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OBSERVATIONS ON

BEES AND WASPS.

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The Social Hymenoptera, according to Messrs. Kirby and Spence*, "have the means of communicating to each other information of various occurrences, and use a kind of language which is mutually understood . . . . . and is not confined merely to giving intelligence of the approach or absence of danger; it is also coextensive with all their other occasions for communicating their ideas to each other."

Huber assures us as regards Ants † that he has "frequently seen the antennæ used on the field of battle to intimate approaching danger, and to ascertain their own party when mingled with the enemy; they are also employed in the interior of the ant-hill to warn their companions of the presence of the sun, so favourable to the development of the larvæ, in their excursions and emigrating to indicate their route, in their recruitings to determine the time of departure," &c. Elsewhere also he says ‡ "that should an Ant fall in with any of her associates from the nest they put her in the right way by the contact of their antennæ."

These statements are most interesting; and it is much to be regretted that he has not given us in detail the evidence on which they rest. In another passage, indeed, he himself says § "if they have a language, I cannot give too many proofs of it." Unfortunately, however, the chapter which he devotes to this important subject is very short, and occupied with general statements rather than with the accounts of the particular experiments and observations on which those statements rest. Nor is there any serious attempt to ascertain the nature, character, and capabilities of this antennal language. Even if by motions of these organs Bees can caress, can express love, fear, anger, &c., it does not follow that they can narrate facts or describe localities.

* Introduction to Entomology, ii. p. 50.
† *L. c.* p. 206.
‡ *L. c.* p. 157.
§ *L. c.* p. 205.

Nor are the facts recorded by Kirby and Spence more explicit. It is therefore disappointing to read in the chapter especially devoted to this subject *, that, as regards the power possessed by Ants and Bees to communicate and receive information, "it is only necessary to refer you to the endless facts in proof, furnished by almost every page of my letters on the history of Ants and of the Hive Bee. I shall therefore but detain you for a moment with an additional anecdote or two, especially with one respecting the former tribe, which is valuable from the celebrity of the narrator."

The first of these anecdotes refers to a Beetle (*Ateuchus pilularius*), which having made for the reception of its eggs a pellet of dung too heavy for it to move "repaired to an adjoining heap and soon returned with three of his companions. All four now applied their united strength to the pellet and at length succeeded in pushing it out, which being done, the three assistant Beetles left the spot and returned to their own quarters." This observation rests on the authority of an anonymous German artist; and though we are assured that he was a "man of strict veracity," I am not aware that any similar fact has been recorded by any other observer.

The second case is related by Kalm, on the authority of Dr. Franklin; but it does not seem to me to justify the conclusions drawn from it by Messrs. Kirby and Spence. Dr. Franklin having found a number of Ants in a jar of treacle, shook them out and suspended the jar "by a string from the ceiling. By chance one Ant remained, which, after eating its fill, with some difficulty found its way up the string, and, thence reaching the ceiling, escaped by the wall to its nest. In less than half an hour a great company of Ants sallied out of their hole, climbed the ceiling, crept along the string into the pot and began to eat again; this they continued until the treacle was all consumed, one swarm running up the string while another passed down. It seems indisputable that the one Ant had in this instance conveyed news of the booty to his comrades, who would not otherwise have at once directed their steps in a body to the only accessible route" †.

As regards Wasps, Huber states that they are also acquainted with the mode of imparting information to their companions. When a single Wasp discovers a strong-hold of sugar, honey,

* *L. c.* p. 422. † *L. c.* p. 422.

or other article of food, it returns to its nest and brings off in a short time a hundred other Wasps; but we are yet ignorant if it be by visible or palpable signs they are mutually informed of this discovery" *.

A short but very interesting paper by Dujardin on this subject is contained in the 'Annales des Sciences' for 1852. He satisfied himself that some Bees which came to honey put out by him for the purpose "avaient dû recevoir dans la ruche un avertissement porté par quelques-unes de celles qui étaient venues isolément, soit à dessein, soit par hasard" †. That no doubt might remain, he tried the following experiment, which, he says, "me paraît tout-à-fait concluante".

"Dans l'épaisseur d'un mur latéral, à 18 mètres de distance des ruches A et B, se trouve une niche pratiqué, suivant l'usage du pays, pour constater la mitoyenneté, et recouverte par un treillage et par une treille, et cachée par diverses plantes grimpantes. J'y introduisis, le 16 novembre, une soucoupe avec du sucre légèrement humecté; puis j'allai présenter une petite baguette enduite de sirop à une abeille sortant de la ruche A. Cette abeille s'étant cramponuée à la baguette pour sucer le sirop, je la transportai dans la niche sur le sucre, où elle resta cinq ou six minutes jusqu'à ce qu'elle se fut bien gorgée; elle commença alors à voler dans la niche, puis deçà et delà devant le treillage, la tête toujours tournée vers la niche, et enfin elle prit son vol vers la ruche, où elle rentra.

"Un quart d'heure se passa sans qu'il revînt une seule abeille à la niche; mais, à partir de cet instant, elles vinrent successivement au nombre de trente, explorant la localité, cherchant l'entrée de la niche qui avait dû leur être indiquée, et où l'odorat ne pouvait nullement les guider, et, à leur tour, vérifiant, avant de retourner à la ruche, les signes qui leur feraient retrouver cette précieuse localité ou qui leur permettraient de l'indiquer à d'autres. Tous les jours suivants les abeilles de la ruche A vinrent plus nombreuses à la niche où j'avais soin de renouveler le sucre humecté, et pas une seule de la ruche B n'eut le moindre soupçon de l'existence de ce trésor et ne vint voler de ce côté. Il était facile, en effet, de constater que les premières se dirigeaient exclusivement de la ruche à la niche, et réciproquement."

* Huber's Natural History of Ants, p. 374.

† Ann. des Sci. Nat. 1852, p. 233.

Considering the immense number of bees in a hive and the number of very young ones, it seems almost incredible that the bees of a hive should all be known to one another. Yet we are assured by some writers that it is so. Gelien, for instance, says, "Qu'une abeille tombe par accident, ou soit poussé par le vent dans une ruche qui n'est pas la sienne, elle est saisie et mise à mort à l'instant, comme suspecte de mauvais desseins"*.

Burmeister also, in his excellent 'Manual of Entomology,' says that "The power of communicating to their comrades what they purpose is peculiar to insects. Much has been talked of the so-called signs of recognition in bees, which is said to consist in recognizing their comrades of the same hive by means of peculiar signs. This sign serves to prevent any strange bee from intruding into the same hive without being immediately detected and killed. It, however, sometimes happens that several hives have the same signs, when their several members rob each other with impunity. In these cases the bees whose hive suffers most alter their signs, and then can immediately detect their enemy."†.

Huber mentions that some ants which he had kept in captivity having accidentally escaped, "met and recognized their former companions, fell to mutual caresses with their antennæ, took them up by their mandibles, and led them to their own nests; they came presently in a crowd to seek the fugitives under and about the artificial ant-hill, and even ventured to reach the bell-glass, where they effected a complete desertion by carrying away successively all the ants they found there. In a few days the *ruche* was depopulated. These ants *had remained four months without any communication*"‡. This statement has been very naturally copied by succeeding writers, and adopted without hesitation. See, for instance, Kirby and Spence's 'Introduction to Entomology,' vol. ii. p. 66, and Newport, 'Trans. of the Entomological Society of London,' vol. ii. p. 239.

Latreille also mentions that he once cut off the antennæ of an ant, and that one of its companions, "evidently pitying its sufferings, anointed the wounded part with a drop of transparent fluid from its mouth;" but the constant repetition of this statement in works on entomology indicates that other similar cases have not been met with. Messrs. Kirby and Spence, indeed, say that "whoever

* 'Le Conservateur des Abeilles,' p. 140.

† Burmeister's 'Entomology,' p. 502.

‡ Huber, p. 172.

attends to what is going forward in the neighbourhood of one of their nests, will be pleased to observe the readiness with which they seem disposed to assist each other in difficulties. When a burthen is too heavy for one, another will soon come to ease it of part of the weight; and if one is threatened with an attack, all hasten to the spot to join in repelling it"*.

These statements imply, on the part of bees, wasps, and ants, a great amount of intelligence. As I have already observed, however, the observations recorded do not seem to me in all cases to bear out the inferences that have been drawn from them. Moreover, when the conclusions are so important, we cannot be too sure of the facts; and however eminent, therefore, the authority may be, it is most desirable that the observations should be repeated.

Another question connected with these insects on which I was anxious to make some experiments was the use of the antennæ. That they are the means of communication there can be no doubt; but it is also the general opinion that they are, in addition, organs of sense. Whether, however, their functions are olfactory, or whether they serve as ears, is still a point on which entomologists are divided.

Our great entomologist Newport, in a paper specially devoted to the subject, says:—

"These facts, connected with the previous experiments, have convinced me that the antennæ in all insects are the auditory organs, whatever may be their peculiar structure—and that, however this is varied, it is appropriated to the perception and transmission of sound."

Dr. Ormerod also, who was so careful an observer of our British wasps, was of opinion that "the proper function of the antennæ seems to be that of an instrument of communication in the social tribes, and of an organ of hearing in insects generally"‡.

"The majority of modern physiologists and entomologists agree in explaining the antennæ as organs of hearing, as we have already remarked. Kirby and Spence's representation (whose names were inadvertently omitted to be mentioned there as the authorities for our opinions) conveys so much conviction that we may

* Vol. ii. p. 55.

† Newport, "On the Antennæ of Insects." Trans. Ent. Soc. vol. ii. p. 245.

‡ Natural History of Wasps, p. 73.

almost consider it settled, although we must at the same time admit that all the difficulties are not solved"*.

Dr. Braxton Hicks, also, and M. Lespès, who have specially studied the anatomical structure of antennæ, are of opinion that they are organs of hearing†.

The weight of authority, then, in favour of this view (comprising, as it does, Sulzer, Scarpa, Schneider, Borkhausen, Bonsdorf, Carus, Straus-Dürckheim, Oken, Burmeister, Kirby and Spence, Lespès, and Hicks) is very great. Nevertheless other eminent entomologists, as, for instance, Lyonet, Küster, Robineau-Desvoidy, Vogt, and Erichson, regard these organs as the seat of the sense of smell.

These are but a few of the many interesting questions which yet remain unsolved with reference to the social Hymenoptera. I present, therefore, the following observations to the Society with much diffidence; for I am well aware that they are but fragmentary. It will, however, be some months before I shall be able to prosecute them any further; and I trust that in some points they may be found not devoid of interest. I hope also that in consequence of bringing them before the Society I may receive some suggestions with reference to future inquiries.

Bees.

It will be observed that the current statements with reference to the language of social insects depend much on the fact that when one of them, either by accident or in the course of its rambles, has discovered a stock of food, in a very short time many others arrive to profit by the discovery. This, however, does not necessarily imply any power of describing localities. If the Bees or Ants merely follow their more fortunate comrade, the matter is simple enough; if, on the contrary, others are sent, the case becomes very different.

In order to test this, I proposed to keep honey in a given place for some time, in order to satisfy myself that it would not readily be found by the Bees, and then, after bringing a Bee to the honey, to watch whether it brought others, or sent them—the latter of course implying a much higher order of intelligence and power of communication.

I therefore placed some honey in a glass, close to an open

* Burmeister's 'Entomology,' p. 415.

† Transactions of the Linnean Society, vol. xxii. p. 395.

window in my sitting-room and watched it for sixty hours of sunshine, during which no bees came to it.

I then, at 10 o'clock in the month of June, went to my hives, and took a bee which was just starting out, brought it in my hand up to my room (a distance of somewhat less than 200 yards), and gave it some honey, which it sucked with evident enjoyment. After a few minutes it flew quietly away, but did not return; nor did any other bee make its appearance.

The following morning I repeated the same experiment. At 7·15 I brought up a bee, which sipped the honey with readiness, and after doing so for about five minutes flew away with no appearance of alarm or annoyance. It did not, however, return; nor did any other bee come to my honey.

On several other occasions I repeated the same experiments with a like result. Altogether I tried it more than twenty times; and I am satisfied that these bees cannot all have lost themselves or met with accidents. Indeed I never found bees to return if brought any considerable distance at once. By taking them, however, some twenty yards each time they came to the honey, I at length trained them to come to my room. On the whole, however, I found it more convenient to procure one of Marriott's observatory hives, both on account of its construction and also because I could have it in my room, and thus keep the bees more immediately under own eye. My room is square, with two windows on the south-west side, where the hive was placed, and one on the south-east. Besides the ordinary entrance from outside, the hive had a small postern door opening into the room; this door was provided with an alighting-board and closed by a plug; as a general rule the bees did not notice it much unless the passage was very full of them.

I then placed some honey on a table close to the hive, and from time to time fed certain bees on it. Those which had been fed soon got accustomed to come for the honey; but partly on account of my frequent absence from home, and partly from their difficulty in finding their way about, and their tendency to lose themselves, I never could keep any marked bee under observation for more than a few days.

Out of a number of similar observations I give the following in detail, as throwing some light on the power of communicating facts possessed by the bees; they will also illustrate the daily occupations of a working bee.

August 24. I opened the postern door at 6.45 and watched some marked bees till the middle of the day.

Bee no. 1.

6.50. One came to the honey. She then flew to the window, but after buzzing about for some time returned to the hive.
7.21. Back to honey. 7.23. Back to hive.
7.26. „ 7.30. Flew to window and then fell on the floor. I was afraid she would be trodden on, so at 7.45 I showed her the way to the hive.
8.40. Back to honey. 8.45. Back to hive. I now closed the postern door till 10.15.
10.35. Back to honey. 10.39. To hive.
10.45. „ and then to hive.
12.35. „ 12.37. To hive again.

Bee no. 2.

7. 0.	She came to the honey.	7. 5.	She went back to the hive.
7.12.	Back to the honey.	7.22.	„
7.24.	„	7.30.	„
7.42.	„	7.46.	„
7.52.	„	7.57.	„
8. 5.	„	8. 9.	„
8.15.	„	8.20.	„
8.26.	„	8.30.	„
8.40.	„	8.44.	„
8.55.	„	9. 0.	„

I then closed the door till 10.15; at 9.5, however, she came round to the honey through an open window, but could not find her way back, so I had to put her into the hive.

10.15.	Back to the honey.	10.17.	She went back to the hive.
10.20.	„	10.23.	„
10.30.	„	10.33.	„
10.50.	„	10.55.	„
11. 1.	„	11. 6.	„
11.17.	„	11.23.	„
11.33.	„	?	„
11.45.	„	11.50.	„
12. 0.	„	12. 3.	„
12.10.	„	12.15.	„
12.24.	„	12.30.	„

12.37.	Back to the honey.	12.43.	She went back to the hive.
12.52.	„	12.56.	„

Bee no. 3.

Also on August 24.

10.16.	Came to honey.	10.19.	Returned to hive.
10.30.	„	10.34.	„
10.55.	„	10.57.	„
11. 2.	„	11. 5.	„
11.11.	„	11.15.	„
11.24.	„	11.27.	„
11.35.	„	11.37.	„
11.45.	„	11.47.	„
11.57.	„	?	„
12.13.	„	12.16.	„
12.26.	„	12.30.	„
12.36.	„	12.42.	„
12.56.	„	12.59.	„

The next day I timed this bee as follows:—

7.23.	Came to honey.	7.25.	Returned to hive.
7.35.	„	7.37.	„
7.44.	„	7.45.	„
8.10.	„	8.12.	„
8.53.	„	8.55.	„

(The door was then closed till 9.30.)

9.35.	„	9.40.	To window, and at 9.49 to hive.
10.	„	10. 5.	Returned to hive.
10.13.	„	10.15.	„
10.22.	„	10.26.	„
10.35.	„	10.40.	„
10.45.	„	10.48.	„
10.56.	„	?	„
11. 7.	„	11.12.	„
11.18.	„	11.20.	„
11.35.	„	11.37.	„
11.47.	„	11.51.	„
12. 2.	„	12. 6.	„
12.25.	„	12.29.	„
12.51.	„	12.54.	„

August 26. Opened the postern at 6.30.

6.46.	The same bee as before came to the honey.	6.47.	Back to hive.
6.58.	She returned to the honey.	7. 0.	,,
7.23.	,,	7.25.	,,
7.32.	,,	7.35.	,,
7.45.	,,	7.48.	,,
7.55.	,,	7.59.	,,
8. 4.	,,	8. 7.	,,
8.19.	,,	8.22.	,,
8.39.	,,	8.43.	,,

During these observations scarcely any unmarked bees came to the honey.

In these cases, the postern being small, and on one side, was not very easily found. If the honey had been in an open place, no doubt the sight of their companions feasting would have attracted other bees; but in this case the honey was rather out of sight, being behind the hive-entrance, and was moreover only accessible by the narrow and winding exit through the little postern door.

But however exposed the honey might be, I found similar results, unless the bees were visible to their fellows. Thus on the 2nd, 3rd, 4th, and 5th October two or three marked bees were paying regular visits to some honey in my sitting-room; but during the whole time very few unmarked bees came to the honey.

I will now give a few more cases which tend to show that bees which have found a supply of sweets do not tell their fellows of the discovery.

9.19.	I brought a bee to some honey.	9.24.	She returned to the hive.
9.55.	She came back to the honey.	10. 0.	,, ,,
10. 8.	,, ,,	10.10.	,, ,,
10.16.	,, ,,	10.19.	,, ,,
10.28.	,, ,,	10.30.	,, ,,
10.37.	,, ,,	10.40.	,, ,,
10.50.	,, ,,	10.53.	,, ,,
11. 0.	,, ,,	11. 4.	,, ,,
11.11.	,, ,,	11.15.	,, ,,
11.22.	,, ,,	11.27.	,, ,,
11.34.	,, ,,	11.37.	,, ,,
11.46.	,, ,,	11.50.	,, ,,
11.55.	,, ,,	12. 0.	,, ,,
12. 6.	,, ,,	12. 7.	,, ,,

12.40.	She came back to the honey.	12.46.	She returned to the hive.
12.54.	,, ,,	12.57.	,, ,,
1. 2.	,, ,,	1. 4.	,, ,,
	Flew about.		
1.15.	,, ,,	1.18.	,, ,,
1.23.	,, ,,	1.27.	,, ,,
1.34.	,, ,,	1.41.	,, ,,
1.54.	,, ,,	2. 0.	,, ,,

After which she did not return. During this time no other bee came to the honey.

Again on another occasion I watched several bees, which on my list of marked bees stood as Nos. 3, 4, 7, 8, 10 and 11.

9.45.	Bee No. 10 came.	9.50.	Went back to hive.
10. 0.	,, 10 ,,	10. 3.	,,
10.18.	,, 10 ,,	10.21.	,,
10.26.	,, 11 ,,	10.30.	,,
10.30.	,, 4 ,,	10.35.	,,
10.36.	,, 7 ,,	10.45.	,,
10.46.	,, 4 ,,	10.52.	,,
10.49.	,, 7 ,,	10.52.	,,
11. 0.	,, 7 ,,	11. 9.	,,
11. 5.	,, 4 ,,	11. 9.	,,
11.11.	,, 7 ,,	11.16.	,,
11.21.	,, 7 ,,	11.29.	,,
11.22.	A strange bee came.		
11.26.	Bee No. 4 came.	11.31.	,,
11.30.	,, 7 ,,	11.39.	,,
,,	,, 10 ,,	11.36.	,,
11.40.	,, 4 ,,	11.45.	,,
11.45.	,, 7 ,,	11.50.	,,
11.47.	,, 10 ,,	11.59.	,,
,,	Another strange bee came.		
12. 1.	Bee No. 4 ,,	12. 6.	,,
12. 2.	,, 7 ,,	12. 8.	,,
12. 3.	,, 3 ,,	12. 7.	,,
12. 4.	,, 10 ,,	12. 7.	,,
12.14.	,, 7 ,,	12.18.	,,
12.17.	,, 4 ,,	12.21.	,,
12.24.	,, 7 ,,	12.31.	,,
12.30.	,, 10 ,,	12.33.	,,
12.36.	,, 7 ,,	12.46.	,,
12.37.	,, 4 ,,	12.44.	,,
12.37.	,, 10 ,,	12.40.	,,
12.45.	,, 10 ,,	12.49.	,,
12.50.	,, 7 ,,	12.54.	,,
12.50.	,, 4 ,,	12.54.	,,
12.53.	,, 10 ,,	12.56.	,,

12.57.	Bee No. 7 came.	1. 0.	Went back to hive.
12.57.	„ 4 „	1. 2.	„
1. 0.	„ 10 „	?	„
1. 2.	„ 7 „	1. 6.	„
1. 9.	„ 4 „	1.12.	„
1.10.	„ 8 „	1.16.	„
1.10.	„ 7 „	1.16.	„
1.16.	„ 4 „	1.19.	„
1.17.	„ 5 „	1.21.	„
1.20.	„ 7 „	1.24.	„
1.20.	„ 8 „	1.25.	„
1.21.	„ 4 „	1.24.	„
1.23.	„ 5 „	1.27.	„
1.29.	„ 4 „		
1.29.	„ 7 „		

After this I ceased recording in detail; but the above shows that while the marked bees came regularly, only in two cases did any unmarked bees come to the honey.

In the above cases the honey was poured into saucers, but not weighed. In the following I used a wide-mouthed jar containing rather more than 1 lb. of honey.

1.44.	Bee No. 5 came.	1.45.	Went away.
1.54.	„ 5 „	1.58.	„
2. 2.	„ 5 „	2. 5.	„
2. 9.	„ 5 „	2.13.	„
2. 9.	„ 1 „	2.15.	„
2.18.	„ 5 „	2.20.	„
2.19.	„ 1 „	2.21.	„
2.28.	„ 1 „	2.31.	„
2.37.	„ 1 „	2.41.	„
2.32.	„ 5 „	2.40.	„
3.49.	„ 5 „	2.51.	„
2.52.	„ 1 „	2.55.	„
3.10.	A stranger came which I numbered as No. 14.		
3.11.	Bee No. 1 came.	3.13.	Went away.
3.19.	„ 5 „	3.22.	„
3.20.	„ 1 „	3.23.	„
3.19.	„ 14 „	3.23.	„
3.30.	„ 5 „	3.32.	„
3.31.	„ 14 „	3.33.	„
3.37.	„ 1 „	3.40.	„
3.38.	„ 5 „	3.42.	„
3.38.	„ 14 „	3.41.	„
3.47.	„ 5 „	3.49.	„
3.46.	„ 14 „	3.51.	„ } She was disturbed.
3.54.	„ 14 „	3.56.	„

4.	Bee No. 1 came.	4. 3.	Went away.
4.	„ 5 „	4. 3.	„
4. 5.	„ 14 „	4.11.	„
4.10.	„ 5 „	4.12.	„
4.15.	„ 14 „	4.20.	„
4.22.	„ 1 „	4.25.	„
4.24.	„ 14 „	4.29.	„
4.26.	„ 5 „	4.29	„

During the whole of this time only one strange bee came, as recorded above.

In the following case I put out, besides 1 lb. of honey, also 4 oz. of honey spread over two plates.

12.15.	One of my marked bees came.	12.21.	She went.
12.26.	She returned.	12.31.	„
12.36.	„	12.44.	„
12.51.	„	12.57.	„
1. 4.	„	1.12.	„
1.15.	„	1.19.	„
1.25.	„	1.32.	„
1.38.	„	1.44.	„
1.49.	„	1.55.	„
2.	„	2. 7.	„
2.14.	„	2.19.	„
2.25.	„	2.33.	„
2.38.	„	2.44.	„
2.50.	„	2.58.	„
3. 5.	„	3.13.	„
3.20.	„	3.32.	„ She was dis-
3.39.	„	3.45.	„ [turbed.
3.52.	„	4.	„
4. 7.	„	4. 9.	„
4.15.	„	4.20.	„
4.27.	„	4.32.	„
4.43.	„	4.45.	„
4.50.	„	4.59.	„
5. 7.	„	5.13.	„
5.25.	„	5.31.	„
5.42.	„	5.48.	„
5.56.	„	6. 1.	„
6.14.			

During this time no other bee came to the honey.

Not satisfied with this, I procured a fine honeycomb weighing 12½ lbs, and brought to it one of my marked bees

at 2.40.		2.47.	She went back to the hive.
3. 4.	She returned.	3.13.	„ „
3.27.	„	3.37.	„ „
3.46.	„	3.56.	„ „
4. 6.	„	4.18.	„ „
4.26.	„	4.44.	„ „
4.54.	„	5.10.	„ „
5.18.	„	5.26.	„ „
5.36.	„	5.46.	„ „
5.54.	„	6. 7.	„ „
6.16.	„	6.27.	„ „
6.34.	„	6.46.	„ „
6.51.	„	7. 4.	„ „
7.14.	„		

During the whole of which time only one strange bee came. In this case it will be observed that she remained longer at the honey than in the previous instances. The intervals during which she was away were as follows:—

1st	visit 9	minutes,
2nd	„ 10	„
3rd	„ 8	„
4th	„ 10	„
5th	„ 8	„
6th	„ 10	„
7th	„ 8	„
8th	„ 9	„
9th	„ 7	„
10th	„ 5	„
11th	„ 10	„

It seems obvious, then, that the bees which had found the honey did not communicate their discovery to the others.

Though the bees came readily out through the little postern door of my observatory hive, they had much difficulty in finding their way back until they had done so several times. For instance, the following may be taken as a typical case:—

August 8th.

At 6.50 a bee came out through the little postern door. After she had fed, she evidently did not know her way home; so I put her back.

At 7.10 she came out again. I again fed her and put her back.

