transverse bars. The account of these most curious insects given by MM. de Normann and Wesmael has been fully confirmed by subsequent observers; as, for instance, by Lucas†, Saunders‡, Edwards§, Blake‖, Loew¶, and McCook.

On one very important point, however, M. Wesmael was in error; he states that the abdomen of these abnormal individuals "ne contient aucun organe; ou plutôt, il n'est lui-même qu'un vaste sac stomacal." Blake even asserts that "the intestine of the insect is not continued beyond the thorax," which must surely be a misprint; and also that there is no connexion "between the intestine and the cloaca"! These statements, however, are entirely erroneous; and, as M. Forel has shown, the abdomen does really contain the usual organs, which, however, are very easily overlooked by the side of the gigantic stomach.

I have now the honour of exhibiting to the Society a second species of ant, which has been sent me by Mr. Waller, in which a similar habit has been evolved and a similar modification has been produced. The two species, however, are very distinct, and the former is a native of Mexico, while the present comes from Adelaide in Australia. The two species, therefore, cannot be

* Bull. de l'Acad. des Sci. de Bruxelles.
† Ann. Soc. Ent. de France, v. p. 111.
‡ 'Canadian Entomologist,' vol. vii. p. 12.
§ Proc. California Academy, 1873. ‖ Ibid. 1874.
¶ American Nat. viii. 1874.

descended one from the other; and it seems incredible that the modification has originated independently in the two species.

It is interesting that, although these specimens apparently never leave the nest, and have little use therefore for legs, mandibles, &c., the modifications which they have undergone seem almost confined to the abdominal portion of the digestive organs. The head and thorax, antennæ, jaws, legs, &c. differ but little from those of ordinary ants.

CAMPONOTUS INFLATUS, n. sp. (Plate VIII.)

Operaria. Long. 15 mill. Nigra, tarsis pallidioribus; subtiliter coriacea, setis cinereo-testaceis sparsis; antennis tibiisque haud pilosis; tarsis infra hirsutis; mandibulis punctatis, hirsutis, sexdentatis; clypeo non carinato, antice integro; petioli squama modice incrassata, antice convexa, postice plana emarginata.

Hab. Australiam?

The colour is black, the feet being somewhat paler. The body is sparsely covered with stiff cinereo-testaceous hairs, especially on the lower and anterior part of the head, the mandibles, and the posterior edge of the thorax. The head and thorax are finely coriaceous.

The antennæ are of moderate length, twelve-jointed; the scape about one third as long as the terminal portion and somewhat bent. At the apex of the scape are a few short spines, bifurcated at the point. At the apex of each of the succeeding segments are a few much less conspicuous spines, which decrease in size from the basal segments outwards. The antenna is also thickly clothed with short hairs, and especially towards the apex with leaf-shaped sense-hairs. The clypeus is rounded, with a slightly developed median lobe and a row of stiff hairs round the anterior border; it is not carinated.

The mandibles have six teeth, those on one side (fig. 3) being rather more developed and more pointed than those on the other. They decrease pretty regularly from the outside inwards.

The maxillæ (fig. 5) are formed on the usual type. The maxillary palpi are six-jointed, the third segment being but slightly longer than the second, fourth, or fifth; while in *Myrmecocystus* the third and fourth are greatly elongated. The segments of the palpi have on the inner side a number of curious curved blunt hairs besides the usual shorter ones.

The labial palpi are four-jointed (fig. 4). The eyes are elliptical and of moderate size. The ocelli are not developed.

Fig. 1.

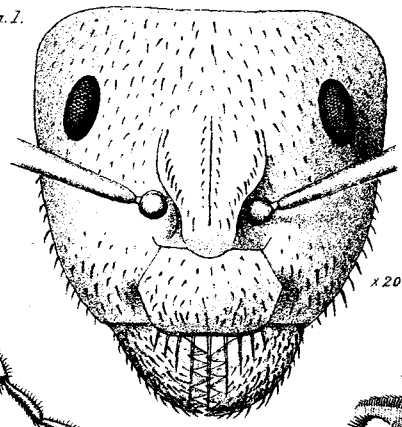


Fig. 2.