At 10.15 she came out a third time; and again I had to put her back.
At 10.55 she came out again, and still did not remember the door. Though I was satisfied that she really wished to return, and was not voluntarily remaining outside, still, to make the matter clear, I turned her out of a side window into the garden, when she at once returned to the hive.
At 11.15 she came out again; and again I had to show her the way back.
At 11.20 she came out again; and again I had to show her the way back (this makes five times); when, however,
at 11.30 she came out again after feeding, she returned straight to the hive.
At 11.40 she came out, fed, and returned straight to the hive.
At 11.50 she came out, fed, and returned straight to the hive; she then stayed in for some time.
At 12.30 she came out again, but seemed to have forgotten the way back; after some time, however, she found the door and went in.

Again:—August 24 at 7.20 a bee came through the postern; I fed her; and though she was not frightened or disturbed, when she had finished her meal she flew to the window and had evidently lost her way; so at 8 o'clock I in pity put her back myself.

August 29. A bee came out to the honey at 10.10; at 10.12 she flew to the window and remained buzzing about till 11.12, when, being satisfied that she could not find her way, I put her in.

Nay, even those who seemed to know the postern, if taken near the other window, flew to it, and seemed to have lost themselves.

This cost me a great many bees. Those which got into my room by accident continually died on the floor near the window.

This is also well shown by the following experiments:—At 10.15 I put a bee into a bell-glass 18 inches long and with a mouth $6\frac{1}{2}$ inches wide, turning the closed end to the window; she buzzed about till 11.15, when, as there seemed no chance of her getting out, I put her back into the hive. Two flies, on the contrary, which I put in with her, got out at once. At 11.30 I put another bee and a fly into the same glass; the latter flew out at once. For half an hour the bee tried to get out at the closed end; I then turned the glass with its open end to the light, when she flew out at once. To make sure, I repeated the experiment once more, with the same result.

Some bees, however, have seemed to me more intelligent in this respect than others. A bee which I had fed several times and which had flown about in the room, found its way out of the glass in a quarter of an hour, and when put in a second time came out at once. Another bee, when I closed the postern door, used to come round to the honey through an open window.

Bees seem to me much less clever in finding things than I had expected. One day (April 14, 1872) when a number of them were very busy on some berberries, I put a saucer with some honey between two bunches of flowers; these were repeatedly visited, and were so close that there was hardly room for the saucer between them, yet from 9.30 to 3.30 not a single bee took any notice of the honey. At 3.30 I put some honey on one of the bunches of flowers, and it was eagerly sucked by the bees; two kept continually returning till past five in the evening.

One day when I came home in the afternoon I found that at least a hundred bees had got into my room through the postern and were on the window, yet not one was attracted by an open jar of honey which stood in a shady corner about 3 feet 6 inches from the window.

One day (29th April, 1872) I placed a saucer of honey close to some Forget-me-nots, on which bees were numerous and busy; yet from 10 A.M. till 6 only one bee went to the honey.

I put some honey in a hollow in the garden wall opposite the hives at 10.30 (this wall is about five feet high and four feet from the hives); yet the bees did not find it during the whole day.

On the 30th March, 1873, a fine sunshiny day, when the bees were very active, I placed a glass containing honey at 9 in the morning on the wall in front of the hives; but not a single bee went to the honey the whole day. On April 20 I tried the same experiment, with the same result.

September 19. At 9.30 I placed some honey in a glass about four feet from and just in front of the hive; but during the whole day not a bee observed it.

As it then occurred to me that it might be suggested that there was something about this honey which rendered it unattractive to the bees, on a following day I placed it again on the top of the wall for three hours, during which not a single bee came, and then moved it close to the alighting-board of the hive. It remained unnoticed for a quarter of an hour, when two bees observed it; and others soon followed in considerable numbers.

Some days, indeed, the bees did not seem to care about honey. Thus, September 19, I placed eleven bees one by one on some honey not far from the hive; they all fed well and returned quietly to the hive, but not one came back to the honey.

Indeed, under such circumstances, though the bees almost invariably fed with every appearance of enjoyment, comparatively few returned to the honey, even when it was not above 20 or 30 yards from the hive.

As regards time, the examples given above may be taken as fair illustrations; and on the whole it seems that, if honey is easily procurable and near the hive, a bee will on an average make about five excursions in the hour.

Sometimes, however, a bee will stay for hours inside the nest, even when the day is suitable and other bees are out; for instance, on the 24th August a marked bee remained in the hive all the morning.

Burmeister, in the passage already quoted (ante p. 115), says that bees have a sign which serves to prevent any strange bee from intruding into the hive without being immediately detected and killed, This seems to rest on a statement of Gélien, who believed that in each hive the bees had some common sign or pass-word. As evidence of this, he mentions * that one of his hives had been for some days robbed by the bees from another; "et je désespérais de conserver cet essaim, lorsqu'un jour, sur le soir, je le vis fort inquiet, fort agité, comme s'il eût perdu sa reine. Les abeilles couraient en tout sens sur le devant et le tablier de la ruche, se flairant, se tâtant mutuellement, comme si elles eussent voulu se dire quelque chose. C'était pour changer leur signe de reconnaissance, qu'elles changèrent en effet pendant la nuit. Toutes les pillardes qui revinrent le lendemain, furent arrêtées et tuées. Plusieurs échappèrent aux gardes vigilantes qui défendaient l'entrée, avertirent sans doute les autres du danger qu'elles avaient couru, et que l'on ne pouvait plus piller impunément. Aucune de celles qui voulurent recommencer leur déprédation, ne pénétra dans la ruche dont elles avaient fait leur proye, et qui prospéra merveilleusement."

Dujardin, however, has suggested another explanation of this case. He thinks that the behaviour of the bees indicated not a change of sign or password, but an alteration in the state of the

* Le Conservateur des Abeilles, p. 143.

queen in relation to the colony, which thus resumed its ordinary condition, and found itself in a position to repel the invaders. However this may be, the observation of Gélien, though curious and interesting, scarcely seems to bear out the conclusion he has drawn from it.

So far as my own observations go, though bees habitually know and return to their own hive, still, if placed on the alighting-board of another, they enter it without molestation. Thus:—

On May 4 I put a strange bee into a hive at 2 o'clock. She remained in till 2.20, when she came out, but entered again directly. I was away most of the afternoon, but returned at 5.30; at six she came out of the hive, but soon returned; and after that I saw no more of her.

May 12. A beautiful day, and the bees very active. I placed twelve marked bees on the alighting-board of a neighbouring hive. They all went in; but before evening ten had returned home.

May 13. Again put twelve marked bees on the alighting-board of another nest; eleven went in. The following day I found that seven had returned home; the other five I could not see.

May 17. Took a bee and, after feeding her and marking her white, put her to a hive next but one to her own at 4.18. She went in.

4.22. Came out and went in again.

4.29. Came out. I fed her and sent her back.

4.35. Came out. Took a little flight and came back. 4.45 went in, but returned. 4.52. Went in.

4.53. Came out. 4.56. „

4.57. „ 4.58. „

5. 1. „ , took another little flight, and returned. I fed her again. 5.25. Went in again.

5.28. Came out again. 5.29. „

5.31. „ 5.33. „

5.36. „ 5.40. „

5.46. Shut her and the others in with a piece of note-paper.

6.36. One of the bees had eaten its way through. I opened the door; and several, including the white one, came out directly. Till 6.50 she kept on going in and out every minute or two. Hardly any bees were flying, only a few standing at the doors of most of the hives. At 7.20 she was still at the hive-door.

May 20. Between 6 and 7 I marked a bee and transferred her to another hive.

May 21. Watched from 7.30 to 8.9 in the morning without seeing her. At half past six went down again, directly saw and fed her. She was then in her new hive; but a few minutes after I observed her on the lighting-stage of her old hive; so I again fed her, and when she left my hand she returned to the new hive.

May 22. 8 o'clock. She was back in her old hive.

May 23. About 12.30 she was again in the new hive.

As far as my experience goes, bees which have stung and lost their sting always die; not, however, immediately. On August 25 a bee which had come several times to my honey was startled, flew to one of the windows, and had evidently lost her way. While I was putting her back, she stung me, and lost her sting in doing so. I put her in through the postern, and for twenty minutes she remained on the landing-stage; she then went into the hive, and after an hour returned to the honey. After this, however, I did not see her any more.

As regards the affection of bees for one another, it is no doubt true that when they have got any honey on them, they are always licked clean by the others; but I am satisfied that this is for the sake of the honey rather than of the bee. On the 27th of September, for instance, I tried with two bees: one had been drowned, the other was smeared with honey. The latter was soon licked clean; of the former they took no notice whatever. I have, moreover, repeatedly placed dead bees by honey on which live ones were feeding, but the latter never took the slightest notice of the corpses.

Dead bees are indeed usually carried out of the hive; but if one is placed on the alighting-stage, the others seem to take no notice of it, though it is soon pushed off by the movements of the others. I have even seen the bees sucking the juices of a dead pupa.

Light.—Though bees do not come out at night, they seem to be much affected by light. One evening I lit a small covered lamp to go down to the cellar. A bee which was out came to it, and, flying round and round like a moth, followed me the whole of the way there.

Colour.—I have also made a number of experiments with reference to colours, on which, however, I will not now dwell. I will only say that it seems clear that bees can distinguish colours. For

instance, on the 2nd of October I placed some honey on slips of glass resting on black, white, yellow, orange, green, blue, and red paper. A bee which was placed on the orange returned twenty times to that slip of glass, only once or twice visiting the others, though I moved the position and also the honey. The next morning again two or three bees paid twenty-one visits to the orange and yellow, and only four to all the other slips of glass. I then moved the glass, after which, out of thirty-two visits, twenty-two were to the orange and yellow. These and other experiments seemed to me to show a real disposition, which was also well marked in the case of wasps, towards the orange and yellow. That they can see blue, however, is indicated by the following experiment:—Oct. 6. I had ranged my colours in a line, with the blue at one end. It was a cold morning, and only one bee came. She had been several times the preceding day, generally to the honey which was on the blue paper. This day also she came to the blue; I moved the blue gradually along the line one stage every half hour, during which time she paid fifteen visits to the honey, in every case going to that which was on the blue paper.

Sound.—Aug. 29. The result of my experiments on the hearing of bees has surprised me very much. It is generally considered that to a certain extent the emotions of bees are expressed by the sounds they make *, which seems to imply that they possess the power of hearing. I do not by any means intend to deny that this is the case. Nevertheless I never found them take any notice of any noise which I made, even when it was close to them. I tried one of my bees with a violin. I made all the noise I could, but to my surprise she took no notice. I could not even see a twitch of the antennæ. The next day I tried the same with another bee, but could not see the slightest sign that she was conscious of the noise. On Aug. 31 I repeated the same experiment with another bee, with the same result. On the 12th and 13th of September I tried several bees with a dog-whistle and a shrill pipe; but they took no notice whatever, nor did a set of tuning-forks which I tried on a subsequent day have any more effect. These tuning-forks extended over three octaves, beginning with *a* below the ledger-line. I also tried with my voice, shouting &c. close to the head of a bee; but in spite of my utmost efforts, the

* See for instance Landois, Zeits. f. wiss. Zool. 1867, p. 184.

bees took no notice. I repeated these experiments at night when the bees were quiet; but no noise that I could make seemed to disturb them in the least.

Temper.—I found the temper of the bees very variable. Generally they allowed me to handle them without any sign of irritation; while at other times, without any reason which I could discover, they stung me sometimes several times in a day; they seemed the more prone to do so the hotter the weather.

Wasps.

Sept. 18. I had in my room a nest of Humble Bees, which I fed with honey. The honey was also visited by wasps. One evening I marked one of these wasps (No. 1) which visited this honey; she was a large female of *V. germanica*; her last visit to the honey that day was at 6.30.

The next morning she came for the first time

at 7.25, and fed till 7.28, when she began flying about the room and even into the next; so I thought it well to put her out of the window, when she flew straight away to her nest. My room, as already mentioned, had windows on two sides; and the nest was in the direction of a closed window, so that the wasp had to go out of her way in going out through the open one.

At 7.45 she came back. I had moved the glass containing the honey about 2 yards; and though it stood conspicuously, the wasp seemed to have much difficulty in finding it. Again she flew to the window in the direction of her nest, and I had to put her out, which I did at 8.2.

At 8.15 she returned to the honey almost straight. 8.21, she flew again to the closed window, and apparently could not find her way; so at 8.35 I put her out again. It seems obvious from this that wasps have a sense of direction, and do not find their way merely by sight.

At 8.50 back to honey, and 8.54 again to wrong window; but finding it closed, she took two or three turns round the room, and then flew out through the open window.

At 9.24 back to the honey; and 9.27 away, first, however, paying a visit to the wrong window, but without alighting.

At 9.36. Back to the honey, and 9.39 away, but, as before, going first to wrong window. She was away therefore 9 minutes.

9.50.	"	"	9.53	away, this time straight.	"	11	"
10.	"	"	10. 7	"	"	11	"
10.19.	"	"	10.22	"	"	12	"
10.35.	"	"	10.39	"	"	13	"
10.47.	"	"	10.50	"	"	9	"
11. 4.	"	"	11. 7	"	"	14	"
11.21.	"	"	11.24	"	"	14	"
11.34.	"	"	11.37	"	"	10	"
11.49.	"	"	11.52	"	"	11	"
12. 3.	"	"	12. 5	"	"	11	"
12.13.	"	"	12.15½	"	"	8	"
12.25.	"	"	12.28	"	"	10	"
12.39.	"	"	12.43	"	"	11	"
12.54.	"	"	12.57	"	"	11	"
1.15.	"	"	1.19	"	"	18	"
1.27.	"	"	1.30	"	"	8	"

Here for the first time another specimen came to the honey.

At 1.37. Back to the honey, and 1.39 away (was rather disturbed,

1.46.	"	"	1.49	"	as I tried to mark her).	Interval 7	"
1.54.	"	"	1.58	"		" 5	"
2. 5.	"	"	2. 7	"		" 7	"
2.15.	"	"	2.19	"		" 8	"
2.27.*	"	"	2.32	"		" 8	"
2.39.	"	"	2.42	"		" 7	"
2.50.	"	"	2.54	"		" 8	"
3. 2.	"	"	3. 6	"		" 8	"
3.14.	"	"	3.17	"		" 8	"
3.26.	"	"	3.29	"		" 9	"
3.38.	"	"	3.42	"		" 9	"
3.50.	"	"	3.58	"		" 8	"
4. 7.	"	"	4.12	"		" 9	"
4.20.	"	"	4.23	"		" 8	"
4.32.	"	"	4.36	"		" 9	"
4.46.	"	"	4.49	"		" 10	"
5.	"	"	5. 3	"		" 11	"
5.13.	"	"	5.17	"		" 10	"
5.26.	"	"	5.30	"		" 9	"
5.40.	"	"	5.44	"		" 10	"
5.54.	"	"	5.59	"		" 10	"
6. 7.	"	"	6.11	"		" 8	"
6.20.	"	"	6.25	"		" 9	"

She did not come any more that day; but, as will be seen,

* She very often, however, throughout the day, in going away, flew to the other window first, and then, without alighting, returned to and went through the open one.

she had made forty-five visits to the honey in eleven hours. During the whole of this time no strange wasp, except the one above mentioned, came to this honey.

The following day, September 20th, this wasp made her appearance in my room at 6.55, when she flew straight to the honey.

At 6.55 came to the honey.		6.59 went away.				
7. 8	,,	7.10	,,	Absent 9 minutes.		
7.18	,,	7.22	,,	,,	8	,,
7.30	,,	7.32	,,	,,	8	,,
7.41	,,	7.45	,,	,,	9	,,
7.53	,,	7.56	,,	,,	8	,,
8. 4	,,	8. 7	,,	,,	8	,,
8.15	,,	8.18	,,	,,	8	,,
8.27	,,	8.30	,,	,,	9	,,
8.38	,,	8.41	,,	,,	8	,,
8.50	,,	8.53	,,	,,	9	,,
9. 1	,,	9. 4	,,	,,	8	,,
9.12	,,	9.15	,,	,,	8	,,
9.22	,,	9.25	,,	,,	7	,,
9.34	,,	9.36	,,	,,	9	,,
9.46	,,	9.51	,,	,,	10	,,
10. 1	,,	10. 3	,,	,,	10	,,
10.13	,,	10.18	,,	,,	10	,,
10.28	,,	10.30	,,	,,	10	,,
10.38	,,	10.42	,,	,,	8	,,
10.53	,,	10.56	,,	,,	11	,,
11. 7	,,	11.11	,,	,,	11	,,
11.21	,,	11.25	,,	,,	10	,,
11.32	,,	11.36	,,	,,	7	,,
The wasp which came once yesterday returned and rather disturbed the first.						
At 11.49 came to the honey.		11.50 went away.		,,	13	,,
11.57	,,	12	,,	,,	7	,,
12. 8	,,	12.11	,,	,,	8	,,
Here I was away for about two hours.						
2.42 came to the honey.		2.46 went away.				
2.58		3. 2	,,	Interval 12 minutes.		
3.15	,,	3.17	,,	,,	13	,,
3.25	,,	3.28	,,	,,	8	,,
Here I was called away.						
4.25 came to the honey.		4.28	,,			
4.41	,,	4.45	,,	,,	13	,,
5.15	,,	5.19	,,	,,	30	,,
5.30	,,	5.35	,,	,,	11	,,
5.45	,,	5.50	,,	,,	10	,,
6. 2	,,	6. 6	,,	,,	12	,,
6.15	,,	6.17	,,	,,	9	,,

This was the last visit that day. She made therefore thirty-eight visits during the time she was watched, which was not quite eight hours. She was at work from 6.55 to 6.15; and assuming that she was occupied in the same manner during the three hours when she was not watched, as during the rest of the time, she would have made over fifty visits to the honey during the day.

Wishing, however to have a complete record of a day's work, I watched her the following day without intermission.

September 21. I began watching at ten minutes past six.

6.16.	She came to the honey.	6.19.	She went away.			
6.29.	,,	6.32.	,,	An interval of	10	minutes.
6.41.	,,	6.44.	,,	,,	9	,,
6.55.	,,	7.	,,	,,	11	,,
7.11.	,,	7.15.	,,	,,	11	,,
7.23.	,,	7.26.	,,	,,	8	,,
7.37.	,,	7.42.	,,	,,	11	,,
7.56.	,,	8. 3.	,,	,,	14	,,
	Was disturbed and seemed rather troubled.					
8.11.	She came to the honey.	8.14.	She went away.	An interval of	8	minutes.
8.20.	,,	8.24.	,,	,,	6	,,
8.31.	,,	8.34.	,,	,,	7	,,
8.40.	,,	8.42.	,,	,,	6	,,
8.50.	,,	8.52.	,,	,,	8	,,
8.58.	,,	9.	,,	,,	6	,,
9. 8.	,,	9.11.	,,	,,	8	,,
9.18.		9.22.	,,	,,	7	,,
9.30.	,,	9.32.	,,	,,	8	,,
9.39.	,,	9.40.	,,	,,	7	,,
9.50.	,,	9.54.	,,	,,	10	,,
10. 1.	,,	10. 5.	,,	,,	7	,,
10.14.	,,	10.17.	,,	,,	9	,,
10.25.	,,	10.28.	,,	,,	8	,,
10.37.	,,	10.40.	,,	,,	9	,,
10.47.	,,	10.51.	,,	,,	7	,,
11.	,,	11. 6.	,,	,,	9	,,
11.17.	,,	11.20.	,,	,,	11	,,
11.34.	,,	11.37.	,,	,,	14	,,
11.50.	,,	11.53.	,,	,,	13	,,
12. 5.	,,	12. 8.	,,	,,	12	,,
12.20.	,,	12.24.	,,	,,	12	,,
12.36.	,,	12.40.	,,	,,	12	,,
1. 8.	,,	1.11.	,,	,,	28	,,
1.26.	,,	1.28.	,,	,,	15	,,
1.40.	,,	1.42.	,,	,,	12	,,
1.57.	,,	2. 2.	,,	,,	15	,,

2.10.	She came to the honey.	2.13.	She went away.	An interval of	8 minutes
2.25.	,,	2.30.	,,	12	,,
2.45.	,,	2.56.	,,	15	,,

She buzzed about at the other window for a few minutes, which made the interval longer than usual.

3.13.	She came to the honey.	3.18.	She went away.	An interval of	17 minutes
3.29.	,,	3.31.	,,	11	,,
3.41.	,,	3.45.	,,	10	,,
3.49.	,,	3.52.	,,	4	,,
4. 2.	,,	4. 6.	,,	7	,,
4.19.	,,	4.22.	,,	13	,,
4.29.	,,	4.33.	,,	7	,,
4.40.	,,	4.44.	,,	7	,,
4.51.	,,	4.53.	,,	7	,,
5. 4.	,,	5. 6.	,,	11	,,
5.16.	,,	5.20.	,,	10	,,
5.32.	,,	5.35.	,,	12	,,
5.45.	,,	5.50.	,,	10	,,

It will be seen that the intervals of her absence were remarkably regular. On one occasion, indeed, she was only away four minutes; but this time I think she had been disturbed and had not provided herself with a regular supply of food.

The number of visits was fifty-one in eleven hours and a half. I tried whether she would be in any way affected by a dead wasp, so I put one on the honey; but she took no notice whatever.

I observed with other wasps, that when the open window was not the shortest way to their nests, they had a great tendency to fly to that which was in the right direction, and to remain buzzing about there.

During the whole of this day, only four or five strange wasps came to the honey.

As regards the regularity of their visits, and the time occupied, other wasps which I observed agreed very closely with this one. For comparison, it may be worth while to give one or two other cases. I will commence with that of a worker, I believe *V. vulgaris*, observed on the 19th September.

10 A.M. I put her to the honey, she fed and then flew about the room and at last got into my bee-hive.

10.54. She came in again at the window. I again put her to the honey. She again flew all about the room.

11.41. She returned and this time came to the honey; but when she had fed again flew round and round the room, and

did not seem able to find her way out. I therefore put her out.

12·11. She returned, and the same thing happened again.

12.28.	She came back to the honey.	12.31.	Flew straight away.	
12.45.	,,			
12.53.	,,	12.57.	,,	
1.10.	,,			
1.26.	,,	1.29.	,,	
				Interval
1.38.	,,	1.41.	,,	9 minutes.
1.50.	,,	1.53.	,,	9 ,,
2. 3.	,,	2. 6.	,,	10 ,,
2.12.	,,	2.16.	,,	6 ,,
		Was disturbed.		
2.20.	,,	2.25.	,,	4 ,,
2.40.	,,	2.43.	,,	15 ,,
2.51.	,,	2.54.	,,	8 ,,
3. 1.	,,	3. 4.	,,	7 ,,
3.13.	,,	3.16.	,,	9 ,,
3.25.	,,	3.28.	,,	9 ,,
3.35.	,,	3.38.	,,	7 ,,
3.46.	,,	3.50.	,,	8 ,,
3.58.	,,	4. 1.	,,	8 ,,
4 10.	,,	4.14.	,,	9 ,,
4.23.	,,	4.25.	,,	9 ,,
4.34.	,,	4.38.	,,	9 ,,
4.46.	,,	4.50.	,,	8 ,,
4.58.	,,	5. 4.	,,	8 ,,
5.14.	,,	Was disturbed and flew about.		8 ,,

She did not return any more that evening, but made her appearance again at half-past six the next morning.

From twelve o'clock, when she had learnt her way, till five, she made twenty-five visits in five hours, or about five an hour, as in the previous cases.

It struck me as curious that on the following day this wasp seemed by no means so sure of her way, but over and over again went to the closed window.

I will give one other illustration:—

September 21. At 11.50 I fed a wasp.

11.56.	She returned to honey.		11.57.	Flew away.	
12. 6.	,,	,,	12. 8.	,,	
1.25.	,,	,,	1.27.	,,	
1.37.	,,	,,	1.39.	,,	
1.57.	,,	,,	2. 0.	,,	

2.15.	She returned	to honey.	2.17.	Flew	away.
2.22.	,,	,,	2.25.	,,	
2.32.	,,	,,	2.36.	,,	
2.50.	,,	,,	2.55.	,,	
3. 2.	,,	,,	3. 4.	,,	
3.14.	,,	,,	3.18.	,,	
3.28.	,,	,,	3.30.	,,	
3.40.	,,	,,	3.44.	,,	
3.51.	,,	,,	3.55.	,,	
4. 4.	,,	,,	4. 8.	,,	
4.16.	,,	,,	4.20.	,,	
?	,,	,,	4.31.	,,	
4.37.	,,	,,	4.41.	,,	
4.46.	,,	,,	4.48.	,,	
4.57.	,,	,,	5.	,,	
5. 9.	,,	,,	5.12.	,,	
5.22.	,,	,,	5.26.	,,	
5.31.	,,	,,	5.36.	,,	

After the above facts we may, I think, well say "How doth the little busy *wasp*." Even Mr. Ormerod seems hardly to have done justice to his favourites. He is very severe on those wasps which "take up their quarters on the wrong sides of our window." "I have nothing" he continues * "to say on behalf of these wasps; they are a nuisance and a terror to all who have little children. They are mere stragglers, who have lost all feeling of good fellowship, have deserted their nest, and are leading a freebooter's life." Many of them, on the contrary, I am satisfied, are perfectly respectable wasps which have unfortunately lost their way.

My experiments, then, in opposition to the statements of Huber and Dujardin, seem to show that wasps and bees do not convey to one another information as to food which they may have discovered. No doubt, when one wasp has discovered and is visiting a supply of syrup, others are apt to come too; but I believe that they merely follow one another. If they communicated the fact, considerable numbers would at once make their appearance; but I have never found this to be the case. The frequent and regular visits which my wasps paid to the honey put out for them proves that it was very much to their taste; yet few others made their appearance. For instance, on the 19th September, as recorded above, only one wasp came of herself to the honey; this wasp returned on the 20th, but not one other. The 21st was a hot day, and there were many wasps

* Natural History of Wasps, p. 245.

about the house; my honey was regularly visited by the two marked wasps; but during the whole day only five others came to it.

September 22. Again only only one strange wasp came up to one o'clock.

September 27. Only one strange wasp came.

October 2 and 3. These days were cold; a few marked bees and wasps came to my honey, but no strangers.

October 4. Two strangers.

October 6. Only one stranger.

On these days the honey was watched almost without intermission the whole day, and was more or less regularly visited by the marked bees and wasps.

These and other observations of the same tendency seem to show that, even if wasps have the power of informing one another when they discover a store of good food, at any rate they do not habitually do so.

On the whole, wasps seem to me more clever in finding their way than bees. I tried wasps with the glass mentioned on p. 124; but they had no difficulty in finding their way out.

Sounds.—My wasps, though courageous, were always on the alert, and easily startled. It was, for instance, much more difficult to paint them than the bees; nevertheless, though I tried them with a set of tuning-forks covering three octaves, with a shrill whistle, a pipe, a violin, and my own voice, making in each case the loudest and shrillest sounds in my power, I could see no symptoms in any case that they were conscious of the noise.

I made also a number of experiments with reference to colour, which have satisfied me that wasps, like bees, are capable of distinguishing colours. I am anxious, however, to repeat and extend these observations, and shall then hope to have the opportunity of laying them before the Society.

The following fact struck me as rather remarkable. The wasp already mentioned at the foot of p. 135 one day smeared her wings with syrup, so that she could not fly. When this happened to a bee, it was only necessary to carry her to the alighting-board, when she was soon cleaned by her comrades. But I did not know where this wasp's nest was, and therefore could not pursue a similar course with her. At first, then,

I was afraid that she was doomed. I thought, however, that I would wash her, fully expecting, indeed, to terrify her so much that she would not return again. I therefore caught her, put her in a bottle half full of water and shook her up well till the honey was washed off. I then transferred her to a dry bottle and put her in the sun. When she was dry I let her out, and she at once flew to her nest. To my surprise, in 13 minutes she returned as if nothing had happened, and continued her visits to the honey all the afternoon.

This experiment interested me so much that I repeated it with another marked wasp, this time, however, keeping the wasp in the water till she was quite motionless and insensible. When taken out of the water she soon recovered; I fed her; she went quietly away to her nest as usual, and returned after the usual absence. The next morning this wasp was the first to visit the honey.

I was not able to watch any of the above-mentioned wasps for more than a few days; but I kept a specimen of *Polistes gallica* for no less than three months.

I took her, with her nest, in the Pyrenees early in May. The nest consisted of about twenty cells, the majority of which contained an egg; but as yet no grubs had been hatched out, and, of course, my wasp was as yet alone in the world.

I had no difficulty in inducing her to feed on my hand; but at first she was shy and nervous. She kept her sting in constant readiness; and once or twice in the train, when the railway officials came for tickets, and I was compelled to hurry her back into her bottle, she stung me slightly—I think, however, entirely from fright.

Gradually she became quite used to me, and when I took her on my hand apparently expected to be fed. She even allowed me to stroke her without any appearance of fear, and for some months I never saw her sting.

When the cold weather came on she fell into a drowsy state, and I began to hope she would hibernate and survive the winter. I kept her in a dark place, but watched her carefully, and fed her if ever she seemed at all restless.

She came out occasionally, and seemed as well as usual till near the end of February, when one day I observed she had nearly lost the use of her antennæ, though the rest of the body was as usual. She would take no food. Next day I tried again to feed her; but the head seemed dead, though she could still move her

legs, wings, and abdomen. The following day I offered her food for the last time; but both head and thorax were dead or paralyzed; she could but wag her tail, a last token, as I could almost fancy, of gratitude and affection. As far as I could judge, her death was quite painless; and she now occupies a place in the British Museum.

Ants.

My experiments with ants have not been very successful; I may, however, just mention the following:—

On the 29th of December I took some red ants and placed them in a glass in my room. On the 4th of March following I put four of them back into their nest, but could not see any sign of joy on their part, or any evidence that they were recognized by their former companions. As, however, they soon went down into their nest and were out of sight, this observation was not very satisfactory. I therefore took some of the ants which had been left in the nest, and placed them in the glass. They joined the others, and crossed antennæ in the usual way; but I saw no special signs of satisfaction or recognition. For the sake of comparison, I put some other red ants with them, and I could observe no difference of behaviour.

[*Extracted from the* Linnean Society's Journal.—Zoology, vol. xii.]

Observations on Bees, Wasps, and Ants.—Part II.

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

In the Twelfth Volume of the Journal, the Society has done me the honour to publish some observations on Bees and Wasps, of which the present paper is a continuation.

Bees.

Following up the observations recorded in my previous paper, on the 19th July I put a bee (No. 10) to a honeycomb containing 12 lbs. of honey

at	12.30;		at	12.36	she went back to the hive;
„	12.50	she returned;	„	12.55	„ „
„	1. 6	„	„	1.12	„ „
„	1.53	„	„	1.57	„ „
„	2. 5	„	„	2. 9	„ „
„	2.16	„	„	2.20	„ „
„	2.28	„	„	2.32 ?	„ „
„	2.49	„	„	2.55	„ „
„	3.13	„	„	3.20	„ „
„	3.31	„	„	3.39	„ „
„	3.45	„	„	3.55	„ „
	4. 2	„	„	4. 8	„ „
„	4.18	„	„	4.24	„ „
„	4.31	„	„	4.37	„ „
„	8.47	„	„	4.58	„ „
„	5.10	„	„	5.19	„ „
„	5.27	„	„	5.30	„ „
„	6. 9	„	„	6.15	„ „
„	6.23	„	„	6.29	„ „
„	7.19	„	„	7.24	„ „
„	7.35	„	„	7.40	„ „
„	7.50	„	„	7.55	„ „

and during all this time no other bee came to the comb.

On the following morning, July 20, this bee came to the honeycomb

at	6. 5 A.M.;		at	6.10	she went back to the hive;
„	6.37	she returned;	„	6.42	„ „
„	7.17	„	„	7.21	„ „

at	7.41	she returned;	at	7.47	she went back to the hive;
„	8. 8	„	„	8.12	„ „
„	8.21	„	„	8.25	„ „
„	8.32	„	„	8.54	„ „
„	9. 4	„	„	9. 9	„ „
„	9.45	„	„	9.51	„ „
„	10. 4	„	„	10.10	„ „
„	10.19	„	„	10.26	„ „
„	10.40	„	„	10.47	„ „
„	10.59	„	„	11. 4	„ „
„	11.14	„	„	11.19	„ „
„	11.44	„	„	11.52	„ „
„	11.59	„	„	12. 6	„ „
„	12.15	„	„	12.23	„ „
„	12.29	„	„	12.35	„ „
„	12.41	„ was disturbed,	„	12.52	„ „
„	1. 2	„	„	1. 9	„ „
„	1.16	„	„	1.30	„ „
„	1.46	„	„	1.55	„ „

I then left off observing; but during the whole of this time no other bee had come to the comb.

Oct. 9. I took a bee (No. 11) out of the hive and put her to some honey; she returned and kept on visiting it regularly.

Oct. 10. This bee came to the honey at 7.30 A.M., and went on visiting it; but I was not able to watch her continuously. During these two days no other bee came to this honey.

Oct. 11. No. 11 came to the honey

at	7.12 A.M., but did not alight;				
„	7.18	she returned, and at 7.21 went back to the hive;			
„	7.27	„	„	7.31	„
„	7.38	„	„	7.44	„
„	7.51	„	„	7.56	„
„	8. 2	„	„	8. 8	„
„	8.15	„	„	8.22	„
„	3.30	„	„	8.35	„
„	8.41	„	„	8.46	„
„	8.55	„	„	8.59	„
„	9. 6	„	„	9.11	„
„	9.20	„	„	9.25	„
„	9.45	„	„	9.50	„

Oct. 11. No. 11 (*continued*).

at 9.55 she returned, and at 10. 1 went back to the hive;
„ 10. 7 „ „ 10.11 „
„ 10.19 „ „ 10.23 „
„ 10.30 a strange bee came; I killed her.
At 10.35 No. 11 returned, and at 10.40 went back to the hive;
„ 10.55 „ „ 10.59 „
„ 11. 4 „ „ 11. 8 „
„ 11.26 „ „ 11.30 „
„ 11.35 „ „ 11.38 „
Another strange bee came; I killed her also.
At 11.52 she returned, and at 11.55 went;
„ 12. 7 „ „ 12.12 „
„ 12.17 „ „ 12.22 „
„ 12.31 „ „ 12.36 „
„ 12.58 „ „ 1. 2 „
„ 1. 8 „ „ 1.12 „
„ 1.19 „ „ 1.23 „
1.30 „ „ 1.34 „
1.45 „ „ 1.48 „
2. 2 „ „ 2. 6 „
„ 2.15 „ „ 2.18 „
„ 2.29 „ „ 2.35 „
„ 2.45 „ „ 2.47 „
„ 2.50 „ „ 2.52 „
„ 2.57 „ „ 3 „

after which she did not come any more that day. It was, however, a bad day, and after 1 o'clock she was almost the only bee which came out of the hive. The following morning she came to the honey at 7.58 A.M., but did not alight, behaving just as she had done the day before.

At 8. 6 A.M. No. 11 returned to the honey, and at 8. 9 she went;
„ 8.14 „ „ „ 8.20 „
„ 8.30 „ „ „ 8.34 „
„ 8.42 „ „ „ 8.46 „
„ 8.54 „ „ „ 8.59 „
„ 9. 9 „ „ „ 9.14 „
„ 9.19 „ „ „ 9.24 „
„ 9.29 „ „ „ 9.33 „
„ 9.37 „ „ „ 9.44 „
„ 9.54 „ „ but was disturbed.

A strange bee came, which I killed. At 9.59 No. 11 went away;
at 10. 5 she returned to the honey, „ 10. 8 „
„ 10.12 „ „ „ 10.13 „
„ 10.16 „ „ „ 10.20 „
„ 10.26 „ „ „ 10.28 „
„ 10.33 „ „ „ 10.36 „
„ 10.40 „ „ „ 10.46 „
„ 10.55 a strange bee came which I killed. No. 11 returned to the honey regularly; and went on coming.

Oct. 13. 6.28 A.M. she came, but, as before, flew away again without alighting.

At 6.32 she came to the honey, at 6.36 she went away;
„ 6.42 „ „ „ 6.46 „
„ 6.51 „ „ „ 6.56 „
„ 7.10 „ „ „ 7.14 „
„ 7.26 „ „ „ 7.34 „
„ 7.46 „ „ „ 7.50 „
„ 7.55 „ „ „ 8. „
„ 8.12 „ „ „ 8.15 „
„ 8.20 „ „ „ 8.26 „
„ 8.30 „ „ „ 8.33 „
„ 8.37 „ „ „ 8.44 „
„ 8.50 „ „ „ 8.56 „
and so on.

Oct. 14. She came for the first time at 8.15 A.M., and went on visiting the honey at the usual intervals. After this day I saw her no more; she had probably met with some accident. But these facts show that some bees, at any rate, do not communicate with their sisters, even if they find an untenanted comb full of honey, which to them would be a perfect Eldorado. This is the more remarkable because these bees began to work in the morning before the rest, and continued to do so even in weather which drove all the others into the shelter of the hive. That the few strange bees which I have recorded should have found the honey is natural enough, because there were a good many bees about in the room.

The following fact is mentioned by F. Müller as seeming also to show a limited power of communicating facts on the part of bees: —"Once," he says*, "I assisted at a curious contest, which took place between the queen and the worker bees in one of my hives,

* 'Nature,' June 11, 1874.

and which throws some light on the intellectual faculties of these animals. A set of forty-seven cells had been filled, eight on a nearly completed comb, thirty-five on the following, and four around the first cell of a new comb. When the queen had laid eggs in all the cells of the two older combs she went several times round their circumference (as she always does, in order to ascertain whether she has not forgotten any cell), and then prepared to retreat into the lower part of the breeding-room. But as she had overlooked the four cells of the new comb, the workers ran impatiently from this part to the queen, pushing her, in an odd manner, with their heads, as they did also other workers they met with. In consequence the queen began again to go around on the two older combs; but as she did not find any cell wanting an egg she tried to descend, but everywhere she was pushed back by the workers. This contest lasted for a rather long while, till the queen escaped without having completed her work. Thus the workers knew how to advise the queen that something was as yet to be done, but they knew not how to show her *where* it had to be done."

I have already mentioned with reference to the attachment which bees have been said to show for one another, that though I have repeatedly seen them lick a bee which had smeared herself in honey, I never observed them show the slightest attention to any of their comrades who had been drowned in water. Far, indeed, from having been able to discover any evidence of affection among them, they appear to be thoroughly callous and utterly indifferent to one another. As already mentioned, it was necessary for me occasionally to kill a bee; but I never found that the others took the slightest notice. Thus on the 11th of October I crushed a bee close to one which was feeding—in fact, so close that their wings touched; yet the survivor took no notice whatever of the death of her sister, but went on feeding with every appearance of composure and enjoyment, just as if nothing had happened. When the pressure was removed, she remained by the side of the corpse without the slightest appearance of apprehension, sorrow, or recognition. It was, of course, impossible for her to understand my reason for killing her companion; yet neither did she feel the slightest emotion at her sister's death, nor did she show any alarm lest the same fate should befall her also. In a second case exactly the same occurred. Again, I have several times, while a bee has been feeding, held a second bee by the leg

close to her; the prisoner, of course, struggled to escape and buzzed as loudly as she could; yet the selfish (?) eater took no notice whatever. So far, therefore, from being at all affectionate, I doubt whether bees are in the least fond of one another.

Their devotion to their queen is generally quoted as a most characteristic trait; yet it is of the most limited character. For instance, I was anxious to change my black queen for a Ligurian; and accordingly on the 26th of October Mr. Hunter was good enough to bring me a Ligurian queen. We removed the old queen, and we placed her with some workers in a box containing some comb. I was obliged to leave home on the following day; but when I returned on the 30th I found that all the bees had deserted the poor queen, who seemed weak, helpless, and miserable. On the 31st the bees were coming to some honey at one of my windows, and I placed this poor queen close to them. In alighting, several of them even touched her; yet not one of her subjects took the slightest notice of her. The same queen, when afterwards placed in the hive, immediately attracted a number of bees.

Although the experiments on colour which I have already recorded seem to me tolerably conclusive, still I thought it would be worth while to make a few more. Accordingly, on the 12th July I brought a bee to some honey which I placed on blue paper, and about 3 feet off I placed a similar quantity of honey on orange paper. After she had returned twice, I transposed the papers; but she returned to the honey on the blue paper. After she had made three more visits, always to the blue paper, I transposed them again, and she again followed the colour, though the honey was left in the same place. The following day I was not able to watch her; but on the 14th, at

7.29 A.M. she returned to the honey on the blue paper. 7.31 left.
7.44 „ 7.47 „
7.56 „ „

I then again transposed the papers. At 8.5 she returned to the old place, and was just going to alight; but observing the change of colour, without a moment's hesitation, darted off to the blue. No one who saw her at that moment could have entertained any further doubt about her perceiving the difference between the two colours. At 8.9 she went;

8.13 she returned to the blue. 8.16 went.
8.20 „ „ 8.23 „
8.26 „ „ 8.30 „

Transposed the colours again.

At 8.35 she returned to the blue, and at 8.39 went;
8.44 „ „ 8.47 „
8.50 „ „ 8.53 „

Transposed the colours again.

8.57 she returned again to the blue. 9 „
9. 4 „ „ 9. 7 „
9.12 „ „ 9.15 „
9.19 „ „ 9.22 „
9.25 „ „ 8.27 „
9.30 „ „ 9.34 „
9.40 „ „ 9.44 „
9.50 „ „ 9.55 „

Transposed the colours again.

10. 2 she returned again to the blue. 10. 6 „
10.10 „ „ 10.14 „
10.20 „ „ 10.25 „
10.30 „ „ 10.34 „
10.40 „ „ 10.44 „
10.48 „ „ 10.51 „
11.12 „ „ 11.14 „
11.21 „ „ and flew about, having been disturbed.
11.26 „ „ 11.28 went.
11.36 „ „ 11.40 „
12. 5 came and flew about, but did not settle till 12.17. 12.17 went;
12.21 came and flew about.

Though it was a beautiful afternoon, she did not return any more that day.

That bees can distinguish scents is certain. On the 5th Oct. I put a few drops of Eau de Cologne in the entrance, and immediately a number (about fifteen) of bees came out to see what was the matter. Rose-water also had the same effect; and, as will be mentioned presently, in this manner I called the bees out several times; but after a few days they took hardly any notice of the scent. For instance, on the 17th Oct. I tried them with twenty drops of Eau de Cologne, the same quantity of essence of violet, of lavender-water, of essence of musk, of essence of Patchouli, and of spirits of wine; but they took no apparent notice of any of them.

I have also made some observations with the view of ascertaining whether the same bees act as sentinels. With this object, on the 5th of October, I called out the bees by placing some eau de Cologne in the entrance, and marked the first three bees that came out. At 5 P.M. I called them out again; about twenty came, including the three marked ones. I marked three more.

Oct. 6. Called them out again. Out of the first twelve five were marked ones. I marked three more.

Oct. 7. Called them out at 7.30 A.M. as before. Out of the first niue, seven were marked ones.

At 5.30 P.M. called them out again. Out of six, five were marked ones.

Oct. 8. Called them out at 7.15. Six came out, all marked ones.

Oct. 9. Called them out at 6.40. Out of the first ten, eight were marked ones.

„ „ „ 11.30 A.M. Out of six, three were marked. I marked the other three.

„ „ „ 1.30 P.M. Out of ten, six were marked.

„ „ „ 4.30. Out of ten, seven were marked.

Oct. 10. „ „ 6.5 A.M. Out of six, five were marked.

„ Shortly afterwards I did the same again, when out of eleven, seven were marked ones.

„ 5.30, P.M. Called them out again. Out of seven, five were marked.

Oct. 11. 6.30 A.M. Called them out again. Out of nine, seven were marked.

„ 5 P.M. „ Out of seven, five were marked.

After this day they took hardly any notice of the scents.

Thus in these nine experiments, out of the ninety-seven bees which came out first, no less than seventy-one were marked ones, though out of the whole number of bees in the hive there were only twelve marked for this purpose, and, indeed, even fewer in the earlier experiments. I ought, however, to add that I generally fed the bees when I called them out.

It is sometimes said that the bees of one hive all know one

another, and immediately recognize and attack any intruder from another hive. At first sight this certainly implies a great deal of intelligence. It is, however, possible that the bees of particular hives have a particular smell. Thus Langstroth, in his interesting 'Treatise on the Honey Bee,' says :—"Members of different colonies appear to recognize their hive companions by the sense of smell;" and I believe that if colonies are sprinkled with scented syrup, they may generally be safely mixed. Moreover, a bee returning to its own hive with a load of treasure is a very different creature from a hungry marauder; and it is said that a bee, if laden with honey, is allowed to enter any hive with impunity. Mr. Langstroth continues, "There is an air of roguery about a thieving bee which, to the expert, is as characteristic as are the motions of a pickpocket to a skilful policeman. Its sneaking look and nervous guilty agitation, once seen, can never be mistaken." It is at any rate natural that a bee which enters a wrong hive by accident should be much surprised and alarmed, and would thus probably betray herself.

On the whole, then, I do not attach much importance to their recognition of one another as an indication of intelligence.

I had made some observations also with the view of ascertaining whether the bees which collect honey also work in the hive and attend to the brood, or whether they devote themselves exclusively to one or other of these duties. My observations, however, were not conclusive; but some light has been thrown on the subject by Dzierzon, from which it would appear that for the first fortnight of a bee's life she attends exclusively to indoor duties, and only afterwards takes to the collection of honey and pollen. Dzierzon's statements have been confirmed by Dr. Dönhoff. On the 18th April he introduced a Ligurian queen into a hive of black bees. The first Ligurian workers emerged on the 10th May, and made their first appearance outside the hive on the 17th; but not until the 25th did any of the Ligurian workers appear on his feeding-troughs, which were constantly crowded with common bees, nor were any seen to visit the flowers. Repeated observations, says Dr. Donhoff, "force me to conclude that during the first two weeks of the worker-bee's life the impulse for gathering honey and pollen does not exist, or at least is not developed, and that the development of this impulse proceeds slowly and gradually. At first the young bee will not even touch the honey presented to her; some days later she will simply taste it; and only

after a lapse of time will she consume it eagerly. Two weeks elapse before she readily eats honey; and nearly three weeks pass before the *gathering*-impulse is sufficiently developed to impel her to fly abroad and seek for honey and pollen among the flowers"*.

In my first memoir I alluded to the difficulty which bees experience in finding their way about. In this respect they certainly differ considerably. Some of the bees which came out through the little postern door (already described) were able to find their way back after it had been shown to them a few times. Others were much more stupid; thus, one bee came out on the 9th, 11th, 12th, 14th, 15th, 16th, 17th, 18th, and 19th, and came to the honey; but though I repeatedly put her back through the postern, she was never able to find her way for herself.

I often found that if bees which were brought to honey did not return at once, still they would do so a day or two afterwards. For instance, on July 11, 1874, a hot thundery day, and when the bees were much out of humour, I brought twelve bees to some honey; only one came back, and that one only once; but on the following day several of them returned.

My bees sometimes ceased work at times when I could not account for their doing so. Oct. 19 was a beautiful, sunshiny, warm day. All the morning the bees were fully active. At 11.25 I brought one to the honey-comb, and she returned at the usual intervals for a couple of hours; but after that she came no more, nor were there any other bees at work. Yet the weather was lovely, and the hive is so placed as to catch the afternoon sun.

I have made a few observations to ascertain, if possible, whether the bees generally go to the same part of the hive. Thus,

Oct. 5. I took a bee out of the hive, fed her and marked her. She went back to the same part.

Oct. 9. At 7.15 I took out two bees, fed and marked them. They returned; but I could not see them in the same part of the hive. One, however, I found not far off.

At 9.30 brought out four bees, fed and marked them. One returned to the same part of the hive. I lost sight of the others.

Since their extreme eagerness for honey may be attributed rather to their anxiety for the commonweal than to their desire for personal gratification, it cannot fairly be imputed as greediness; still the following scene, one which most of us have witnessed, is incompatible surely with much intelligence. "The sad

* 'Hive- and Honey-Bee,' Langstroth, p. 195.

fate of their unfortunate companions does not in the least deter others who approach the tempting lure from madly alighting on the bodies of the dying and the dead, to share the same miserable end. No one can understand the extent of their infatuation until he has seen a confectioner's shop assailed by myriads of hungry bees. I have seen thousands strained out from the syrup in which they had perished; thousands more alighting even upon the boiling sweets; the floor covered and windows darkened with bees, some crawling, others flying, and others still, so completely besmeared as to be able neither to crawl nor fly—not one in ten able to carry home its ill-gotten spoils, and yet the air filled with new hosts of thoughtless comers"*.

If, however, bees are to be credited with any moral feelings at all, I fear the experience of all bee-keepers shows that they have no conscientious scruples about robbing their weaker brethren. "If the bees of a strong stock," says Langstroth, "once get a taste of forbidden sweets, they will seldom stop until they have tested the strength of every hive." And, again, "Some bee-keepers question whether a bee that once learns to steal ever returns to honest courses." Siebold has mentioned similar facts in the case of wasps (*Polistes*).

Wasps.

Sept. 13. At 6 A.M. I put a wasp to some honey on green paper, and about a foot off I put some more honey on orange paper. The wasp kept returning to the honey at the usual intervals. At 8.30 I transposed the papers; but the wasp followed the colour. At 9 o'clock I transposed the papers again, but not the honey; she returned again to the green, from which it would appear that she was following the colour, not the honey. At 10.20 I again transposed them, with the same result.

Ants.

M. Forel, in his excellent work 'Les Fourmis de la Suisse.' asserts that Ants, when they first quit the pupal state, like the bees, devote themselves to household duties and the care of the young, not taking any part in the defence of the nest until a later period of life. He has repeated many of Huber's experiments. As regards the memory of ants, he convinced himself that they recognized their companions after a separation of

* 'Hive- and Honey-Bee,' Langstroth, page 277.

four months; but he believes they would not do so for more than one season. In my previous memoir I have described the behaviour of ants to companions from whom they had been separated for several months, and mentioned that I could not satisfy myself as to the lively manifestations of joy and satisfaction described by Huber as being shown under such circumstances. M. Forel, in the above-mentioned work, expresses his opinion that the signs which Huber regarded as marks of affection, were in reality signs of distrust and fear, which, however, were soon removed.

Ants of different nests are generally enemies; but M. Forel assures us (p. 262) that when they first quit the pupa-stage, ants do not distinguish friends from foes, though three or four days are sufficient to enable them to do so. It is to be regretted that he does not give the facts on which this interesting statement is based.

The behaviour of ants to one another differs very much according as they are alone or supported by numerous companions. An ant which would run away in the first case, will fight bravely in the second (p. 249).