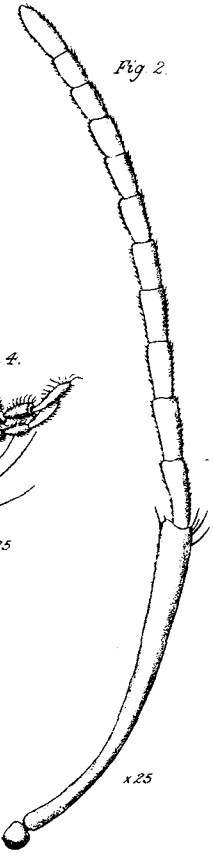


Fig. 5.

Fig. 4.

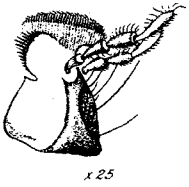


Fig. 3.

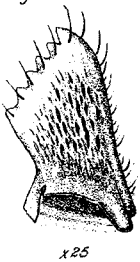
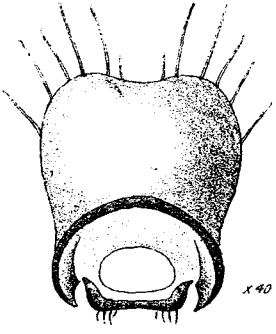


Fig. 6.



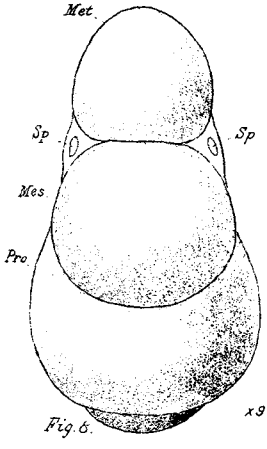


Fig. 6.

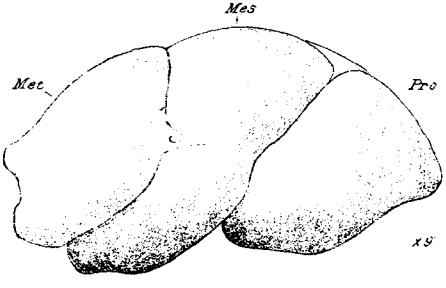


Fig. 7.

The thorax (figs. 7 and 8) is arched, broadest in front, without any marked incision between the meso- and metanotum; the mesonotum itself is, when seen from above, very broadly oval, almost circular, rather broader in front and somewhat flattened behind. Figs. 7 & 8 give outlines of the thorax, seen laterally and from above. The legs are of moderate length, the hinder ones somewhat the longest. The scale or knot (fig. 6) is heart-shaped, flat behind, slightly arched in front, and with a few stiff, slightly diverging hairs at the upper angles. The length is about two thirds of an inch.

DESCRIPTION OF PLATE VIII.

Fig. 1. *Camponotus inflatus.* Head, seen from above, ×20.
2. „ „ Antenna, „ ×25.
3. „ „ Mandible, „ „
4. „ „ Labium, „ „
5. „ „ Maxilla, „ „
6. „ „ Knot, seen from behind „
7. „ „ Outline of thorax, seen from the side, ×9.
8. „ „ Outline of thorax, seen from above, ×9.

Pro. Pronotum; *Mes.* Mesonotum; *Met.* Metanotum.

On the Genus *Solanocrinus*, Goldfuss, and its Relations to recent *Comatulæ*. By P. Herbert Carpenter, M.A., Assistant Master at Eton College.

[Read June 3, 1880.]

(Plates IX–XII.)

The genus *Solanocrinus* was established by Goldfuss* to include certain fossil Crinoids which he regarded as intermediate between the stalked *Pentacrini* and the free *Comatulæ*. He placed them among the stalked Crinoids, however, on account of their usually having a centrodorsal piece somewhat deeper than that of the few recent *Comatulæ* known to him; so that he was led to regard it as a short stem composed of but few joints. Between this so-called

* 'Petrefacta Germaniæ,' i. p. 162.