MM. Forel and Ebrard both assert that if an ant is a little ill or slightly wounded, she is carefully tended by her companions; while, on the other hand, those which are dangerously ill or wounded are carried out of the nest to die. I have not met with any cases of this kind.

Again, some days I found no ants about on my window-sill as usual, although there seemed nothing in the weather to account for it.

I quote the following in order to show the steadiness with which ants work.

July 13. At 6.20 A.M. I put an ant to some honey; at 6.40 she went, 7.2 she returned, and at 7.8 went away again, but not to the nest; at 7.11 she returned, and at 7.15 went away again.

At	7.27	she came back.	7.40 went.
	7.49	„	8. 5 „
	8.14	„	8.19 „
	8.31	„	8.39 „
	8.43	„	8.47 „
	8.55	„	9 „
	9. 8	„	9.10 „
	9.17	„	9.26 „

At	9.34	she came back, and	at 9.40 went;
	9.49	„	10 „
	10.11	„	10.20 „
	10.27	„	10.36 „
	10.44	„	10.52 „
	12.52	„	12.54 „
	1. 3	„	1.20 „
	1.30	„	1.41 „
	1.51	„	2. 6 „

after which I was unable to go on watching.

Another ant the same morning

came to the honey at	6.55 A.M.,	at	7. 4 went away.
Returned at	7.10	„	7.14 „
„	7.34	„	7.36 „
„	7.45	„	7.50 „
„	8. 2	„	8. 7 „
„	8.17	„	8.22 „
„	8.31	„	8.36 „
„	8.44	„	8.58 „
„	8.59	„	9 „

after which she came back no more. During this time fifteen others had come to the honey.

That ants have a certain power of communication has been proved by Huber and other observers. Several striking cases are mentioned by M. Forel. For instance (*op. cit.* p. 297), an army of Amazon ants, on an expedition in search of slaves, attacked a nest of *Formica rufibarbis*. In a few seconds (quelques secondes) the dome of the nest was covered with *F. rufibarbis*, which rushed out to defend their house.

On another occasion he placed a number of *Tetramorium cæspitum* about four inches from a colony of *Pheidole pallidula*. "En un clin d'œil," he says (p. 384), "l'alarme fut repandue, et des centaines de Pheidole se jetèrent au devant de l'ennemi."

Again, he (p. 349) placed some earth containing a number of *Tetramorium* about four inches from a nest of *Strongylognathus Huberi*. Several combats took place; but after the lapse of a few minutes (quelques minutes) a whole army of *S. Huberi* emerged and attacked the intruders.

On another occasion, some Amazon ants (p. 301) were searching in vain for a nest of *Formica rufibarbis*. After a while some of them found the nest. "Immediately" (aussitôt), he says, "a

signal was given, the Amazons rushed in the right direction and pillaged the nest in spite of its inhabitants." This is a surprising statement. If it is to be taken literally, the communication cannot have been made by the antennæ; the signal can hardly have been a visible one; are we then to imagine a sound or smell to have been made use of which our auditory and olfactory nerves are incapable of perceiving? or have ants some sense which we do not possess?

It would even appear, from M. Forel's statements, that in some cases one species comprehends the signs of another. This is, of course, the case when different species live in association; but I am now speaking of hostile species. *Formica sanguinea*, he assures us, understand the signals of *F. pratensis*. "Elles savent," he says (p. 359), "toujours saisir l'instant où les *pratensis* se communiquent le signal de la déroute, et elles savent s'apprendre cette découverte les unes aux autres avec une rapidité incroyable. Au moment même où l'on voit les *pratensis* se jeter les unes contre les autres en se frappant de quelques coups rapides, puis cesser toute résistance et s'enfuir en masse, on voit aussi les *sanguinea* se jeter tout-à-coup au milieu d'elles sans la plus petite retenue, mordant à droite et à gauche comme des *Polyergus*, et arrachant les cocons de toutes les *pratensis* qui en portent."

He is of opinion (p. 364) that the different species differ much in their power of communicating with one another. Thus, though *Polyergus rufescens* is smaller than *F. sanguinea*, it is generally victorious, because the ants of this species understand one another more quickly than those of *F. sanguinea*.

It appeared to me that the following experiment might throw some light on the power of communication possessed by ants, viz. to place several small quantities of honey in similar situations, then to bring an ant to one of them, and subsequently to register the number of ants visiting each of the parcels of honey, of course imprisoning for the time every ant which found her way to the honey except the first. If, then, many more came to the honey which had been shown to the first ant than to the other parcels, this would be in favour of their possessing the power of communicating facts to one another, though it might be said they came by scent. Accordingly on the 13th July, at 3 P.M., I took a piece of cork about 8 inches long and 4 inches wide, and stuck into it seventeen pins, on three of which I put pieces of card with a little honey. Up

to 5.15 no ant had been up any of these pins. I then put an ant to the honey on one of the bits of card. She seemed to enjoy it, and fed for about five minutes, when she went away. At 5.30 she returned, but went up six pins which had no honey on them. I then put her on to the card. In the mean time twelve other ants have been up wrong pins and two up to the honey; these I imprisoned for the afternoon. At 5.46 my ant went away. From that time to 6 o'clock seven ants came, but not the first. One of the seven went up a wrong pin, but seemed surprised, came down and immediately went up the right one. The other six went straight up the right pin to the honey. Up to 7 o'clock twelve more ants went up pins—eight right, and four wrong. At 7 two more went wrong. Then my first ant returned, bringing three friends with her; and they all went straight to the honey. At 7.11 she went: on her way to the nest she met and spoke to two ants, both of which then came straight to the right pin and up it to the honey. Up to 7.20 seven more ants came and climbed up pins—six right, and one wrong. At 7.22 my first ant came back with five friends; at 7.30 she went away again, returning at 7.45 with no less than twenty companions. During this experiment I imprisoned every ant that found her way up to the honey. Thus, while there were seventeen pins, and consequently sixteen chances to one, yet between 5.45 and 7.45 twenty-seven ants came, not counting those which were brought by the original ant; and out of these twenty-seven, nineteen went up the right pin. Again, on the 15th July, at 2.30, I put out the same piece of cork with ten pins, each with a piece of card and one with honey. At 4.40 I put an ant to the honey; she fed comfortably, and went away at 4.44.

At 4.45 she returned, and at 5. 5 went away again.
„ 5.40 „ „ 5.55 „
„ 6.13 „ and again at 6.25 and 6.59.

There were a good many other ants about, which, up to this time, went up the pins indiscriminately.

At 7.15 an ant came and went up the right pin, and another at 7.18. At 7.26 the first ant came back with a friend, and both went up the right pin. At 7.28 another came straight to the honey.

At 7.30 one went up a wrong pin.
„ 7.31 one came to the right pin.
„ 7.36 „ „ with the first ant.

At 7.39 one came to the right pin.
„ 7.40 „ „
„ 7.41 „ „
„ 7.43 „ „
„ 7.45 „ „
„ 7.46 „ „
„ „ wrong
„ „ „
„ 7.47 two „ „
„ 7.48 one „ right
„ the first ant came back.
„ 7.49 another came to the right pin.
„ 7.50 „ „ wrong „
„ 7.51 „ „ right „
„ three „ wrong „
„ 7.52 one „ right „
„ 7.55 „ „ wrong „
„ „ „ right „
„ 7.57 „ „ wrong „
„ 7.58 „ „ right „
„ 7.59 „ „ wrong „

Thus after 7 o'clock twenty-nine ants came; and though there were ten pins, seventeen of them went straight to the right pin.

On the 16th July I did the same again. At 6.25 I put an ant to the honey; at 6.47 she went.

At 6.49 an ant came to the right pin.
„ 6.50 another „ „
„ 6.55 „ „ „
„ 6.56 „ „ wrong pin, and then to the right one.
„ 6.58 „ „ right pin.
„ 7 „ „ „
„ 7. 5 the first ant came back, and remained at the honey till 7.11.
„ another came to the right pin; but she was with the first.
„ 7. 6 another ant came to the right pin.
„ 7. 6 „ „
„ 7.12 „ „
„ 7.13 „ „

These two ants were met by the first one, which crossed antennæ with them, when they came straight to the honey.

At 7.14 another ant came straight to the honey.

At 7.21 the first ant returned; at 7.26 she left.
„ 7.24 another ant came, but went to a wrong pin, and then went on to the right one.
„ an ant came to wrong pin.
„ „ „ „
„ „ „ „
„ 7.34 „ „ „
„ 7.35 „ „ „
„ 7.38 the first came back, at 7.45 went away again.
„ 7.42 an ant went to a wrong pin.
„ 7.47 „ „ „
„ 7.48 „ „ „
„ 7.49 „ „ „
„ 7.52 „ the right pin.
„ 7.55 the first ant returned, and at 7.56 went away again.
„ 7.57 an ant went to wrong pin.
„ 7.58 „ right „
„ 8 „ wrong „
„ „ right „
„ 8.1 „ wrong „

After this, for an hour no more ants came. On this occasion, therefore, while there were ten pins, out of thirty ants, sixteen came to the right one, while fourteen went to one or other of the nine wrong ones.

July 18. I put out the boards as before at 4 o'clock. Up to 4.25 no ant came. I then put one (No. 1) to the honey; she fed for a few minutes, and went away at 4.31.

At 4.35 she came back with four friends, and went nearly straight to the honey. At 4.42 she went away, but came back almost directly, fed, and went away again.

At 4.57 she returned, and at 5.8 went away again.
„ 4.45 an ant came to wrong pin.
„ 4.47 „ „
„ 4.49 „ „
„ 4.50 „ right pin.
„ 4.52 „ „
„ 4.55 „ wrong pin.
„ 4.56 „ right pin. This ant (No. 2) I allowed to return to the nest, which she did at 5.23.
„ 5. 6 „ right pin.

At 5.11 an ant came to wrong pin.
„ 5.12 „ right pin.
I changed the pin.
„ 5.16 an ant came to the pin which I had put in the same place.
„ „ right pin.
„ 5.19 „ „
„ 5.20 two ants „ with No. 2.
„ ant No. 1 „ and went at 5.25.
„ 5.25 an ant „ : this ant had been spoken to by No. 2.
„ 5.26 another ant „
„ 5.35 „ „
„ 5.37 „ „
„ 5.40 „ „
„ 5.41 ant No. 1 „ and went at 5.49.
„ 5.45 another ant „
„ 5.50 „ „
„ 5.51 ant No. 1 came back, and 5.54 went.
„ 5.58 two ants came to the right pin.
„ 5.59 another ant „ „
„ „ „ a wrong pin.
I changed the pin again.
„ 6.49 an ant came to the pin which I had put in the same place.
„ 7. 1 another ant came to the right pin.
„ 7.20 „ „
„ 7.33 „ „
„ 7.46 ant No. 1 returned, 7.55 went.

Thus during this time, from 4.50 until 7.50, twenty-nine ants came, twenty-six went to the right pin, while only three went up any of the nine wrong ones. Moreover, out of these twenty-six, only four were distinctly brought by the two ants which I had shown the honey.

On the 19th I tried a similar experiment. The marked ants frequently brought friends with them; but, without counting these, from 3.20 to 8 o'clock, out of forty-five ants, twenty-nine went up the right pin, while sixteen went up the nine wrong ones.

Thus on

July 13, out of 27 ants, 19 went right and 8 wrong.
„ 15, „ 29 „ 17 „ 12 „

July 16, out of 30 ants, 16 went right and 14 wrong.
„ 18, „ 26 „ 23 „ 3 „
„ 19, „ 45 „ 29 „ 16 „

Or adding them all together, while there were ten pins at least, out of 156 ants 103 came up the right pin, and only 53 up the others.

It certainly appeared to me that some of the ants were much cleverer in finding their way to the honey than others; several ants which I put on honey came back to nearly the same place, and yet did not seem able to find the exact spot.

Again, some appeared to communicate more freely with their friends than others; and I have met with cases which show that some ants certainly do not, under such circumstances, summon others to their assistance. From this point of view the following observation may be compared with those already recorded. On the 1st August an ant came to the honey at 4.20 and went away a few minutes afterwards.

At 4.36 she returned, and at 4.41 went away again.
„ 4.52 „ 4.58 „
„ 5.11 „ 5.15 „
„ 5.30 „ 5.35 „
„ 6. 5 „ 6.10 „
„ 6.21 „ 6.31 „
„ 6.39 „ 6.43 „
„ 6.55 „ 6.59 „
„ 7.30 „ 7.36 „
„ 7.49 „ 7.54 „

Yet during all this time she brought no friend with her.

The following additional observations were made after the reading of the paper, at the dates severally mentioned below.

Thus on the 3rd Jan. I placed some larvæ in three small porcelain saucers in a box 7 inches square attached to one of my frame-nests. The saucers were in a row 6 inches from the entrance to the frame and $1\frac{1}{2}$ inch apart from one another.

At 1.10 an ant came to the larvæ in the cup which I will call No. 1, took a larva, and returned to the nest.

At 1.24 she returned and took another.

1.45 „ „

2.10 she went to the further saucer, No. 3. I took her up and put her to No. 1. She took a larva and returned.

2.24 she returned to cup No. 3. As there were only two

larvæ in this cup, I left her alone. She took one and returned.

At 2.31 she returned to cup No. 3 and took the last larva.

2.40 she came back to cup No. 3 and searched diligently, went away and wandered about for two minutes, then returned for another look, and at length at 2.50 went to cup No. 1 and took a larva.

3 came to cup 1 and took a larva.

3. 7 „ „

3.15 „ „ , first, however, going and examining cup 3 again.

3.18 came to cup 3, then went to cup 2 and took a larva.

3.30 „ „ 2 „

3.43 „ „ 2 „

5.53 came to cup 3, but did not climb up it, then went to cup 2 and took a larva, which she either dropped or handed over to another ant; for without returning to the nest, at 3.55 she returned to the empty cup, and then to cup 2, where she took the last larva, so that two cups are now empty.

4. 3 she came to cup 3, then to cup 2, and lastly to cup 1, when she took a larva.

4.15 came to cup 1 and took a larva.

4.22 „ „

4.38 „ „

5 came to cup 3, then to cup 2, and lastly to cup 1, when she took a larva.

5.19 came to cup 1 and took a larva.

5.50 came to cup 2 and then to cup 1 and took a larva.

6.20 „ 1 and took the last larva.

I now put about 80 larvæ in cup 3.

It is remarkable that during all this time she did not come straight to the cups, but took a roundabout and apparently irresolute course.

At 7. 4 she came to cup 1 and then to cup 3, and then home.

There were at least a dozen ants exploring in the box; but she did not send any of them to the larvæ.

At 7.30 she returned to cup 3 and took a larva.

I now left off watching for an hour. On my return at

8.30 she was just carrying off a larva.

8.40 she came back to cup 3 and took a larva.

8.55 she came to cup 1 then to cup 3 and took a larva.
9.12 „ „ „ „
9.30 „ 3 „ „
9.52 „ „ „ „
10.14 „ 1 „ „
10.26 she went and examined cup 2, then to cup 3 and took a larva.

At 10.45 she came to cup 3, and I went to bed. At 7 o'clock the next morning the larvæ were all removed. In watching this ant I was much struck by the difficulty she seemed to experience in finding her way. She wandered about at times most irresolutely, and, instead of coming straight across from the door of the frame to the cups, kept along the side of the box; so that in coming to cup 3 she went twice as far as she need have done. Again, it is remarkable that she should have kept on visiting the empty cups time after time. I watched for this ant carefully on the following day; but she did not come out at all.

During the time she was under observation, from 1 till 10.45, though there were always ants roaming about, few climbed up the walls of the cups. Five found their way into the (empty) cup 1 and one only to cup 3. It is clear, therefore, that the ant under observation did not communicate her discovery of larvæ to her friends.

The following day I watched again, having, at 7 A.M., put larvæ into one of the porcelain cups arranged as before. No ants found them for several hours.

At 11.37 one came and took a larva.
„ 11.50 she returned and took a larva.
„ 11.59 she returned „
„ 12. 9 „ „
„ 12.16 „ „
„ 12.21 „ „
„ 12.26 „ „
„ 12.32 „ „
„ 12.37 „ „
„ 12.41 „ „
„ 12.45 „ „
„ 12.50 „ „
„ 12.57 „ „
„ 1. 5 „ „
„ 1.11 „ „

At 1.21 she returned and took a larva.
" 1.35 " "
" 1.40 " "
" 1.44 " "
" 1.52 " "
" 1.56 " "
" 2. 2 " "
" 2.10 " "
" 2.17 " "
" 2.24 " "
" 2.30 " "
" 2.36 " "
" 2.43 " "
" 2.48 " "
" 2.54 " "
" 2.59 " "
" 3. 3 " "
" 3.10 " "
" 3.14 " "
" 3.19 " "
" 3.34 " "
" 3.39 " "
" 3.47 " ,
" 3.56 " "
" 4. 7 " ,
" 4.13 " "
" 4.20 " "
" 4.28 " "
" 4.39 " "
" 4.44 " "
" 4.50 " "
" 4.55 " "
" 5. 1 " "
" 5. 7 " "
" 5.17 " "
" 5.23 " "
" 5.28 " "
" 5.40 " "
" 5.45 " "
" 5.59 " "
" 6. 9 " "
" 6.13 " "

At 6.35 she returned and took a larva.

,,	6.40	,,	,,
,,	6.46	,,	,,
,,	6.51	,,	,,
,,	6.58	,,	,,
,,	7. 2	,,	,,
,,	7. 8	,,	,,
,,	7.12	,,	,,
,,	7.16	,,	,,
,,	7.21	,,	,,
,,	7.26	,,	,,
,,	7.39	,,	,,
,,	7.44	,,	,,
,,	7.53	,,	,,
,,	7.57	,,	,,
,,	8. 3	,,	,,
,,	8. 8	,,	,,
,,	8.13	,,	,,
,,	8.20	,,	,,
,,	8.26	,,	,,
,,	8.31	,,	,,
,,	8.38	,,	,,
,,	8.45	,,	,,
,,	8.50	,,	,,
,,	8.55	,,	,,
,,	9. 2	,,	,,
,,	9.11	,,	,,
,,	9.19	,,	,,
,,	9.25	,,	,,
,,	9.33	,,	,,
,,	9.40	,,	,,
,,	9.46	,,	,,
,,	9.52	,,	,,

This is an unusually long interval; still I am sure the time is correct.

,,	10.32	,,	,,
,,	10.39	,,	,,
,,	10.49	,,	,,
,,	10.54	,,	,,
,,	11. 1	,,	,,

At this time I went to bed. There were still about twenty-five larvæ in the cup, which had all been removed when I looked at 6.15 the next morning. During the whole time she was under observation, only two strange ants found their way to the cup, though there were some wandering about in the box all day. Towards evening, however, they went into the nest, and for some hours my ant was the only one out. It will be observed that she returned at shorter intervals than the previous ones. This was partly because she had a shorter distance to go, and partly because she was not bewildered by three cups, like the preceding. I had placed a bit of wood to facilitate her ascent into the cup. This she made use of, but instead of going the shortest way to the cup, she followed the side of the box, partly, perhaps, because the floor was covered with a plate of porcelain. This, however, would not account for the fact that at first she invariably went beyond the cup, and even past the second cup; gradually, however, this circuit became smaller and smaller; but to the last she went round the outside of cup 1 instead of going straight to the spot where I had placed the bit of wood.

On the 9th January again I watched her under similar circumstances. From 9.35 to 1.40 she made 55 journeys to and fro, carrying off a larva each time; but during this period only one strange ant found the larvæ.

In the afternoon of the same day I watched the ant which had been under observation on the 3rd Jan. From 3.27 to 9.30 she made forty-two visits, during which time only four strange ants came to the larvæ.

On the 10th Jan. I watched the same ant as on the 4th. Between 11 A.M. and 10 P.M. she made no less than ninety-two visits; and during the whole time only one strange ant came to the larvæ.

On the 18th Jan. I put out some more larvæ in the small porcelain cups. Between 8 and 9 both these ants found them, and kept on coming all day up to 7 P.M., when I left off observing. There were a good many ants wandering about in the box; but up to 4 o'clock only four came to the larvæ. Two of them I imprisoned as usual; but two (which came at 4.30 and 4.36) I marked. These went on working quietly with the first two till I left off observing at 7 P.M.; and during this latter time only three other ants found the larvæ.

On the 31st Jan. I watched another specimen. At 9.14 I put

her into a small cup containing a number of larvæ. She worked continuously till half-past seven in the evening, when I left off watching. During that time she had made more than ninety journeys, carrying each time a larva to the nest. During the whole time not a single other ant came to the larvæ.

Again on the 7th Feb. I watched two ants in the same manner. At 7 A.M. I put some larvæ in the small china cups. Up to 8 no ants had come to them. Soon after 8 I put two marked ants, neither of them being the same as these whose movements are above recorded. They were then watched until a quarter to eight in the evening, during which time one of them had made twenty-six journeys, carrying off a larva each time; the other forty-two. During this period of about eleven hours, two strange ants had come to the cup at which these were working, and the same number to one of the other cups.

None of these ants, therefore, though they had found a large number of larvæ, more than they could carry in a whole day, summoned any other to their assistance.

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Observations on Ants, Bees, and Wasps.—Part III.

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

Ants.

In my second paper on this subject I gave some cases which show that if ants find stores of food, they do not by any means in all instances bring friends to assist in securing the treasure.

Experiments with Larvæ.

Again, Feb. 7, I put some larvæ in three porcelain cups in the feeding-box of a frame containing a nest of *Formica flava*, about 6 inches from the entrance of the frame, and put at 8 and 8.29 A.M. respectively two ants to the larvæ in the left-hand cup. They each carried off a larva and returned as follows:—

No. 1.	No. 2.		
At 8.35	....	returned again and took another.	
9. 0	....	,,	,,
	9. 7	,,	,,
	9.20	,,	,,
9.30	....	,,	,,
	9.43	,,	,,
9.54	....	,,	,,
	9.56	,,	,,
	10.20	,,	,,
10.25	....	,,	,,

At 10.43 a strange ant came to the larvæ in the right-hand cup. I imprisoned her.

At	11. 0	returned again and took another.	
11. 1	....	,,	,,
	11. 9	,,	,,
11.15	....	,,	,,
	11.20	,,	,,
	11.29	,,	,,
11.37	....	,,	,,
	11.40	,,	,,
	11.52	,,	,,

At 12.2 a stranger came to the larvæ in the left-hand cup. I imprisoned her.

No. 1.	No. 2.		
At 12. 3	...	returned again and took another.	
	12.15	„	„
	12.30	„	„
12.37	....	„	„
	12.41	„	„
	12.50	„	„
	15.58	„	„
1. 0	....	„	„
	1. 7	„	„
1.12	....	„	„
	1.16	„	„
	1.28	„	„
1.32	...	„	„
	1.35	„	„
	1.44	„	„
1.50	....	„	„
	1.55	„	„
	2. 6	„	„
2. 9	..	„	„
	2.17	„	„
	2.29	„	„
2.39	..	„	„
	2.42	„	„
2.49	2.49	„	„
3. 0	....	„	„
	3. 3	„	„

At 3.10 a stranger came to the left-hand cup. I imprisoned her.

At	3.14	returned again and took another.	
3.15	....	„	„
	3.24	„	„
3.31	....	„	„
	3.34	„	„
3.36	....	„	„

At 4.10 a stranger came to the middle cup. I imprisoned her.

At 4.45	.. .	returned again and took another.	
	5.50	„	„
6. 2	6. 2	„	„
	6.17	„	„

No. 1.	No. 2.		
At	6.26	returned again and	took another.
	6.46	„	„
	6.52	„	„
7. 4	....	„	„
	7. 7	„	„
	7.13	„	„
	7.18	„	„
7.48	7.48	„	„

After this they were not watched any more. It will be observed that the second ant made many more visits than the first—namely, forty-two in about eleven hours, as against twenty-six in eleven hours and a half. During this time two strangers came to the larvæ in the cup they were visiting, and three to the other two cups.

The following case is still more striking. On July 11, at 11 A.M., I put a *F. flava* to some pupæ of the same species, but from a different nest. She made eighty-six journeys, each time carrying off a pupa, with the following intervals. Commencing

at 11. 0,	At 1.33 again
11. 5 she returned.	1.43 „
11. 9 returned again.	1.49 „
11.16 again	1.52 „
11.20 „	1.56 „
11 24 „	2. 2 „
11.29 „	2.10 „
11.36 „	2.17 „
11.49 „	2.25 „
11.55 „	2.29 „
12. 0 „	2.32 „
12. 5 „	2.35 „
12.16 „	2.37 „
12.30 „	2.40 „
12.40 „	2.43 „
12.44 „	2.47 „
12.50 „	2.53 „
1. 1 „	2.56 „
1.10 „	2.59 „
1.19 „	3. 2 „
1.27 „	3. 7 „

At 3.10 again.	At 4.30 again.
3.13 „	4.33 „
3.16 „	4.40 „
3.20 „	4.43 „
3.25 „	4.45 „
3.33 „	4.49 „
3.35 „	4.53 „
3.38 „	4.55 „
3.40 „	4.58 „
3.47 „	5. 3 „
3.53 „	5. 7 „
3.57 „	5.12 „
4. 0 „	5.19 „
4. 3 „	5.22 „
4. 5 „	5.25 „
4. 8 „	5.28 „
4.12 „	5.32 „
4.15 „	5.35 „
4.18 „	5.39 „
4.20 „	5.50 „
4.23 „	7. 5 „
4.26 „	7.12 „

After which she did not come again till 8, when we left off watching. During the whole of this time she did not bring a single ant to help her. Surely it would have been in many respects desirable to do so. It will be seen that some of the pupæ remained lying about and exposed to many dangers from 11 A.M. till 7 P.M.; and when she left off working at that time, there were still a number of the pupæ unsecured; and yet, though she had taken so much pains herself, she did not bring or send others to assist her in her efforts or to complete her work.

Experiments with Pupæ.

July 11. I had put out some pupæ of *F. flava* in the central park. At 5.55 a *F. fusca* found them and carried one off.

At 6. 0	she returned and took another.	Again
6. 1	„	„
6. 3	„	„
6. 4	„	„
6. 5	„	„

At 6. 6 she returned and took another. Again

6. 7	,,	,,
6. 8	,,	,,
6. 9	,,	,,
6.10	,,	,,
6.11	,,	,,
6.12	,,	,,
6.14	,,	,,
6.15	,,	,,
6.16	,,	,,
6.17	,,	,,
6.19	,,	,,
6.20	,,	,,
6.21	,,	,,
6.23	,,	,,
6.25	,,	,,
6.27	,,	,,
6.29	,,	,,
6.30	,,	,,
6.31	,,	,,
6.33	,,	,,
6.35	,,	,,
6.36	,,	,,
6.37	,,	,,
6.38	,,	,,
6.40	,,	,,
6.41	,,	,,
6.45	,,	,,
6.47	,,	,,
6.49	,,	,,
6.50	,,	,,
6.51	,,	,,
6.52	,,	,,
6.53	,,	,,
6.55	,,	,,
6.56	,,	,,
6.57	,,	,,
7. 0	,,	,,
7. 1	,,	,,
7. 2	,,	,,
7. 6	,,	,,

After the 45 visits, she came no more till 8; but when I returned at 10 I found all the pupæ gone. During the time she was watched, however, she brought no other ant to assist.

Experiments with Larvæ.

I also made similar experiments with *Myrmica ruginodis*, imprisoning (as before) all ants that came except the marked ones.

Sept. 24. I put out two lots of larvæ; and to one of them I placed two specimens, which I will call 1 and 2. They returned as follows, carrying off a larva on each journey:—

No. 1.	No. 2.
10.23	
	10.26
10.28	
	10.32
10.34	
	10.37
10.40	
	10.41 bringing a friend.
10.50	
	10.55
	11. 6
	11.16
11.40	
	11.44
11.45	
	11.46 a stranger came alone.
	11.56
12. 0	
	12. 6 bringing a friend.
12.11	
	12.15
12.16	
	12.17 a stranger came alone.
12.22	12.22 " "
12.29	
12.34	
12.36	
	12.40
	12.45 a stranger found the
12.47	second lot of larvæ.
12.53	

No. 1.	No. 2.	
		12.58 two strangers found
	12.59	the second lot of larvæ.
	1. 5	
1. 6		
		1. 7 a stranger found the
	1.16	second lot of larvæ.
1.20		
	1.21	
	1.26	
	1.35	
1.42		
	1.47	
	1.54	
1.55 with 2 friends.		
	1.59	
2. 2		
		2. 3 a stranger found the
	2. 4	larvæ.
2. 9 with a friend.		
	2.10	
2.16		
	2.18	
2.24		
	2.25	2.25 a stranger found the
	2.34	second lot of larvæ.
2.36		
	2.41	
2.44		
	2.45	
	2.50	
2.51		
	2.55	
	3. 0	
3. 1		
	3. 6	
3.10	3.10	
	3.17	
3.18		
	3.22	
	3.27	

No. 1.	No. 2.
3.28	
	3.36
3.40	
	3.47
3.48	
	3.53
3.55	
	3.59
4. 0	
	4. 7
4. 8	
	4.14
4.16	
	4.20
4.27	
	4.31
4.35	
	4.39 with a friend.
4.42	4.42
	4.47
4.53	4.53
	4.58
	5. 3
5. 5	
	5. 9
5.17	5.17
5.25	
5.32	
5.40	
5.46	
5.55	
6. 5	
	6. 8
6.11	
	6.16
6.20	

They came no more up to 7.30, when we left off watching. The following morning at 6.5 I found No. 1 wandering about, and evidently on the look-out. I put her to some larvæ; and shortly afterwards No. 2 also found them. Their visits were as follows:—

No. 1.	No. 2.	
6.10		
6.21		
6.36		
	6.42	
6.44		
6.52		
7. 1	7. 1	
	7. 8	
7.11		
	7.12	
	7.22	
	7.29	
		7.30 a strange ant
	7.35	found the larvæ.
7.40		
7.49		
	7.54	
8. 5		
8.13		
8.25		
8.31		
8.39		
8.44		
8.48		

Thus, during this period these two ants carried off respectively 62 and 67 larvæ; 10 strangers found the larvæ, half of them exactly coming to the lot visited by the ants under observation.

Again. Sept. 27, at 3.55 P.M., I put a *F. nigra* to some larvæ of *F. flava*. She returned as follows :—

4. 3	4.52
4.11	4.56
4.21	5
4.25	5. 5
4.28	5.10
4.31	5.14
4.37	5.18
4.40	5.23
4.44	5.29
4.48	5.40

5.43	5.54
5.46	5.59
5.50	

when she met with an accident. During this time no other ant came to the larvæ.

On Oct. 1, at 6.15 A.M., I put three specimens of *F. nigra* to some larvæ of *F. flava*. One did not return; the other two behaved as follows:—

No. 1 returned to the larvæ at	No. 2 at	Strangers came at
6.52		
	7.12	
		7.14 to lot 2.
	7.22	
7.30		
	7.32	
7.42	7.42	
		7.45 to lot 3.
	7.50	
7.54		
	8. 0	
8. 1		
8. 6 with a friend.	8. 6	
	8. 9	
8.10		
8.17		
		8.19 to lot 1.
		8.23 ,,
8.25		
	8.26	
8.32		
8.36		
		8.37 ,,
	8.38	
8.39		
	8.41	
8.44		
		8.45 ,,

Here I left off watching for half an hour.

9.22	
	9.28

No. 1 returned to the larvæ at	No. 2 at	Strangers came at
9.29		
9.35	9.35	
9.41		
	9.45	
9.47		
9.50		
	9.52	
9.54 with a friend.		
9.57		
		9.58 to lot 1.
	10. 0	
10. 1		
10. 9		
	10.11	
10.13 with a friend.		
10.16	10.16	
	10.25	
	10.30	
	10.36	
	10.46	
	10.50	
10.55		
	10.58	
11. 0		
	11. 2	
11. 3		
11. 7		
	11. 8	
	11.15	
11.16		
11.19	11.19	
11.23		
	11.25	
11.27		
	11.29 with a friend.	
		11.30
11.33		
	11.35	
11.37		
11.41		

No. 1 returned to the larvæ at	No. 2 at	Strangers came at
	11.42	
11.45		11.47 to lot 1.
	11.48	
11.49		
11.53		
	11.59	
12. 1		
12. 4		
12. 8		
	12. 9	
12.11		
		12.14 ,,
12.15	12.15	
12.18		
		12.19 ,,
	12.20	
12.21		
12.25		
	12.29 with a friend.	
12.30		
12.35		
	12.36	
12.39		
12.42		
	12.43	
12.45		
	12.47	
12.48		
12.51		
	12.53	
12.54		
		12.56 ,,
12.57	12.57	
1. 0 with friend.	1. 0	
1. 2		
1. 5		
1. 7		
	1. 9	
1.10		
		1.11 ,

No. 1 returned to the larvæ a	No. 2 at	Strangers came at
1.13		
	1.14	
1.15		
1.18	1.18	
1.21		
1.24		
1.27		1.27
	1.28	
1.30		
1.33		
	1.35	
1.36		
1.39		
1.42	1.42	
1.45		
		1.46
1.48	1.48	
1.51		
	1.53	
1.57		
	1.59	
2. 1		
2. 4		
	2.15	
2.17		
2.21		
	2.22	
2.25		
2.29		
	2.31	
2.33		
2.37		
	2.39	
2.40		
	2.43	
2.44		
2.47		
	2.49	
2.50		
2.54		

No. 1 returned to the larvæ at	No. 2 at	Strangers came at
2.57		
3. 0		
	3.4 with a friend	
3. 6		
3. 9 with a friend.		
3.12		
3.14		
3.16	3.16	
3.20		
	3.21	
3.23		
3.26	3.26	
3.30	3.30	
3.33	3.33	
3.35	3.35	
3.37		
	3.38	
3.39		
3.41		
3.43		
	3.45	
3.46		
	3.48	
3.49		
3.54		
4. 0		
4. 3		
	4. 4	
4. 7		
4.12		
4.15		
4.20		
4.26		
4.29		
4.31		
		4.32
4.34		
4.36		
4.39		

No. 1 returned to the larvæ at	No. 2 at	Strangers came at
4.42		
		4.43
	4.44	
4.45		
4.49	4.49	
	4.55	
4.56		
	4.58	
4.59		
5. 2	5. 2	
	5. 6 with two friends, after which she came no more.	
5. 7		
5.10		
5.13		
5.15		
5.18		
5.21		
5.25		
5.28		
5.31		
		5.33 to lot 2.
5.35	No. 1 returned to the larvæ at	
5.38	7.14	
5.41	7.18	
5.45	7.21	
5.51	7.24	
5.54	7.25	
6. 0	7.28	
6. 4	7.31	
6. 7	7.34	
6.14	7.38	
6.17	7.41	
6.20	7.44	
6.28	7.47	
6.31	7.51	
6.48	7.55	
6.54	7.59	
7. 0	8. 2	
7. 3	8. 5	
7. 6	8.12	
7.11	8.15	

No. 1 returned to the larvæ at	No. 1 returned to the larvæ at
8.18	8.38
8.20	8.42
8.24	8.44 stranger
8.28	8.45 came.
8.32	9.44
8.35	

We continued to watch till 10.15, but she came no more. She had, however, in the day carried off to the nest no less than 187 larvæ. She brought 5 friends with her; less than 20 other ants came to the larvæ.

October 3. I put a *F. nigra* to some larvæ of *F. flava*. She returned as follows, viz.:—

1.42	3.35
1.48	3.38
1.52	3.41
2. 0	3.49 with a friend.
2. 4	3.51
2. 8	3.54
2.12 with a stranger.	3.57
2.15	4. 1
2.19	4. 4
2.24	4. 7
2.27	4.10
2.32	4.12
2.36	4.15
2.40	4.18
2.44	4.22
2.49	4.25
2.57	4.29
3. 1	4.32
3. 4	4.35
3. 7	4.38
3.10	4.43
3.13	4.46
3.15	4.49
3.18	4.54
3.20	4.57
3.23	5. 0
3.31	5. 3

5. 6	5.22
5.10	5.26
5.14	5.29
5.18	She dropped on the floor; I picked her up; and she returned at
6.40	7. 7 with 3 friends.
6.50	7.11. She now fell into some water.
6.54	
7. 4	

Experiments with Honey.

In addition to the above experiments with larvæ, I tried the following with syrup.

April 19. I put out a little syrup on eleven slips of glass, which I placed on eleven inverted flower-pots on the lawn. At 8.35 a *F. nigra* found the honey on one of the flower-pots.

8.50 she returned to the honey, and at 9. 5 went back to the nest.

9.21	,,	,,	9.30	,,
9.42	,,	,,	9.50	,,
10.12	,,	,,	10.21	,,
10.35	,,	,,	10.46	,,
11. 9	,,	,,	11.20	,,
11.45	,,	,,	11.50	,,
11.57	,,	,,	12. 2	,,
12·20	,,	,,	12.30	,,
12.45	,,	,,	12.53	,,
1. 8	,,	,,	1.18	,,
1.34	,,	,,	1.43	,,
1.57	,,	,,	2. 7	,,
2.28	,,	,,	2.33	,,
2.49	,,	,,	2.53	,,
2.59	,,	,,	3. 2	,,
3. 9	,,	,,	3.11	,,
3.29	,,	,,	3.30	,,
3.59	,,	,,	4. 8	,,

After which we watched till 6 P.M.; but she did not return again to the honey. During the above time 8 ants came to the same honey, and 21 to the other ten deposits.

On July 11 I put one of my specimens of *F. nigra* to some honey at 7.10. She fed till 7.25, when she returned to the nest

At 7.32 she returned.		At 7.36 another ant	came,	whom I im-
7.47	„	7.50	„	„ [prisoned.
8. 0	„	8.11	„	„
8.18	„			
8.36	„			
8.59	„			
9.17	„			
9.38	„			
9.53	„			
10.10	„			
10.27	„			
10.44	„			
11. 6	„			
11.16	„			
11.38	„			
12. 0	„			
12.36	„	12.45	„	„
12.56	„			
1.21	„			
1.44	„			
2.10	„			
2.21	„			
2.29	„			
2.50	„	2.51	„	„
3. 5	„			

After this she did not come back any more up to 8 P.M.

April 25 was a beautiful day. At 9 A.M. I put some syrup in the same way on five inverted flower-pots, and at

9.10 put an ant to one of the deposits of syrup. At
9.34 another ant came to the same syrup. This one I will call No. 2. At
9.40 No. 1 returned.
10.45 No. 2 „ At 11 one came to the same honey; this I will call No. 3.
11. 7 No. 1 „ but did not come back any more.
12.31 No. 2 „ and at 12.47 went.
1.15 No. 3 „ „ 1.25 „
1.22 No. 2 „ „ 1.48 „
1.54 No. 3 „ „ 2. 3 „
2.18 No. 2 „ „ 2.30 „

2.35 No. 3 returned, and at 2.36 went.
2.56 No. 2 „ „ 3. 1 „
3.24 No. 2 returned.
4.19 No. 2 „

After which I went on watching till 7, but none of these three returned. During the day 7 ants came to this honey, and 27 to the other four deposits. Here, therefore, it is evident that the three watched ants did not communicate, at any rate, any exact information to their friends.

June 27. I placed four inverted glasses (tumblers) on the grass, and on the top of each placed a little honey. I then, at 8 o'clock, put two ants, belonging to *F. nigra*, to the honey on one of the glasses.

At 8.25 No. 1 came back, and at 8.45 she returned to the nest, but did not come to the honey any more.

At 9.5 No. 2 came out and wandered about; I put her to the honey again; she fed and at 9.22 returned to the nest.

At 9.28 she returned to the honey, and at 9.45 went back to the nest.

10.42	„	„	10.50	„
10.58	„	„	11.10	„
11.21	„	„	11.39	„
12.45	„	„	12.59	„
1.40	„	„		

I continued to watch till 7 P.M., but neither of them returned any more.

Aug. 7. I put out four small deposits of honey (which I continually renewed) on slips of glass placed on square bricks of wood and put an ant (*F. nigra*) to one of them at 9.20. She fed an went away.

At 9.35 she returned, and fed till 9.43			
10.14	„	„	10.17
10.25	„	„	10.27
10.37	„	„	10.40
This time a friend came with her.			
At 10.47 she returned, and fed till 10.53			
11. 0	„	„	11.14
11.35	„	„	11.40
11.52	„	„	11.55
12.13	„	„	12.16
1. 0	„	„	1. 5

At 1.15	she returned,	and fed till	1.18	
1.26	„	„	1.29	
1.45	„	„	1.48	
1.58	„	„	2. 1	
2. 9	„	„	2.14	
2.20	„	„	2.21	She was dis-
2.25	„	„	2.30	[turbed.
2.37	„	„	2.40	
3. 2	„	„	3. 8	
3.16	„	„	3.20	
3.39	„	„	3.41	
3.58	„	„	4. 2	
4.13	„	„	4.20	
4.29	„	„	4.36	

At this time there was a shower of rain, so I removed the honey for half an hour.

At 5. 2	she returned,	and fed till	5.10
5.20	„	„	5.25
5.33	„	„	5.37
5.42	„	„	5.45
5.50	„	„	5.52
5.58	„	„	6. 6
6.15	„	„	6.18
6.21	„	„	6.23
6.25	„	„	6.27
6.32	„	„	6.35
6.40	„	„	6.44
6.49	„	„	6.53
7.15	„	„	7.20
7.25	„	„	7.27
7.30	„	„	7.33
7.36	„	„	7.37

During the whole of this time only three other ants came to the honey.

Aug. 13. At 11 A.M. I placed a *F. fusca* from one of my nests, which I had kept for some days without food, to some honey; she fed for some minutes, leaving at 11.6.

At 11.14	she returned,	leaving at	11.20
11.30	„	„	11.35
11.40	„	„	11.45

At 11.55	she returned,	leaving at	11.58
12. 7	,,	,,	12.11
12.18	,,	,,	12.21
12.28	,,	,,	12.31
12.38	,,	,,	12.41
12.47	,,	,,	12.51
12.56	,,	,,	12.59
1. 9	,,	,,	1.15
1.24	,,	,,	1.27
1.32	,,	,,	1.35
1.46	,,	,,	1.52
1.59	,,	,,	2. 3
2.12	,,	,,	2.15
2.26	,,	,,	2.30
2.38	,,	,,	2.43
2.55	,,	,,	3. 2
3.17	,,	,,	3.24
3.35	,,	,,	3.43
3.55	,,	,,	4. 0
4.13	,,	,,	4.17
4.35	,,	,,	4.51
5.15	,,	,,	5.26
6.29	,,	,,	6.45

I continued to watch till 8, but she came no more. During the whole time no other ant came to the honey; indeed very few left the nest at all. I kept my eye on this ant for some days, and she visited the honey every now and then, while very few others came to it.

As to Power of Communication.

With reference to the cases above recorded, in which, when ants had discovered a store of food or larvæ, others also found their way to it, I was anxious to ascertain in what manner this was effected. Some have regarded the fact as a proof of the power of communication; others, on the contrary, have denied that it indicated any such power. Ants, they said, being social animals, naturally accompany one another; moreover, seeing a companion coming home time after time with a larva, they would naturally conclude that they also would find larvæ in the same spot. It seemed to me that it would be very interesting to determine whether the ants in question were brought to the larvæ, or whether they came casually. To solve this question, I tried the following

experiments during the latter days of October. I took three tapes, each about 2 feet 6 inches long, and arranged them parallel to one another and about 6 inches apart. One end of each I attached to one of my nests (*F. nigra*), and at the other end I placed a glass. In the glass at the end of one tape I placed a considerable number (300 to 600) of larvæ. In the second I put two or three larvæ only; in the third none at all. The object of the last was to see whether many ants would come to the glasses under such circumstances by mere accident; and I may at once say that scarcely any did so. I then took two ants and placed one of them to the glass with many larvæ, the other to that with two or three. Each of them took a larva and carried it to the nest, returning for another, and so on. After each journey I put another larva in the glass with only two or three larvæ to replace that which had been removed. Now, if other ants came under the above circumstances as a mere matter of accident, or accompanying one another by chance, or if they simply saw the larvæ which were being brought and consequently concluded that they might themselves also find larvæ in the same place, then the numbers going to the two glasses ought to be approximately equal. In each case the number of journeys made by the ants would be nearly the same; consequently, if it was a matter of scent, the two glasses would be in the same position. It would be impossible for an ant, seeing another in the act of bringing a larva, to judge for itself whether there were few or many larvæ left behind. On the other hand, if the strangers were brought, then it would be curious to see whether more were brought to the glass with many larvæ, than to that which only contained two or three. I should also mention that every stranger was imprisoned until the end of the experiment. The results were as follows:—

Exp. 1.—Time occupied, 1 hour. The ant with few larvæ made 6 visits and brought no friends. The one with many larvæ made 7, and brought 11 friends.

Exp. 2.—Time occupied, 2 hours. The ant with few larvæ made 13 journeys, and brought 8 friends. The one with many larvæ did not come back.

Exp. 3.—Time occupied, 3 hours. The ant with few larvæ made 24 journeys, and brought 5 friends. The one with many larvæ made 38 journeys, and brought 22 friends.

Exp. 4.—Time occupied, $2\frac{1}{2}$ hours. The ant with few larvæ did

not come back. The one with many made 32 journeys, and brought 19 friends.

Exp. 5.—Time occupied, 1 hour. The ant with few larvæ made 10 journeys, and brought 3 friends. The other made 5 journeys and brought 16 friends.

Exp. 6.—Time occupied, 1½ hour. The ant with few larvæ made 15 journeys, but brought no friends. The other made 11 journeys and brought 21 friends.

Exp. 7.—I now the reversed the glasses. Time occupied 3 hours. The ant with few larvæ made 23 journeys and brought 4 friends.

Exp. 8.—Time occupied, 1½ hour. The ant with few larvæ made 7 journeys and brought 3 friends. The one with many larvæ made 19 journeys and brought 6 friends.

Exp. 9.—Time occupied, 1 hour. The ant with few larvæ made 11 journeys and brought 1 friend. The one with many larvæ made 15 journeys and brought 13 friends.

Exp. 10.—I now reversed the glasses, the same two ants being under observation; but the ant which in the previous observation had few larvæ, now consequently had many, and *vice versâ*. Time occupied 2 hours. The ant with few larvæ made 21 journeys and brought 1 friend. The one with many larvæ made 32 journeys and brought 20 friends. These two experiments are, I think, very striking.

Exp. 11.—Time occupied, 5 hours. The ant with few larvæ made 19 journeys and brought 1 friend. The one with many larvæ made 26 journeys and brought 10 friends.

Exp. 12.—Time occupied, 3 hours. The ant with few larvæ made 20 journeys and brought 4 friends. The one with many larvæ brought no friends and made 17 journeys.

Exp. 13.—Time occupied, 1 hour. The ant with few larvæ made 5 journeys and brought no friends. The one with many made 10 journeys and brought 16 friends.

Exp. 14.—I now reversed the glasses. Time occupied, 2½ hours The ant with few larvæ made 10 journeys and brought 2 friends. The other made 41 journeys and brought 3 friends.

Exp. 15.—Time occupied, 4½ hours. The ant with few larvæ made 40 journeys and brought 10 friends. Of these, 8 came at the beginning of the experiment, and I much doubt whether they were brought; during the last hour and a half she only brought 1 friend. However, I think it fair to record the observation.

The ant with many larvæ made 47 journeys and brought 1 friend.

Exp. 16.—Time, 4½ hours. The ant with few larvæ made 20 journeys and brought 1 friend. She did not return after the first 2 hours. The other ant made 53 journeys and only brought 2 friends. This latter was the same one as in the previous experiment, when, however, she had the glass with only two or three larvæ.

Exp. 17.—Time, 1 hour. The ant with few larvæ made 6 journeys and brought no friend. The one with many larvæ made 11 journeys and brought 12 friends.

Exp. 18.—Time, 1½ hour. The ant with few larvæ made 25 journeys and brought four friends. The one with many larvæ made 20 journeys and brought 15 friends.

Exp. 19.—Time, 4½ hours. The ant with few larvæ made 74 journeys and brought no less than 27 friends. This is quite in opposition to the other observations; and I cannot account for it. She was the ant who brought 15 friends in the previous experiment, and it certainly looks as if some ants were more influential than others. The ant with many larvæ made 71 journeys and only brought 7 friends.

Exp. 20.—Time, 2 hours. The ant with few larvæ made 35 journeys and brought 4 friends. The one with many larvæ made 34 journeys and brought 3 friends.

Exp. 21.—I now transposed the two glasses. Time, 1½ hour. The ant with few larvæ made 15 journeys and brought no friends. The other made 35 journeys and brought 21 friends.

Exp. 22.—I now transposed the glasses again. Time, 2 hours. The ant with many larvæ made 37 journeys and brought 9 friends. The ant with few larvæ made 18 journeys and brought no friend. This, I think, is a very striking case. She was under observation 5½ hours; and the scene of her labour was the same throughout. The first 2 hours she had few larvæ and brought 4 friends; then for 1½ she had many larvæ and brought 21 friends; then again for 2 hours she had few larvæ and brought no friend.

Exp. 23.—Time, 1½ hour. The ant with few larvæ made 25 journeys and brought 3 friends. The other made only 9 journeys, but brought 10 friends.

Exp. 24.—I now transposed the glasses. Time occupied, 2 hours. The ant which now had few larvæ made 14 journeys, but brought no friends. The other made 37 journeys and brought 5 friends.

Exp. 25.—Time 3 hours. I put an ant for an hour to a full glass; she made 10 journeys and brought 4 friends. I then left only two or three larvæ: in the second hour she made 7 journeys and brought no friend. I then again filled the glass; and during the third hour she made 14 journeys and brought 3 friends.

The results of the above experiments are shown at a glance in the following Table.

Tabular View of Experiments on Power of Communication.

Ants.	Glass with many larvæ.			Glass with one or two larvæ.		
	Time observed.	Number of journeys.	Number of strangers.	Time observed.	Number of journeys.	Number of strangers.
	hours.			hours.		
1.	1	7	11			
2.	...	...	...	1	6	0
3.	...	...	...	2	13	8
4.	...	...	...	3	24	5
5.	3	38	22	1	10	3
6.	2½	32	19			
7.	1	5	16			
8.	1½	11	21	3	23	2
9.	...	...	...	1½	7	3
10.	1	15	13	2	21	1
11.	2	32	20	1	11	1
12.	5	26	10			
13.	...	...	...	5	19	1
14.	...	...	...	3	20	4
15.	2½	41	3	2	5	0
16.	1	10	16	2½	10	2
17.	4½	53	2	4½	40	10
18.	...	...	...	2	20	1
19.	1	11	12			
20.	...	...	...	1	6	0
21.	1½	20	15	4½	74	27
22.	...	...	...	1½	25	4
23.	4½	71	7			
24.	...	...	...	2	35	4
25.	2	34	3			
26.	1½	35	21	2	18	0
27.	2	37	9	1½	15	0
28.	1½	9	10	2	14	0
29.	2	37	5	1½	25	3
30.	1½	9	10	2	14	0
31.	2	37	5	1½	25	3
32.	2	24	7	1	7	0

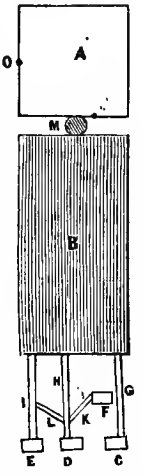
It must be admitted that this mode of observing is calculated to increase the number of frieuds brought by the ants to the glass with only 2 or 3 larvæ, for several reasons, but especially because

in many cases an ant which had for some time had access to a glass with many larvæ was suddenly deprived of it, and it might well be that some time elapsed before the change was discovered. Some stray ants would, no doubt, in any case have found the larvæ; and we ought probably to allow for at least 25 under this head. Again, some would, no doubt, casually accompany their friends: if we allow 25 also in this respect, we must deduct 50 from each side, and we shall have 205 against 37. Nevertheless even without any allowances, the results seem to me very definite. Some of the individual cases, especially perhaps experiments 9 10, 20, 21, and 22, are very striking; and, taken as a whole, during 47½ hours, the ants which had access to a glass containing numerous larvæ brought 257 friends; while during 53 hours those which were visiting a glass with only 2 or 3 larvæ brought only 82 to their assistance.

One case of apparent communication struck me very much. I had had an ant (*F. nigra*) under observation one day, during which she was occupied in carrying off larvæ to her nest. At night I imprisoned her in a small bottle; in the morning I let her out at 6.15, when she immediately resumed her occupation. Having to go to London, I imprisoned her again at 9 o'clock. When I returned at 4.40, I put her again to the larvæ. She examined them carefully, but went home without taking one. At this time no other ants were out of the nest. In less than a minute she came out again with 8 friends, and the little troop made straight for the heap of larvæ. When they had gone two thirds of the way, I again imprisoned the marked ants; the others hesitated a few moments, and then, with curious quickness, returned home. At 5.15 I put her again to the larvæ. She again went home *without a larva*, but, after only a few seconds' stay in the nest, came out with no less than 13 friends. They all went towards the larvæ; but when they got about two thirds of the way, although the marked ant had on the previous day passed over the ground about 150 times, and though she had just gone straight from the larvæ to the nest, she seemed to have forgotten her way and wandered; and after she had wandered about for half an hour, I put her to the larvæ. Now in this case the 21 ants must have been brought out by my marked one; for they came exactly with her, and there were no other ants out. Moreover it would seem that they must have been told, because (which is very curious in itself) she did not in either case bring a larva, and consequently it cannot

have been the mere sight of a larva which had induced them to follow her.

Fig. 1.



It remained to ascertain whether the ants which came by themselves to the larvæ found them by scent, or whether the road had been described to them; for it is obvious that the latter would imply a higher intelligence than the former. In many of the above cases ants came by themselves almost straight to the larvæ which were being visited by my marked ants, while other larvæ close by remained entirely unvisited. The stranger ants must therefore either have had the way described to them, or, having been told of the existence of larvæ, have tracked the marked ant by scent, and so found their way to the larvæ. To determine which, I made the following experiment.

In the above figure A is the ants' nest, *o* the door of the nest. M is the section of a pole on which the whole apparatus is supported. B is a board 2 feet long; C, D, E, and F are slips of glass connected with the board B by narrow strips of paper G, H, I. K is a moveable strip of paper, 1½ inch long, connecting the glass F with the strip H; and L is another moveable strip of paper, as nearly as possible similar, connecting H and I. On each of the slips of glass C and F I put several hundred larvæ of *F. flava*. The object of the larvæ on C was to ascertain whether, under such circumstances, other ants would find the larvæ accidentally; and I may say at once that none did so. I then put the ant (A), whom I had imprisoned over night, to the larvæ on F. She took one, and, knowing her way, went straight home over the bridge K and down the strip H. Now it is obvious that by always causing the marked ant (A) to cross the bridge K on a particular piece of paper, and if at other times the papers K and L were reversed, I should be able to ascertain whether other ants who came to the larvæ had had the direction and position explained to them, or whether, having been informed by A of the existence of the larvæ, they found their way to them by tracking A's footsteps. If the former, they would in any case pass over the bridge K by whichever strip of paper it was constituted. On the other hand, if they found the larvæ by tracking, then as the piece of paper by which A passed was transferred to L, it would mislead them and carry them away from the larvæ to I. In every case,

then, I transposed the two papers forming the little bridges K and L as soon as the ant A had crossed over.

I put her to the larvæ on F at 6.15 A.M. After examining them carefully, she returned to the nest at 6.34. No other ants went out; but she at once reappeared with 4 friends and reached the larvæ at 6.38. None of her friends, however, crossed the bridge; they went on to D, wandered about, and returned home. A returned to the larvæ at 6.47, this time with one friend, who also went on to D and returned without finding the larvæ.

7. 0.	Ant A to larvæ.				
7. 8	„	An ant at 7.10 went over L to I.			
7.17	„	with a friend, who at 7.21		„	„
7.25	„	with two friends, one of whom at	7.27	„	„
7.32	„	the other at 7.35		„	„
7.39	„	with a friend who went on to D, and then at	7.41	„	„
7.46	„	An ant at 7.42		„	„
7.55	„	„	7.47	„	„
8. 3	„	„	7.48	„	„
8. 8	„	„	7.54	„	„
8.19	„	„	7.57	„	„
8 24	„	„	9 10 found the larvæ.		
8 39	„	„	9.30 went over L to I.		
8.50	„				
9.12	„				
9.22	„				
9.40	„				
9.47	„				
9.55	„				
10.35	„				

At 10.35 I imprisoned her till 12.30, when I put her again to the larvæ.

12.48 back to larvæ.

12.55	„	An ant at 12.58 went over L to I.			
1. 0	„	„	1. 1	„	„
1.15	„	„	1.10	„	„
1.20	„	„	1.13	„	„

After this she did not come any more. During the time she

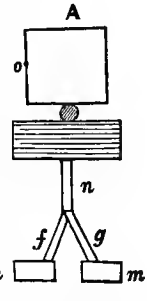
made, therefore, 25 visits to the larvæ; 21 other ants came a distance of nearly 4 feet from the nest and up to the point of junction within 2 inches of the larvæ; but only 1 passed over the little bridge K, while 15 went over the bridge L to I. On repeating this experiment with another marked ant, she herself made 40 journeys, during which 19 other ants found their way to the point of junction. Only 2 went over the little bridge to the larvæ, 8 went over L to I, and the remainder on to D.

Another made 16 journeys; and during the same time 13 other ants came to the point of junction. Of these 13, 6 went on to D, 7 crossed over L to I, and not one found the larvæ. Thus altogether, out of 53 ants, 20 went on to D, 30 crossed over in the wrong direction to I, and only 3 found their way to the larvæ.

From Jan. 2 to Jan. 24 (1875) I made a series of similar observations; and during this time 39 strangers came in all. Of these, 10 went straight on to D, 21 across to the paper to I, and only 8 to the larvæ.

This, I think, gives strong reason to conclude that, under such circumstances, ants track one another by scent.

Fig. 2.



I then slightly altered the arrangement of the papers as shown in the accompanying diagram (fig. 2). A, as before, is the nest, *o* being the door. B is the board; *h* is a glass on which are placed the larvæ; *m* is a similar glass, but empty; *n* a strip of paper: to the end of *n* are pinned two other strips *f* and *g* in such a manner that they can be freely turned round, so that they can be turned at will either to *h* or *m*. Under ordinary circumstances the paper *f*, as in the figure, was turned to the larvæ; but whenever a strange ant came, I turned the papers, so that *f* led to *m* and *g* to *h*. The result was so striking that I give the observation in full.

Jan. 24. I put an ant, which already knew her way, on the larvæ at 3.22.

At 3.30 she returned.

4.15 „

4.25 „

4.34 „

4.42 „

4.50 „

4.56 „

At 3.38 a stranger came; and the bridge *f* being there, she went over it to *m*.

3.50 „ „

4.35 „ „

5.15 „ „

At 5. 5 she returned.
5.14 „
5.25 „

Jan. 25, 6.30 A.M. Put two ants, which knew their way, to the larvæ.

No. 1.	No. 2.	
Returned at 6.55.		
„ 7. 7		
	Returned at 7.11.	
„ 7.15		
	„ 7.27	
„ 7.35		
„ 7.46		
	„ 7.47	
„ 7.49		
	„ 7.51	
„ 7.53		
	„ 7.57	
„ 8. 0		
„ 8. 3		
„ 8. 8		
		8.16 stranger to *m*.
„ 8.17		
	„ 8.18	
„ 8.21		
		8.22 „
„ 8.25	„ 8.25	
		8.27 „
„ 8.29		
	„ 8.30	
„ 8.31		
	„ 8.34	
„ 8.35		
	„ 8.36	
„ 8.40	„ 8.40	
„ 8.44		
		8.45 „
	„ 8.46	
„ 8.47		
„ 8.51	„ 8.51	

No. 1.	No. 2.	
Returned at 8.55		
	Returned at 8.59	
„ 9. 3		
„ 9. 8		
„ 9.18		
„ 9.24		
„ 9.27		
„ 9.30		
„ 9.32		
„ 9.34		
	„ 9.35	
„ 9.37		
„ 9.43	„ 9.43	
		9.44 stranger to *m*.
„ 9.45	„ 9.45	
„ 9.47		
„ 9.50		
	„ 9.51	
„ 9.55	„ 9.55	
„ 9.58	„ 9.58	
„ 10. 1	„ 10. 1	
„ 10. 7	„ 10. 7	
„ 10.10	„ 10.10	
		10.11 „
	„ 10.15	
„ 10.16		
	„ 10.17	
„ 10.18		
„ 10.20	„ 10.20	
„ 10.22	„ 10.22	
„ 10.24		
„ 10.28		
	„ 10.30	
„ 10.32		
	„ 10.33	
„ 10.35	„ 10.35	
„ 10.38		
	„ 10.39	
„ 10.42	„ 10.42	
„ 10.45		

No. 1.	No. 2.		
	Returned at 10.46		
Returned at 10.48			
	,, 10.49		
,, 10.51	,, 10.51		
,, 10.53	,, 10.53		
,, 10.55			
,, 10.58	,, 10.58		
,, 11. 0			
	,, 11. 1		
,, 11. 2			
,, 11. 5			
,, 11.10			
,, 11.12			
		11.15 stranger to *m.*	
,, 11.16			
,, 11.21			
,, 11.23			
	,, 11.24		
,, 11.26	,, 11.26		
,, 11.30	,, 11.30		
,, 11.35	,, 11.35		
,, 11.36			
,, 11.40	,, 11.40	11.40	,,
		11.42	,,
	,, 11.43		
,, 11.45	,, 11.45		
,, 11.46			
	,, 11.50		
	,, 11.51		
	,, 11.56		
,, 11.58			
	,, 11.59		
,, 12. 0			
,, 12. 2	,, 12. 2		
,, 12. 6	,, 12. 6		
,, 12.10	,, 12.10		
,, 12.14			
,, 12.16			
,, 12.20	,, 12.20	12.20	,,
,, 12.24	,, 12.30 dropped.		
	1. 2 imprisoned her.		

Returned at 12.31 — 12.35 stranger to *m*.

„ 12.36
„ 12.44
„ 12.46
„ 12.50
„ 12.54
„ 12.59
„ 1. 1

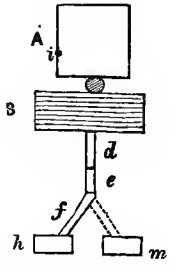
I then put her into a small bottle.

I let them out again at 7.10 on the 27th. Though the interval was so long, they began at once to work; but one unfortunately met with an accident. The other returned as follows, viz. at

7.20
7.30
7.40
7.48 stranger to *m*.
7.46
7.51
7.55
7.59

In these experiments, therefore, 17 strangers came; but at the point *n* they all took the wrong turn, and not one reached the larvæ.

Fig. 3.



Although the observations above recorded seem to me almost conclusive, still I varied the experiments once more (see fig. 3), making the connexion between the board B and the glass containing the larvæ by three separate, but similar strips of paper, *d*, *e*, and *f*, as shown in the figure. Whenever, however, a strange ant came, I took up the strip *f* and rubbed my finger over it two or three times so as to remove any scent, and then replaced it. As soon as the stranger had reached the paper *e*, I took up the strip *d*, and placed it so as to connect *e* with the empty glass *m*. Thus I escaped the necessity of changing the paper *f*, and yet had a scented bridge between *e* and *m*. The results were as follows:—

Jan. 27. At 5.30 I let out the same two ants as were under observation in the preceding experiments.

No. 1.			No. 2.	
Returned at 5.40, the other not till 6.49				
„ 6. 0				
„ 6. 8				
„ 6.26				6.22 stranger to *m*.
„ 6.32				
„ 6.37				
„ 6.41				
„ 6.45				
„ 6.48	„	„	6.49	6.50 „
„ 6.51				6.52 „
„ 6.54	„	„	7. 0	6.53 stranger to larvæ.
„ 7. 1				
„ 7. 5	„	„	7. 6	
„ 7. 9	„	„	7.12	
„ 7.17	„	„	7.17	
	„	„	7.22	7.27 stranger to *m*.
„ 7.25	„	„	7.28	
„ 7.29	„	„	7.34	

I then put them into the bottle.

Jan. 28. Let them out at 6.45.

No. 1.	No. 2.	
Back at 7. 0		
„	7. 3	
„ 7. 5		
„ 7.11		
„	7.12	
„ 7.16		
„ 7.21		
„ 7.27		
		7.31 stranger to *m*.
„	7.32	
„	7.42	
„ 7.45	She dropped into	
„ 7.52	some water.	
„ 8. 2		
„ 8.11		
„ 8.20		
„ 8.26		
„ 8.30		
„ 8.36		

No. 1.

Back at 8.40
„ 8.44
„ 8.48

I then put them into the bottle.

Jan. 29. I let them out at 7.35 A.M.

No. 1 returned at 7.47, after which I saw her no more. I fear she must have met with an accident.

No. 2 returned at

7.56
8. 8
8.18
8.28
8.35
8.42
8.48

8.50 a stranger came to the larvæ, marked her No. 3.

8.56
9. 5
9.19 No. 3
9.20
9.26
9.36
9.46 2 strangers to larvæ.
9.47 5 strangers to *m*.

At 9.40 I found one of the ants which had been under observation on the 24th, and put her to the larvæ. She returned as follows (No. 4).

No. 4.

9.50
9.52
9.55
9.58
10. 3
10.10
10.12
10.15
10.20 10.20
10.23
10.26 10.26
10.29
10.33
10.36
10.37
10.40
10.41 10.41

No. 2.	No. 3.	No. 4.	
10.44		10.44	
10.48			
		10.51	
10.53			
	10.56		
		10.57	
10.59	10.59		
	11. 2	11. 2	
11. 4			
			11. 5 stranger to larvæ.
	11. 7		
			11. 8 „ „
11. 9	11. 9		
		11.10	
	11.13		
		11.14	
	11.16		
11.17			
		11.18	
	11.20		11.20 „ „
			11.21 „ „
			11.22 stranger to *m*.
11.23	11.23	11.23	
			11.25 stranger to larvæ.
	11.26		
11.28			
	11.30		
11.33	11.33		
			11.35 „ „
11.40			
	11.42		
		11.44	
	11.46		
11.47			
	11.50	11.50	
	11.54	11.54	
			11.55 stranger to *m*.
	11.58	11.58	
12. 0			
	12. 1		
			12. 6 „ „
		12. 7	
	12. 8		
12.10			
	12.13		
		12.14	

No. 2.	No. 3.	No. 4.
12.15		
	12.18	
		12.24
	12.25	
12.27		
	12.30	
12.36	12.36	
		12.39
	12.40	
	12.43	
		12.45
	12.47	
	12.50	
		12.52
	12.53	
	12.56	
		12.57
	12.59	
		1. 0
	1. 7	1. 7
	1.12	
1.13		
		1.18
1.22		
		1.25
		1.33
		1.41
1.44		
		1.51
	1.55	
		1.56
		2. 9
		2.35

I then put her into a small bottle. We kept a look-out for Nos. 2 and 3 till 7.30 P.M.; but they did not return.

Jan. 30. Let No. 4 out at 7 A.M. She returned at 7.45.

No. 3 came of herself at	8. 0	No. 4.	
Returning at	8. 9	8. 9	
		8.15	stranger to larvæ.
„	8.20		
		8.25	
„	8.30		
„	8.36		

	No. 3.	No. 4.	
Returning at	8.40		
		8.43	
		8.51	Stranger to *m*.
,,	8.52		
		9. 3	
,,	9. 5		
	Imprisoned them.		
Let them out at 10.55.			
Returning at	11. 1		
		11. 3	
		11. 8	
,,	11. 9		
		11.14	Stranger to *m*.

And they went on coming regularly till 1, when I put them again in a bottle.

Jan. 31. Let them out at 6.35 A.M.

No. 3.	No. 4.	
6.55		
7.12		
	7.15	
7.21		
	7.29	
7.37		
7.42	7.42	
7.48		
	7.53	
		7.55 stranger to *m*.
		8. 0 ,,
8. 1		
8.12		
	8.18	
8.20		
		8.24 ,,
8.27		
	8.28	
8.32		
		8.36 stranger to larvæ.
8 39		
.44		

I imprisoned them.

Jan 31. Let them out at 5.35 P.M.

No. 3.	No. 4.	
	5.47	
6.25		
6.35		
6.48		
6.53		
7. 2		
7. 7		
7.11		
7.16		
7.20		
	7.23 stranger to larvæ.	
7.25		
	7.26	„ „
	7.27	„ *m.*
	7.29	„ „
7.30	7.30	„ larvæ.
Imprisoned her	7.31	„ *m.*

Feb. 1. Let her out at 7.5.

	No. 3.	
She returned at	7.20	
„	7.30	
		7.38 stranger to *m.*
„	7.40	
„	7.48	
„	7.58	
		7.59 „
„	8. 6	
„	8.12	
		8.14 „
		8.17 „
„	8.22	

Imprisoned her and let her out again at 6.20 P.M.

She returned at	6.35
„	6.52
„	7. 0
„	7. 5
„	7.15

No. 3.

She returned at 7.20
„ 7.25
Imprisoned her.

Feb. 2. Let her out at 6.30 A.M.

She returned at 6.50
„ 7. 0
7. 2 stranger to *m*.
„ 7. 7
7.10 two strangers to *m*.
„ 7.13
„ 7.17
7.27 stranger to larvæ.
„ 7.28
„ 7.36
7.38 „ *m*.
„ 7.45
„ 7.50
7.51 „ .
7.55
„ 8. 4
8. 6 „ „
„ 8.11
„ 8.18
„ 8.25
„ 8.30
„ 8.35
„ 8.45
„ 8.46
Imprisoned her.

In this experiment, then, the bridge over which the marked ant passed to the larvæ was left in its place, the scent, however, being removed or obscured by the friction of my finger; on the other hand, the bridge had retained the scent, but was so placed as to lead away from the larvæ; and it will be seen that, under these circumstances, out of 41 ants which found their way towards the larvæ as far as *e*, 14 only passed over the bridge *f* to the larvæ, while 27 went over the bridge *d* to the empty glass *m*.

Taking these observations as a whole, 150 ants came to the point *e*, of which 21 only went on to the larvæ, while 95 went

away to the empty glass. These experiments, therefore, are in entire accordance with those already laid before the Society, and seem to me to show that when an ant has discovered a store of food and others gradually flock to it, they are guided, in some cases by sight, while in others they track one another by scent.

As to their Intelligence and Provident Habits.

It is generally stated that our northern ants do not store up food. But it must be remembered that their nourishment is, for the most part, of a very perishable character, and could not be preserved. Those ants, however, which collect *Aphides* may fairly be said, in doing so, to provide for themseleves the means of subsistence.

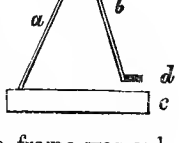
M. Lund tells the following story as bearing on the intelligence of ants *:—

"Passant un jour près d'un arbre presque isolé, je fus surpris d'entendre, par un temps calme, des feuilles qui tombaient comme de la pluie. Ce qui augmenta mon étonnement, c'est que les feuilles détachées avaient leur couleur naturelle, et que l'arbre semblait jouir de toute sa vigueur. Je m'approchai pour trouver l'explication de ce phénomène, et je vis qu'à peu près sur chaque pétiole était postée une fourmi qui travaillait de toute sa force; le pétiole était bientôt coupé et la feuille tombait par terre. Une autre scène se passait au pied de l'arbre : la terre était couverte de fourmis occupées à découper les feuilles à mesure qu'elles tombaient, et les morceaux étaient sur le champ transportés dans le nid. En moins d'une heure le grand œuvre s'accomplit sous mes yeux, et l'arbre resta entièrement dépouillé."

With reference to this interesting account, I tried the following experiment:—

Oct. 15, noon. (See fig. 4.) At a distance of 10 inches from the door of a nest of *F. nigra* I fixed an upright ash wand 3 feet 6 inches high (*a*), and from the top of it I suspended a second, rather shorter wand (*b*). To the lower end of this second wand, which hung just over the entrance to the nest (*c*), I fastened a flat glass cell (*d*) in which I placed a number of larvæ of *F. flava*, and to them I put three or four specimens of *F. nigra*. The drop from the glass cell to the upper part of the frame was only

Fig. 4.



* Ann. des Sci. Nat. 1831, p. 112.

½ an inch; still, though the ants reached over and showed a great anxiety to take this short cut home, they none of them faced the leap, but all went round by the sticks, a distance of nearly 7 feet. At 6 P.M, there were over 550 larvæ in the glass cell, and I reduced its distance from the upper surface of the nest to about ⅖ of an inch, so that the ants could even touch the glass with their antennæ, but could not reach up nor step down. Still, though the drop was so small, they all went round. At 11 P.M. the greater number of the larvæ had been carried off; so I put a fresh lot in the cell. The ants were busily at work. At 3 A.M. I visited them again. They were still carrying off the larvæ, and all going round. At 6 A.M. the larvæ were all removed. I put a fresh lot, and up to 9 A.M. they went on as before.

The following day (Oct. 17), I took two longer sticks, each 6 feet 6 inches in length, and arranged them in a similar manner, only horizontally instead of vertically. I also placed fine earth under the glass supporting the larvæ. At 8 o'clock I placed an ant on the larvæ; she took one, and I then coaxed her home along the sticks. She deposited her larva and immediately came out again, not, however, going along the stick, but under the larvæ, vainly reaching up and endeavouring to reach the glass. At 8.30 I put her on the larvæ again, and as she evidently did not know her way home, but kept stretching herself down and trying to reach the earth under the glass cell, I again coaxed her home along the sticks. At 9.3 she came out again, and again went under the larvæ and wandered about there. At 10 I put her on the larvæ and again helped her home. At 10.15 she came out again, and this time went to the stick, but still wanted some guidance. At 10.45 she again reached the frame, but immediately came out again, and I once more coaxed her round. After wandering about some time with a larva in her mouth, she dropped down at 11.14. After depositing her larva, she came out directly and went under the larvæ. I again coaxed her round, and this time also she dropped off the glass with her larva. At 12.30 she came out again, and for the last time I helped her round. After this she found her way by herself. At 12.20 another (No. 2) found her way round and returned at 12.37. For the next hour their times were as follows:—

No. 1.	No. 2.
12.46	
	12.47
12.54	12.54
	1. 0
1. 1	
1. 7	
	1. 8
1.12	
	1.14
1.19	
	1.21
1.26	
	1.28
1.32	
	1.34
1.38	
	1.41
1.45	
	1.47
1.52	
	1.54

Thus they both made 9 visits in an hour. As regards actual pace, I found they both did about 6 feet in a minute. Soon after these began, other ants came with them. It was a beautiful day, and all my ants were unusually active. At 1 P.M. I counted 10 on the sticks at once, by 1.30 over 30, and at 5 in the afternoon over 60. They went on working very hard, and forming a continuous stream till I went to bed at 11; and at 4 in the morning I found them still at work; but though they were very anxious and, especially at first, tried very hard to save themselves the trouble of going round, they did not think of jumping down, nor did they throw the larvæ over the edge.

Moreover, as I had placed some sifted mould under the glass, a minute's labour would have been sufficient to heap up one or two particles, and thus make a little mound which would have enabled them to get up and down without going round. A mound $\frac{1}{8}$ inch high would have been sufficient; but it did not occur to them to form one.

The following morning (Oct. 18) I put out some larvæ again at 6 A.M. Some of them soon came; and the same scene continued till 11.30, when I left off observing.

Again, on the 22nd Oct. I placed a few larvæ of *F. flava* in a glass, which I kept continually replenished, which was suspended $\frac{1}{3}$ of an inch above the surface of the frame containing their nest, but only connected with it by tapes 5 feet long. I then, at 6.30, put a *F. nigra* to the larvæ; she took one and tried hard to reach down, but could not do so, and would not jump; so I coaxed her round the tapes. She went into the nest, deposited her larva, and immediately came out again. I put her back on the larvæ at 7.15; she took one, and again tried hard, but ineffectually, to reach down. I therefore again coaxed her round. She went into the nest, deposited her larva, and came out again directly as before. I put her back on the larvæ at 7.35, when the same thing happened again. She got back to the nest at 7.40, and immediately came out again. This time she found her way round the string, with some help from me, and reached the larvæ at 7.50. I helped her home for the last time. The next journey she found her way without assistance, and reached the larvæ at 8.26. After this she returned as follows, viz.:—

At 8.50
9. 0
9.10
9.17
9.28

I now made the length of the journey round the tapes 10 feet. This puzzled her a little at first.

She returned as follows:—

9.41	10.35
9.55	10.44
10. 8	10.55
10.16	11. 6
10.26	11.14 with a friend.

I now made the length 16 feet.

She returned at 11.34
„ 12.14
12·20 two strangers found the larvæ.
„ 12.31
„ 12.50
„ 1.10
„ 1.30

She returned at 1.46
" 1.59
" 2.10
" 2.20
" 2.35
" 2.45
" 2.52
" 3.10
3.19
" 3.29
" 3.40
" 3.50 I now put between 700 and 800
" 4.14 larvæ in the glass.
" 4.31
4.33 a stranger came.
" 4.44
" 4.56
" 5. 8
5.12 "
5.20 "
" 5.25
" 5.40
" 6. 6
6.10 "
" 6.51
" 7. 0
" 7.11
7.15 "

It surprised me very much that she preferred to go so far round rather than to face so short a drop.

In illustration of the same curious fact, I several times put specimens of *F. nigra* on slips of glass raised only one third of an inch from the surface of the nest. They remained sometimes three or four hours running about on the glass, and at last seemed to drop off accidentally.

Myrmica ruginodis has the same feeling. One morning, for instance, I placed one in an isolated position, but so that she could escape by dropping one third of an inch. Nevertheless at the same hour on the following morning she was still in captivity, having remained out twenty four hours rather than let herself down this little distance.

In my previous memoir I called attention to M. Forel's interesting statement that when ants quit the pupa stage, they cannot distinguish friends from foes, though three or four days are sufficient to enable them to do so. On this point M. Forel has favoured me with the following interesting explanation :—

"Je prends des fourmis toutes jeunes (blanches encore) de *fourmilières* et *d'espèces* entièrement différentes ; elles se mêlent toutes amicalement les unes aux autres sans distinction, à l'exception d'une *rufibarbis* ♀ qui se trouve être un peu plus âgée et se retire à l'écart avec un cocon ; elle ne se décide à s'allier aux autres que le lendemain. *Dix jours* après le commencement de l'expérience j'établis mes fourmis *qui ont formé une communauté* dans un coin, et je leur apporte de nouvelles jeunes fourmis toutes blanches prises au dehors. Les nouvelles venues elles ne sont pas mal disposées ; elles entrent au contraire dans la fente de mur où sont les autres, mais les anciennes les repoussent, les menacent et les jettent dehors. Cette expérience démontre qu'au bout de dix jours les fourmis distinguent *leurs camarades* des étrangères, tandis qu'elles ne font pas cette distinction dans les premiers jours qui suivent leur éclosion. Si je me suis permis d'écrire qu'il suffit de trois ou quatre jours de vie pour qu'une nouvelle éclose sache reconnaître un ami d'un ennemi, ce n'est pas à la suite d'une expérience directe faite dans le but de fixer ce terme, mais parceque dans les innombrables observations faites sur ces fourmis je me suis assuré qu'il le fallait à peu près ce temps pour atteindre un certain degré de coloration et de consistance, et qu'à ce degré de coloration et de consistance elles commencent à distinguer leurs ennemis, soit qu'elles s'enfuient, soit qu'elles leur montrent les dents. J'aurais du reste peut-être mieux fait de ne pas fixer ainsi ce temps, car il y a tant de variations individuelles, suivant la température &c. que l'on ne peut être assez prudent avant de généraliser. En hiver les jeunes fourmis deviennent beaucoup moins vite adultes qu'en été."

Division of Labour.

In a nest of *F. fusca* which I established in my room on the 13th of December 1874, and in which the females began laying eggs about the middle of April, the pupæ had all come to maturity by the end of August ; and after this very few of the ants came out of the nest. On the 3rd of September I noticed an ant at some

honey which I had put out for their use. From that time to the present (Oct. 30) I have observed no other ant at the honey, while, on the contrary, I have found this particular ant feeding over and over again,—for instance, on the 12th, 13th, 14th, 15th, 17th, 19th, 20th, 24th, 25th, 26th, 27th, and 28th of September, 1st, 5th, 12th, 19th, 22nd, 24th, and 30th of October. As I was away sometimes for two or three days together, and am generally only at home in the mornings and evenings, it is very probable that this ant visited the honey every day, and took in stores to her companions. I have already mentioned a somewhat similar though less marked case.

Concerning Affection and Behaviour to Wounded.

As regards the affection of ants for one another, Latreille makes the following statement:—"Le sens de l'odorat," he says*, "se manifestant d'une manière aussi sensible, je voulois profiter de cette remarque pour en découvrir le siége. On a soupçonné depuis longtemps qu'il résidoit dans les antennes. Je les arrachai à plusieurs fourmis fauves ouvrières, auprès du nid desquelles je me trouvois. Je vis aussitôt ces petits animaux que j'avois ainsi mutilés tomber dans un état d'ivresse ou une espèce de folie. Ils erroient çà et là, et ne reconnoissoient plus leur chemin. Ils m'occupoient; mais je n'étais pas le seul. Quelques autres fourmis s'approchèrent de ces pauvres affligées, portèrent leur langue sur leurs blessures, et y laissèrent tomber une goutte de liqueur. Cet acte de sensibilité se renouvela plusieurs fois; je l'observoi avec une loupe. Animaux compatissans! quelle leçon ne donnez-vous pas aux hommes."

"Jamais," says M. de Saint Fargeau†, "une Fourmi n'en rencontre une de son espèce blessée, sans l'enlever et la transporter à la fourmilière. L'y soigne-t-elle? Je ne sais, mais je vois dans ce fait une bienveillance que je ne retrouve dans aucun autre insecte, même social."

I have not felt disposed to repeat M. Latreille's experiment, nor have I been so fortunate as to witness such a scene accidentally. My limited experiences have been of the opposite character. On one occasion (Aug. 13) a worker of *F. nigra*, belonging to one of my nests, had got severely wounded, but not so much so that she could not feed; for though she had

* Hist. Nat. des Fourmis, p. 41.

† Hist. Nat. des Ins. Hymèn. vol. i. p. 99.

lost five of her tarsi, finding herself near some syrup, she crept to it and began to feed. I laid her gently on her back close to the entrance into the nest. Soon an ant came up to the poor sufferer, crossed antennæ with her for a moment, then went quietly on to the syrup and began to feed. Afterwards three other ants did the same; but none took any more notice of her.

Aug. 15. I found at 1 P.M. a *Myrmica ruginodis* which had lost the terminal portion of both her antennæ. She seemed to have lost her wits. I put her into her nest; but the others took no notice of her; and after wandering about a little, she retired into a solitary place, where she remained from 3 P.M. to 8 without moving. The following morning I looked for her at 5.30, and found her still at the same spot. She remained there till 9, when she came out. She remained out all day; and the following morning I found her dead.

Indeed I have often been surprised that in certain cases ants render one another so little assistance. The tenacity with which they retain their hold on an enemy they have once seized is well known. M. Moequerys even assures us that the Indians of Brazil made use of this quality in the case of wounds; causing an ant to bite the two lips of the cut and thus bring them together, after which they cut off the ant's head, which thus holds the lips of the wound together. He asserts that he has often seen natives with wounds in course of healing with the assistance of seven or eight ants' heads*! Now I have often observed that some of my ants had the heads of others hanging on to their legs for a considerable time; and as this must certainly be very inconvenient, it seems remarkable that their friends should not relieve them of such an awkward encumbrance.

Recognition of Friends.

I have also made some experiments on the power possessed by ants of recognizing their friends. It will be remembered that Huber gives a most interesting account of the behaviour of some ants, which, after being separated for four months, when brought together again, immediately recognized one another, and "fell to mutual caresses with their antennæ." Forel, on the contrary, regards these movements as indicating fear and surprise rather than affection, though he also is quite inclined to believe, from his

* Ann. Soc. Ent. France, 2 Sér. tom. ii. p. 67.

own observations, that ants would recognize one another after a separation of some months. The observation recorded by Huber was made casually; and he does not seem to have taken any steps to test it by subsequent experiments. The fact is one, however, of so much interest that it seemed to me desirable to make further experiments on the subject. On the 4th of August I separated one of my nest of *F. fusca* into two halves, which I kept entirely apart from one another.

Four days afterwards (August 8th) I put an ant from a different nest into one of these at 8 A.M. She was at once attacked; two hung on to her till about 11, when they left her. Before evening she seemed to have fraternized with them.

Aug. 13. I put another stranger into one of these nests at 9 A.M. At 10.30 one of the ants was dragging her about by an antenna; at 1 she was free; and at 2 I found her among the rest, apparently received as a friend. Two days afterwards she was still well.

Aug. 16. I took one of the ants which I had removed from the others on the 4th and replaced her with her old companions. They seemed to take no notice of her, and certainly did not attack her.

Aug. 20. I put in a stranger at 7.30. At 7.45 one of them had hold of her by the mandibles; at 9.30 one was hanging on to her hind leg; at 10.45 she was free; and I did not see them attack her any more.

Aug. 22. At 7.30 put in a stranger and one of their former companions. One of the ants attacked the former; they took no notice of the latter so far as I could see. At 10.45 they both seemed at home. This stranger I saw repeatedly afterwards, and she had evidently been received completely into the community.

Sept. 3. At 7 A.M. I put a stranger in and also one of their old companions. Neither of them was attacked.

Sept. 17. Put in three strangers; but they were not attacked.

Oct. 3. I put in another stranger; but they did not seem to mind her.

As, therefore, in some cases these ants did not appear disposed to attack strangers, I tried similar experiments with a nest of *Myrmica ruginodis*.

On the 20 August I divided a colony of this species, so that one half were in one nest (No. 9) and the other half in another (No. 15), and kept them entirely apart.

On the 3rd Oct. I put into nest 15 a stranger and an old com-

panion from nest 9. One of them immediately flew at the stranger; of the other they took no notice.

Oct. 18. At 10 A.M. I put in a stranger and a friend from nest 9. In the evening the former was killed, the latter was all right.

Oct 19. I put one in a small bottle with a friend from nest 9. They did not show any enmity. I then put in a stranger; and one of them immediately began to fight with her. In the evening the stranger was dead.

Oct. 24. I again put in a stranger and a friend. The former was attacked, but not the latter. The following day I found the former almost dead, while the friend was all right.

Oct. 31. I again put in a stranger and a friend. The former was at once attacked; but in this case the friend also was, after a bit, seized by the leg, but eventually released again. On the following morning the stranger was dead, the friend was all right.

Nov. 7. Again I put in a stranger and a friend. The ~~latter~~ was soon attacked and eventually killed; of the ~~former~~ they did not seem to me to take any particular notice. I could see no signs of welcome, no gathering round a returned friend; but, on the other hand, she was not attacked.

The Senses.

Much has been written on the use of the antennæ of insects. That they serve as organs of touch all are agreed; but it is almost equally clear that this is not in most cases their only function. Some entomologists regard them as auditory, some as olfactory organs. There is, however, a third alternative, which I would venture to suggest, namely that in those insects in which the sense of hearing is highly developed they may serve as ears, while in those which have a very delicate sense of small, they may act as olfactory organs. This view is not in itself so improbable as might at first sight appear. It is evident that, in the Articulata, organs of sense are developed in various parts of the body. Whether the curious organ discovered by Müller in the metathorax of certain Orthoptera be an ear or not, it must surely be an organ of some sense. Hicks and others have described structures in the halteres and wings of various insects which have all the appearance of being organs of sense; while among the Crustacea we find the remarkable case of *Mysis*, which even has an organ of sense in its tail. It is not then so improbable as might at first sight

appear, that the antennæ should in some species act as ears and in others serve for the perception of odours. The position, moreover, which they occupy renders them a most advantageous situation for an organ of sense. This suggestion would also explain various experiments and observations recorded by skilful entomologists, and which it is otherwise difficult to reconcile with one another.

The Sense of Hearing.

Many eminent observers have regarded the antennæ as auditory organs, and have brought forward strong evidence in favour of their view. Lespés, for instance, found that a female *Locusta viridissima*, which was very sensitive to sound, lost apparently all power of hearing when the antennæ were removed. She lived a fortnight longer and continued to eat. M. Lespés observed no other result except the loss of hearing.

So far as I am aware, no proof has yet been adduced that ants possess the power of hearing. In order, if possible, to throw some light upon this interesting question, I made a variety of loud noises, including those produced by a complete set of tuning-forks, as near as possible to the ants mentioned in the preceding pages, while they were on their journeys to and fro between the nests and the larvæ. In these cases the ants were moving at a steady pace and in a most business-like manner, and any start or alteration of pace would have been at once apparent. I was never able, however, to perceive that they took the slightest notice of any of these sounds. Thinking, however, that they might perhaps be too much absorbed by the idea of the larvæ to take any notice of my interruptions, I took one or two ants at random and put them on a strip of paper, the two ends of which were supported by pins with their bases in water. The ants imprisoned under these circumstances wandered slowly backwards and forwards along the paper. As they did so, I tested them in the same manner as before, but was unable to perceive that they took the slightest notice of any sound which I was able to produce. I then took a large female of *F. ligniperda*, and tethered her on a board to a pin by a delicate thread about 6 inches in length. After wandering about for a while, she stood still, and I then tried her as before; but, like the other ants, she took no notice whatever of the sounds.

It is of course possible, however, if not probable, that ants, even

if deaf to sounds which we hear, may hear others to which we are deaf. On this subject I hope to make some experiments, in which Mr. Spottiswoode has kindly promised to assist me.

The Sense of Smell.

I have also made similar experiments, though with very different results, on the power of smell possessed by ants. I dipped camel's-hair brushes into peppermint-water, essence of cloves, lavender-water, and other strong scents, and suspended them about $\frac{1}{4}$ of an inch above the strips of paper along which the ants were passing in the experiments above recorded. Under these circumstances, while some of the ants passed on without taking any notice, others stopped when they came close to the pencil, and, evidently perceiving the smell, turned back. Soon, however, they returned and passed the scented pencil. After doing this two or three times, they generally took no further notice of the scent. This experiment left no doubt on my mind; still, to make the matter even more clear, I experimented with ants placed on an isolated strip of paper, as described on p. 495. Over the paper, and at such a distance as almost, but not quite, to touch any ant which passed under it, I again suspended a camel's-hair brush, dipped in assafœtida, lavender-water, peppermint-water, essence of cloves, and other scents. In this experiment the results were very marked; and no one who watched the behaviour of the ants under these circumstances could have the slightest doubt as to their power of smell.

I then took a large female of *F. ligniperda* and tethered her on a board by a thread as before. When she was quite quiet I tried her with the tuning-forks; but they did not disturb her in the least. I then approached the feather of a pen very quietly, so as just to touch first one and then the other of the antennæ, which, however, did not move. I then dipped the pen in essence of musk and did the same; the antenna was slowly retracted and drawn quite back. I then repeated the same with the other antenna. If I touched the antenna, the ant started away, apparently smarting. I repeated the same with essence of lavender and with a second ant.

As to Sentiments of Benevolence.

Mr. Grote, in his 'Fragments on Ethical Subjects,' regards it as an evident necessity that no society can exist without the sentiment of morality. "Every one," he says, "who has either spoken or written on the subject has agreed in considering this sentiment as absolutely indispensable to the very existence of society. Without the diffusion of a certain measure of this feeling throughout all the members of the social union, the caprices, the desires, and the passions of each separate individual would render the maintenance of any established communion impossible. Positive morality, under some form or other, has existed in every society of which the world has ever had experience."

If this be so, then ants also must be moral and accountable beings. I cannot, however, of course urge this, because I have elsewhere attempted to show that even as regards man, the case is not by any means clear. In the case of ants, various observers have recorded instances of attachment and affection, some of which have been referred to in my previous papers. With reference to this part of the subject, I have made some further experiments.

Jan. 3, 1876. I immersed an ant (*F. nigra*) in water for half an hour; and when she was then to all appearance drowned, I put her on the strip of paper I mentioned on p. 473. The strip was half an inch wide; and one of my marked ants belonging to the same nest was passing continually to and fro over it. The immersed ant lay there an hour before she recovered herself; and during this time the marked ant passed by 18 times without taking the slightest notice of her.

I then immersed another ant in water for an hour, after which I placed her on the strip of paper as in the preceding case. She was three quarters of an hour before she recovered: during this time two marked ants were passing to and fro; one of them went by 18 times, the other 20 times; and two strangers also went over the paper; but none of them took the slightest notice of their drowned friend.

I then immersed another ant for an hour, and then put her on the strip of paper. She took an hour to recover. The same two marked ants as in the previous observation were at work. One passed 30 times, the other 28 times, besides which five strangers passed by; but not one took the slightest notice.

I immersed three ants for eight hours, and then put them on

the strip of paper. They began to recover in three quarters of an hour, but were not quite themselves till half an hour afterwards. During the first three quarters of an hour two marked ants passed, each four times; and two others also went by. During the following half-hour the two marked ants passed 16 times, and three strangers; but none of them took any notice.

I immersed another ant for forty minutes, and put her on the strip of paper. She recovered in twenty minutes, during which time two strangers passed, and the marked ants, which were the same as in the preceding case, went by 14 times without taking any notice.

I immersed two ants for ten hours, and then placed them on the strip of paper. The same two marked ants passed respectively 18 and 26 times, and one stranger passed again, without taking any notice. After this I left off watching.

I immersed two ants for four hours, and then put them on the strip of paper. They began to recover in an hour, during which two marked ants, not the same as in the preceding case, passed respectively 28 and 10 times, and two others went by; but none of them took any notice.

I immersed an ant for an hour, and then put her on the same strip of paper as in the previous cases. A marked ant passed her twelve times; and three others also took no notice; but, on the other hand, a fourth picked her up and carried her off into the nest.

Again, I immersed an ant for an hour, and put her on the string. The marked ant passed twice, after which she did not return. Soon after, another ant came by and, picking up the immersed one, carried her off to the nest.

I do not bring forward these cases as proof or even as evidence that ants are less tender to friends in distress than previous observers have stated to be the case; but they certainly show that tenderness is not invariably the rule; and, especially when taken in connexion with the two following cases, they are interesting illustrations of the individual differences existing between ants—that there are Priests and Levites, and good Samaritans, among them as among men.

Bees.

Their Appreciation of Colour.

Bees soon accustom themselves to look for honey on papers of particular colours. For instance, on Sept. 13, at 11 A.M., I brought

up a bee from one of my hives; at 11.40 she returned to honey which I had put on a slip of glass on green paper.

She returned at 11.51. And again

„ 12. 1

„ 12.13

„ 12.22

„ 12.33

„ 12.46

„ 12.58

„ 1.12. This time she lost her way in the room.

„ 1.49

„ 2. 1. This time she got stuck in the honey, and had to clean her.

„ 2.25

„ 2.40. I now put red paper instead of the green, and put the green paper with a similar quantity of honey on it a foot off.

„ 2.51 to the honey on green paper. I then gently moved the green paper with the bee on it, back to the old spot. When the bee had gone, I put yellow paper where the green had been, and put the green again a foot off.

„ 3. to the honey on the yellow paper. I disturbed the bee, and she at once flew to the honey on the green paper; when she had gone, I put orange paper in the old place, and put the green paper about a foot off.

„ 3.10 to the honey on the green paper. I again gently moved the paper, with the bee on it, to the usual place; and when the bee had gone, put white paper in the old place, and put the green a foot off.

„ 3.20 to the honey on the green paper. I again gently moved the green paper, with the bee on it, to the old place; and when she had gone, replaced it by blue paper, putting the green a foot off.

„ 3.30 to the honey on the green paper. I again repeated the same thing, putting yellow instead of blue.

She returned at 3.40 to the green paper. I now reversed the position of the yellow and green papers; but

„ 3.51 to the green. After this
„ 4. 6
„ 4.15
„ 4.28, when she left off for the day,

nor were there any bees still working in the garden. The same afternoon a wasp, which I was observing, remained at work till 6.29.

Aug. 20. About noon I brought five bees to some honey at my window. They all soon returned, and numerous friends came with them. One of them I put to some honey on blue paper. She returned as follows, viz. :—

At 12.36	At 2.30
12.42	2.38
12.53	3. 2
1.28	3.10
1.38	3.22
1.49	3.50
2. 2	4. 4
2.11	4.14
2.24	4.23

when I left off watching and shut her out. The longer intervals are due to her having got some honey every now and then on her wings and legs, when she lost a little time in cleaning herself.

Aug. 21. I opened my window at 6 A.M. No bee came till at 7.33 the above one came to the honey on blue paper.

I also placed some honey on orange paper about 2 feet off.

At 7.42 she returned to the honey on blue paper, and again
7.55 she returned to the honey on blue paper.
8. 3 „ „
8.14 „ „
8.25 „ „
8.36 „ „
8.44 „ „
8.54 „ „
9. 5 „ „

I then transposed the papers, but not the honey.

At 9.16	she came back to the honey on blue paper.		I then tranposed the papers again.
9.29	,,	,,	I then transposed them again.
9.39	,,	,,	,, ,,
9.53	,,	,,	I now put green paper instead of orange, and transposed the places.
10. 0	,,	green paper.	I transposed them again.
10. 8	,,	blue	,, ,,
10.21	,,	green	I now put red paper instead of green, and transposed the places.
10.30	,,	blue	I transposed them again.
10.42	,,	,,	,, ,,
10.53	,,	,,	,, ,,
11. 4	,,	,,	,, ,,
11.16	,,	,,	I now put white paper instead of red, and transposed the places.
11.28	,,	,,	I transposed them again.
11.41	,,	,,	,, ,,
11.56	,,	,,	,, ,,
12. 8	,,	,,	,, ,,
12.17	,,	,,	I now put green paper again instead of white, and transposed the places.
12.27	,,	,,	I transposed them again.
12.40	,,	,,	,, ,,
12.50	,,	,,	,, ,,
1. 0	,,	,,	,, ,,
1.13	,,	,,	,, ,,
1.25	,,	,, and then to the green.	,, ,,
1.40	,,	,,	,, ,,
1.47	,,	green.	
1.57	,,	blue, and then to green.	,, ,,
2. 6	,,	blue.	,, ,,
2.17	,,	,,	

The following day I accustomed this bee to green paper. She

made 63 visits (beginning at 7.47 and ending at 6.44), of which 50 were to honey on green paper.

The following day, Aug. 23, she began work:—

At 7.12 returning to honey on green paper. I then put some on yellow paper about a foot off.

7.19	she turned to the honey on green paper.	I transposed the colours.
7.25	,, ,,	I replaced the yellow paper by orange and transposed the places.
7.36	,, ,,	I transposed the colours so that the orange might be on the spot to which the bee was most accustomed.
7.44	,, ,,	I now put white instead of orange.
7.55	,, ,,	Transposed the papers.
8. 1	,, ,,	I now put blue paper instead of white.
8.12	,, blue paper.	I now put red instead of blue.
8.23	,, green paper.	
8.25	,, ,,	
8.47	,, ,,	

I then ceased observing and removed the honey.

Thus the bee which was accustomed to green returned to that colour when it was removed about a foot, and replaced by yellow, orange, white, and red; but on the other hand, when blue was so placed, she returned to the blue. I kept this bee under observation till the 28th, but not with reference to colours.

Aug. 24. At 7.45 I put a bee (No. 5) to honey on green paper, to which she kept on returning till 9.44. The next day (Aug. 25) she came at 7.38, and I let her come to the green paper till 9. The following morning she returned at 6 A.M., coming back as follows, viz.:—

At 6.10
6.18
6.25
6.35
6.45

6.54
7. 3
7.13

I now put orange in place of green, and put the green a foot off.

At 7.24	she returned	to	the green.	I replaced the paper with the bee on it; and when she had gone I put light blue in place of the green, and again moved the green a foot off.
7.36	„	„	blue.	I again replaced the paper with the bee on it; and when she had gone I put yellow in place of the green, and again moved the green a foot off.
7.44	„	„	green.	I then did exactly the same, only putting vermilion in place of the green.
7.55	„	„	„	I then did exactly the same, only putting white in place of green.
8. 3	„	„	„	

It would almost seem, from these observations, as if there was less distinction in the eye of the bee between green and blue than between green and other colours. If this should be confirmed, it would have an interesting bearing on the colours of flowers.

April 4. A fine day, but cold. I brought a bee to some honey at my window. She returned at the following times:—

1. 1	2.18
1.17	3.11
1.24	3.20
1.41	3.31
1.50	3.38
2. 1	3.50
2. 6	

But during this time only one other bee came to the honey; and, indeed, after 2 no other bees were at work.

I had on Aug. 20 introduced some bees to honey in my room, since which it had been much visited by them. On the 24th I

put a bee to some honey inside a flower-pot 5 inches high and 5 wide at the base. The flower-pot was laid on its side, and the mouth closed, so that the bee had to come out through the hole in the bottom, which was about ½ an inch in diameter. To make things easier for her, I made her a small alighting-board of wood, the top of which was level with the hole. I then placed the flower-pot on the spot where she was accustomed to find the honey. She had made her first visit that morning at 6.45, returning

at 6.55
7. 5
7.14
7.23 And when I put her, while feeding, into the flower-pot, she found her way out without difficulty.

At 7.40 she returned, but did not seem able to find her way; so I put her in. The same thing happened again at
7.50
8. 6
and 8.20

but at 8.38 she found her way in easily, and had no further difficulty. She returned at
8.53
9. 5
9.14
9.25
9.41
9.55
10. 6 This time a friend came with her and followed her in. I captured her. No. 2 took no notice, but returned

At 10.19	At 12. 9
10.30	12.25
10.44	12.37
10.54	12.50
11. 6	1. 2
11.20	1.14
11.31	1.25
11.44	1.36
11.55	1.47

At 1.57	At 4.44
2. 9	4.55
2.19	5.10
2.31	5.24
2.43	5.35
2.59	5.46
3.23	5.58
3.33	6. 9
3.44	6.20
3.56	6.42
4. 7	7. 0
4.21	7.15
4.34	making 59 visits.

After which she came no more that day. With the one exception above mentioned, during the whole time no other bee came to the honey. I might also mention that I had put out six similar flower-pots in a row, and that this seemed to puzzle the bee a good deal; she frequently buzzed about before them, and flew from one to the other before entering. When she went in, she generally stood still just inside the entrance for about thirty seconds, buzzing loudly with her wings. I thought at first whether this could be intended as a sort of gong to summon other bees to the feast; but, though several were flying about, at any rate none came. The following day (Aug. 25) she came at 6.51, and had made nine journeys up to 8.41, when I left off watching. During this time no other bee came.

Aug. 26. She came at 6.32, and up to 8.43 had made 13 journeys.
27 „ 6. 7 „ 8.43 „ 14 „
28 „ 6.17 „ 7.11 „ 5 „

It was a gloomy morning. No other bee came.

Wasps.

In my previous paper I endeavoured to show that wasps are entitled to at least as much credit as bees for industry. Indeed, as far as my experience goes, they both begin to work earlier in the morning and continue later in the evening. But without making any invidious comparisons, the following cases which I give as showing that wasps do not by any means always bring

friends to share any good things they may have discovered, also prove their great industry. Thus:—

July. I marked a wasp (*V. vulgaris*, ♀) and put her to some honey. All day she kept coming back till past 8 in the evening, but brought no friend. I do not think it necessary to give the times of all her visits; but I may give the times for a few. For instance,

At 3.13 she came to the honey, and at 3.14 returned to her nest.

3.16	„	„	3.18	„
3.20	„	„	3.21	„
3.24	„	„	3.25	„
3.27	„	„	3.28	„
3.29	„	„	3.30	„
3.32	„	„	3.33	„
3.35	„	„	3.36	„
3.40	„	„	3.41	„
3.43	„	„	3.44	„
3.46	„	„	3.47	„
3.49	„	„	3.50	„
3.53	„	„	3.54	„
3.56	„	„	3.57	„
4. 0	„	„	4. 1	„
4. 3	„	„	4. 4	„
4. 6	„	„	4. 7	„
4.10	„	„	4.11	„
4.14				

Thus having made no less than 19 journeys in one hour.

On the 10th of September, 1875, I marked a wasp. On the 11th she came to the honey for the first time,

returning at	7.25,	and left at	7.27,
„	7.34	„	7.37
„	7.41	„	7.44
„	7.49	„	7.51
„	7.56	„	7.58
„	8. 3	„	8. 6
„	8.13	„	8.16
„	8.20	„	8.23
„	8.30	„	8.32
„	8.37	„	8.40
„	8.46	„	8.51

She was disturbed.

Returning at	9. 4,	she left at	9. 5
„	9. 9	„	9.10
„	9.15	„	9.16
She was disturbed.			
„	9.30	„	9.32
„	9.50	„	9.54
She was disturbed.			
„	10. 0	„	10. 2
„	10.10	„	10.13
„	10.20	„	10.23
„	10.26	„	10.28
„	10.33	„	10.35
„	10.41	„	10.43
„	10.47	„	10.49
„	10.54	„	10.56
„	11. 0	„	11. 2
„	11. 7	„	11. 9
„	11.14	„	11.16
„	11.20	„	11.22
„	11.26	„	11.29
„	11.33	„	11.35
„	11.39	„	11.41
„	11.45	„	11.47
„	11.53	„	11.54
„	11.59	„	12. 0
„	12. 6	„	12. 8
„	12.14	„	12.16
„	12.20	„	12.22
„	12.28	„	12.30
„	12.35	„	12.37
„	12.42	„	12.44
„	12.49	„	12.52
„	12.55	„	12.57
„	1. 0	„	1. 3
„	1. 8	„	1.10
„	1.14	„	1.15
„	1.19	„	1.21
„	1.25	„	1.27
„	1.31	„	1.33
„	1.37	„	1.39
„	1.43	„	1.45

Returning at	1.51,	she left at	1.53
,,	1.58	,,	2. 0
,,	2. 4	,,	2. 6
,,	2.11	,,	2.13
,,	2.19	,,	2.20
,,	2.28	,,	2.30
,,	2.33	,,	2.35
,,	2.40	,,	2.42
,,	2.45	,,	2.47
,,	2.53	,,	2.56
,,	3. 0	,,	3. 2
,,	3. 4	,,	3. 5
,,	3. 9	,,	3.11
,,	3.15	,,	3.17
,,	3.23	,,	3.25
,,	3.30	,,	3.32
,,	3.37	,,	3.39
,,	3.45	,,	3.47
,,	3.52	,,	3.54
,,	4. 0	,,	4. 2
,,	4. 6	,,	4. 9
,,	4.15	,,	4.17
,,	4.22	,,	4.24
,,	4.29	,,	4.31
,,	4.35	,,	4.37
,,	4.41	,,	4.43
,,	4.50	,,	4.52
,,	4.57	,,	4.59
,,	5. 2	,,	5. 5
,,	5.10	,,	5.12
,,	5.17	,,	5.19
,,	5.23	,,	5.25
,,	5.30	,,	5.32
,,	5.37	,,	5.39
,,	5.44	,,	5.46
,,	5.50	,,	5.52
,,	5.56	,,	5.58
,,	6. 2	,,	6. 4
,,	6. 7	,,	6. 9
,,	6.13	,,	6.15
,,	6.20	,,	6.22

Returning at	6.28,	she left at	6.30
„	6.34	„	6.36
„	6.41	„	6.43

This was her last visit for the evening, making no less than ninety-four visits in the day, during which time only two other wasps found the honey, though it was lying exposed on a table at an open window. The following morning she came at 6.18 and made twenty visits up to 8.18, after which I did not record them. During this time no stranger came.

No doubt, however, if a wasp is put to honey in an exposed place, other wasps gradually find their way to it. To determine, if possible, whether they were purposely brought, I tried the following experiment. On the 20th of September I marked a wasp and put her to some honey, which she visited assiduously. The following morning I opened my window at 6, and she made her first visit at 6.27, the temperature being 61° Fahr. I then placed the honey in a box communicating with the outside by an india-rubber tube 6 inches long and $\frac{1}{3}$ inch in diameter. The wasp, however, soon got accustomed to it, and went in and out without much loss of time. The 22nd was finer; and when I opened my window at 6 in the morning, she was already waiting outside, the temperature being 61°. The 23rd was rather colder, and she came first at 6.20, the temperature being 61°.

I was not at home during these days; but, as far as I could judge from watching in the mornings and evenings, no other wasp found the honey. On the 24th I had a holiday and timed her as follows. It was rather colder than the preceding days, and she did not come till 6.40, when the temperature was 58°. She returned as follows:—

6.49	8.19
6.58	8.26
7.12	8.35
7.22	8.45
7.32	8.52
7.40	9. 2
7.50	9.12
8. 0	9.45
8. 9	

I had almost closed the window; so that she had a difficulty in finding her way.

9.58	10.32
10.10	10.51

The temperature was still only 60°, and it was raining, scarcely any other wasps about.

11. 1	2.59
11.11	3. 8
11.21	3.14
11.29	3.23
11.40	3.32
11.46	3.40
11.56	3.48
12. 6	3.57
12.14	4.12
12.25	4.20
12.33	4.29
1.21	4.39
1.32	4.47
1.42	4.58
1.53	5. 6
2. 0	5.17
2.11	5.28
2.26	5.35
2.35	5.42
2.51	5.52

This was her last visit. During the whole day no other wasp found the honey. I also tried other wasps, concealing the honey in the same manner, and with a similar result.

I have no doubt some wasps would make even more journeys in a day than those recorded above.

Power of distinguishing Colours.

As regards colours, I satisfied myself that wasps are capable of distinguishing colour, though they do not seem so much guided by it as bees are.

July 25. At 7 A.M. I marked a common worker wasp (*V. vulgaris*) and placed her to some honey on a piece of green paper 7 inches by 4½. She worked with great industry, as recorded on p. 506. After she had got well used to the green paper, I moved it 18 inches off, putting some other honey on blue paper,

where the green had previously been. She returned to the blue. I then replaced the green paper for an hour, after which I moved it 18 inches as before, and put brick-red paper in its place. She returned to the brick-red paper. But although this experiment indicates that this wasp was less strongly affected by colours than the bees which I had previously observed, still I satisfied myself that she was not colour-blind.

I moved the green paper slightly and put the honey, which, as before, was on a slip of plain glass, about 4 feet off. She came back and lit on the green paper, but finding no honey, rose again, and hawked about in search of it. After 90 seconds I put the green paper under the honey, and in 15 seconds she found it. I then, while she was absent at the nest, moved both the honey and the paper about a foot from their previous positions, and placed them about a foot apart. She returned as usual, hovered over the paper, lit on it, rose again, flew about for a few seconds, lit again on the paper, and again rose. After 2 minutes had elapsed, I slipped the paper under the honey, when she almost immediately (within 5 seconds) lit on it. It seems obvious, therefore, that she could see green.

I then tried her with red. I placed the honey on brick-red paper, and left her for an hour, from 5 P.M. to 6, to get accustomed to it. During this time she continued her usual visits. I then put the honey and the coloured paper about a foot apart; she returned first to the paper and then to the honey. I then transposed the honey and the paper. This seemed to puzzle her. She returned to the paper, but did not settle. After she had hawked about for 100 seconds I put the honey on the red paper, when she settled on it at once. I then put the paper and the honey again 18 inches apart. As before, she returned first to the paper, but almost immediately went to the honey. In a similar manner I satisfied myself that she could see yellow.

Again, on August 18th I experimented on two wasps, one of which had been coming more or less regularly to honey on yellow paper for four days, the other for twelve—coming, that is to say, for several days, the whole day long, and on all the others, with two or three exceptions, for about three hours in the day. Both therefore had got well used to the yellow paper. I then put blue paper where the yellow had been, and put the yellow paper with some honey on it about a foot off. Both the wasps returned to the honey on the blue paper. I then moved both the papers

about a foot, but so that the blue was somewhat nearer the original position. Both again returned to the blue. I then transposed the colours, and they both returned to the yellow.

Very similar results were given by the wasp watched on the 11th of September. After she had made twenty visits to honey on blue paper, I put it on yellow paper and moved the blue 12 inches off. She came back to the yellow. I then put vermilion instead of yellow; she came back to the vermilion. I transposed the colours; she came back to the vermilion.

I put white instead of vermilion;					she returned to the blue.
„ green	„	white	„	„	„
„ orange	„	green	„	„	„
I transposed the colours			„	„	orange.
I put white instead of orange			„	„	white.
„ green	„	white	„	„	blue.
„ purple	„	green	„	„	purple.
„ orange	„	purple	„	„	orange.
„ green	„	orange	„	„	green.
I transposed the colours			„	„	blue.
„	„		„	„	green.

So far therefore she certainly showed no special predilection for the blue. I then left her the rest of the day to visit honey on blue paper exclusively. She made fifty-eight visits to it. The following morning I opened my window at 6.15, when she immediately made her appearance.

I let her make ten more visits to the honey on blue paper, moving it about a foot or so backwards and forwards on the table. I then put orange paper instead of the blue, and put the blue about a foot off. She returned to the orange.

I put yellow instead of orange;					she returned to the yellow.
„ vermilion	„	yellow	„	„	vermilion.
„ white	„	vermilion	„	„	white.
„ green	„	white	„	„	green.
I transposed the colours;			„	„	blue.
I now put vermilion instead of green, and moved both of them a foot, but so that the vermilion was nearest the window, though touching the blue;			„	„	vermilion.

Again, September 11, I marked a wasp. She returned to the

honey over and over again with her usual assiduity. The following morning I put the honey on green paper; she came backwards and forwards all day. On the 13th I opened my window at 6.8, and she came in immediately. During an hour she made ten journeys. On her leaving the honey for the eleventh time, I placed some honey on vermilion paper where the green had been, and put the honey and the green paper about a foot off.

She returned at

7.25 to the vermilion. I then put orange instead of vermilion.
7.34 „ orange „ blue „ orange.
7.40 „ blue „ white „ blue.
7.47 „ white „ yellow „ white.
7.55 „ yellow and then to the green. I transposed the colours.
8. 2 „ green. I then moved both colours about a foot, but so that the yellow was a little nearer to the old place.

She returned at 8.9 to the yellow.

I then removed the yellow paper and honey, and placed the honey which had been on the green paper about a foot from it on the table.

At 8.15 she returned and lit on the green paper, but immediately flew off to to the honey. I then transposed the honey and the paper.

At 8.24 she returned and again lit on the paper, but immediately flew off to the honey.

Thus, therefore, though it is clear that wasps can distinguish colours, they appear, as might be expected from other considerations, to be less guided by it than is the case with bees.

Direction of Flight.

Every one has heard of a "bee-line." It would be no less correct to talk of a wasp-line. On the 6th of August I marked a wasp, the nest of which was round the corner of the house, so that her direct way home was not out at the window by which she entered, but in the opposite direction, across the room to a window which was closed. I watched her for some hours, during which time she constantly went to the wrong window and lost much time in buzzing about at it. Aug. 7, I was not able to watch her. Aug. 8th and 9th, I watched her from 6.25 A.M., when she made her first visit. She still constantly went to the closed window. Aug. 10th and 11th, I was away from

home. Aug. 12th, she made her first visit at 7.40, and still went to the closed window. Aug. 13th, her first visit was at 6.15; she went to the closed window and remained buzzing about there till 7, when I caught her and put her out at the open one by which she always entered. Aug. 15th and 16th she continued to visit the honey, but still always, even after ten days' experience, continued to go to the closed window, which was in the direct line home; though on finding it closed, she returned and went round through the open window by which she entered.

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Observations on the Habits of Ants, Bees, and Wasps.—Part IV.

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

(Plate XVII.)

Ants.

In my last paper on this subject (Journ. Linn. Soc., Zool. vol. xii. p. 445) I recorded some experiments showing the singular reluctance of Ants to let themselves drop even for a very short distance, and their want of ingenuity in bridging over chasms. Since then I have varied the experiments in the following manner.

Want of ingenuity in crossing Chasms.

I filled a saucer (woodcut, fig. 1, S) with water and put in it a block of wood (W), on the top of which I fastened a projecting wooden rod (B), on the end of which I placed a shallow glass cell (A P) containing several hundred larvæ. From this cell I allowed a slip of paper to hang down to within $\frac{3}{10}$ of an inch of the upper surface of the artificial nest (N). At one side I put another block of wood (C) with a lateral projection (D) which hung over the cell containing the larvæ. I then made a connexion between D and A, so that ants could ascend C, and, passing over D, descend upon the larvæ. I then put some specimens of *Lasius niger* to the larvæ, and soon a large number of ants were engaged in carrying off the larvæ. When this had continued for about three hours, I raised D $\frac{3}{10}$ of an inch above A. The ants kept on coming and tried hard to reach down from D to A, which was only just out of their reach. Two or three, in leaning over, lost their foothold and dropped into the larvæ; but this was obviously an accident; and after a while they all gave up their efforts and went away, losing their prize, in spite of most earnest efforts, because it did not occur to them to drop $\frac{3}{10}$ of an inch.

Fig. 1.

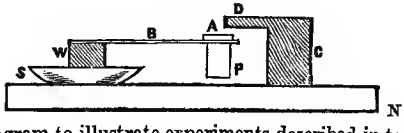


Diagram to illustrate experiments described in text.

At the moment when the separation was made there were fifteen ants on the larvæ. These could, of course, have returned if one had stood still and allowed the others to get on its back. This, however, did not occur to them; nor did they think of letting themselves drop from the bottom of the paper on to the nest. Two or three, indeed, fell down, I have no doubt, by accident; but the remainder wandered about, until at length most of them got into the water. After a time the others abandoned altogether as hopeless the attempt to get at the larvæ.

I waited about six hours, and then again placed the glass (A) containing the larvæ so as to touch the piece of wood (D), and again put some ants to the larvæ. Soon a regular string of ants was established; when I again raised the wood (D) $\frac{3}{10}$ of an inch above the glass (A), exactly the same result occurred. The ants bent over and made every effort to reach the larvæ, but did not drop themselves down, and after a while again abandoned all hope of getting the larvæ.

Experiments testing Intelligence.

In order to test their intelligence, it seemed to me that there was no better way than to ascertain some object which they would clearly desire, and then to interpose some obstacle which a little ingenuity would enable them to overcome. Following up, then, the preceding observations, I placed some larvæ in a cup which I put on a slip of glass surrounded by water, but accessible to the ants by one pathway in which was a bridge consisting of a strip of paper $\frac{2}{3}$ inch long and $\frac{1}{3}$ inch wide. Having then put a *Formica nigra* from one of my nests to these larvæ, she began carrying them off, and by degrees a number of friends came to help her. I then, when about twenty-five ants were so engaged, moved the little paper bridge slightly, so as to leave a chasm, just so wide that the ants could not reach across. They came and tried hard to do so; but it did not occur to them to push the paper bridge, though the distance was only about $\frac{1}{3}$ inch, and they might easily have done so. After trying for about a quarter of an hour, they gave up the attempt and returned home. This I repeated several times.

Then, thinking that paper was a substance to which they were not accustomed, I tried the same with a bit of straw 1 inch long and $\frac{1}{8}$ inch wide. The result was the same. I repeated this twice.

One day (Oct. 27th, 1876) I put some provisions in a shallow box with a glass top and a single hole on one side; I then put some specimens of *Lasius niger* to the food, and soon a stream of ants was at work busily carrying supplies off to the nest. When they had got to know the way thoroughly, and from thirty to forty were so occupied, I poured some fine mould in front of the hole so as to cover it up to a depth of about $\frac{1}{2}$ an inch. I then took out the ants which were actually in the box. As soon as the ants had recovered from the shock of this unexpected proceeding on my part, they began to run all round and about the box, looking for some other place of entrance. Finding none, however, they began digging down into the earth just over the hole, carrying off the grains of earth one by one and depositing them without any order all round at a distance of from $\frac{1}{2}$ to 6 inches, until they had excavated down to the doorway, when they again began carrying off the food as before.

This experiment I repeated on the following days three or four times, always with the same result.

I also tried the same experiment with another species, viz. *L. flavus*, and with the same result.

As to power of Communication.

In my previous paper I have recorded various experiments made with the view of ascertaining whether, when ants have found a store of food, they are able to describe the route to their companions. The following also seems to me instructive. I put an ant (*L. niger*) to some larvæ as usual, and when she knew her way, I allowed her to go home on her own legs; but as soon as she emerged from the nest, if she had any friends with her, I took her up and carried her to the larvæ. Under these circumstances very few ants indeed found their way to them. Thus, June 22, at 5.30, an ant which had been previously under observation was put to some larvæ. She took one and returned as usual to the nest. At 5.34 she came out with no less than 10 friends, and was then transferred to the larvæ. The others wandered about a little, but by degrees returned to the nest, not one of them finding their way to the larvæ. The single one above referred to picked up a larva, returned, and again came out of the nest at 5.39 with eight friends, when exactly the same thing happened. She again came out with companions at the undermentioned times:—

Hour.	Number of friends.	Hour.	Number of friends.
5.44	4	6.44	—
5.47	4	6.46	3
5.49	—	6.49	2
5.52	—	6.56	—
5.54	5	6.59	—
5.57	2	7. 2	2
5.59	2	7. 4	—
6. 1	5	7. 6	3
6. 4	1	7. 8	3
6. 7	—	7.10	5
6.11	3	7.13	—
6.14	4	7.17	3
6.17	6	7.19	7
6.20	—	7.21	5
6.23	5	7.24	—
6.25	6	7.26	3
6.29	8	7.29	1
6.32	2	7.31	2
6.35	—	7.35	—
6.42	4		

Thus during these two hours more than 120 ants came out of the nest in company with the one under observation. She knew her way perfectly; and it is clear that if they had been left alone, all these ants would have accompanied her to the store of larvæ. Three of them were accidentally allowed to do so; but of the remainder, only five found their way to the larvæ; all the others, after wandering about a while, returned hopelessly to the nest.

One of the ants which I employed in my experiments was under observation several days. I was, however, away from home most of the day, and when I left in the morning and went to bed at night I put her in a bottle; but the moment she was let out she began to work again. On one occasion I was away for a week, and on my return I let her out of the bottle, placing her on a little heap of larvæ about 3 feet from the nest. Under these circumstances I certainly did not expect her to return. However, though she had thus been six days in confinement, the brave little creature immediately picked up a larva, carried it off to the nest, and, after half an hour's rest, returned for another.

Individual Influence or Character.

Another point of considerable interest is the difference in individual character or influence which seems to be indicated by some of the experiments.

For instance, on the 21st of June, at 6 minutes past noon, a worker of *F. nigra* was put to some larvæ; she carried off one as usual and returned as follows, viz. at

12. 6	12.19	12.30	12.47
12. 8	12.21	12.33	12.51
12.10	12.23	12.36	
12.15	12.26	12.40	
12.17	12.28	12.44	

During this time only two other ants came to the larvæ. We then imprisoned the ant and put another to the same larvæ at 2.56. This ant already knew her way well, and she returned as follows, viz.:—

2.58	3.11	3.25	3.52
3. 1	3.13	3.27	3.57
3. 4	3.15	3.30	
3. 6	3.20	3.34	
3. 9	3.22	3.36	

During this time no other ant came. We then imprisoned her again, and put a third ant to the same larvæ. She returned at

4.20	4.30	4.46	5. 6
4.23	4.36	4.56	5.10
4.26	4.40	5. 0	
4.28	4.42	5. 2	

when we left off watching her. Between 4.20 and 4.40 this ant brought 10 friends with her; but it is curious that from 4.40 to the end of the observation no more came. I have often observed that when an ant first begins to work, she brings many more friends than afterwards.

Intelligence and Affection.

As evidence both of their intelligence and of their affection for their friends, it has been said by various observers that when ants have been accidentally buried they have been very soon dug out and rescued by their companions. Without for one moment doubting the facts as stated, we must remember the habit which

ants have of burrowing in loose fresh soil, and especially their practice of digging out fresh galleries when their nests are disturbed.

It seemed to me, however, that it would not be difficult to test whether the excavations made by ants under the circumstances were the result of this general habit, or really due to a desire to extricate their friends.

With this view I tried the following experiments :—

(1) I placed (Aug. 20) some honey near a nest of *Lasius niger* on a glass surrounded with water, and so arranged that in reaching it the ants passed over another glass covered with a layer of sifted earth, about one third of an inch in thickness. I then put some ants to the honey, and by degrees a considerable number collected round it. Then at 1.30 P.M. I buried an ant from the same nest under the earth, and left her there till 5 P.M., when I uncovered her. She was none the worse, but during the whole time not one of her friends had taken the least notice of her.

(2) Sept. 1. I arranged some honey again in the same way. At 5 P.M. about 50 ants were at the honey, and a considerable number passing to and fro. I then buried an ant as before, taking of course one from the same nest. At 7 P.M. the number of ants at the honey had nearly doubled. At 10 P.M. they were still more numerous, and had carried off about two thirds of the honey. At 7 A.M. the next morning the honey was all gone, two or three were still wandering about, but no notice had been taken of the prisoner, whom I then let out. In this case I allowed the honey to be finished, because I thought it might perhaps be alleged that the excitement produced by such a treasure distracted their attention, or even (on the principle of doing the greatest good to the greatest number) that they were intelligently wise in securing a treasure of food before they rescued their comrade, who, though in confinement, was neither in pain nor danger. So far as the above ants, however, are concerned, this cannot, I think, be urged.

(3) On the 8th Sept. I repeated the experiment, burying some ants at 4 P.M. Up to 6.3 no attempt had been made to release them. I let them out and buried some more. The next morning, at 7 A.M., the honey was all gone, some ants were still wandering about, but no notice had been taken of the captives, whom I then liberated.

(4) I then (Aug. 21) made exactly the same experiment with *Myrmica ruginodis*, as representing the other great family of

ants. At 2.30 I buried one as before under about ¼ of an inch of fine earth. A great many of her friends were passing continually over her head, but not one of them took any notice of her till 7 P.M., when I let her out.

(5) About a month later, on Sept. 10, I again tried the same experiment, imprisoning some at 10.15 A.M. Up to 4.30 they had not been released. I then let them out, and buried some more. The next morning the honey was all consumed, but some of the ants were still searching about. The prisoners, however, were still in durance.

But even if their friends who are in difficulty are actually in sight, it by no means follows that their companions will assist them.

Of this I could give almost any number of cases. Thus on one occasion several specimens of *Formica fusca* belonging to one of my nests were feeding on some honey spread on a slip of glass (May 22). One of them had got thoroughly entangled in it. I took her and put her down just in front of another specimen belonging to the same nest, and close by placed a drop of honey. The ant devoted herself to the honey and entirely neglected her friend, whom she left to perish.

Again, some specimens of *Cremastogaster scutellaris* were feeding quietly (May 22) on some honey spread on a slip of glass, and one of them had got thoroughly mixed in it. I took her out and put her on the glass close by. She could not disentangle herself; not one of her friends took the least notice of her, and eventually she died. I then chloroformed one and put her on the board among her friends. Several touched her, but from 12 to 2.30 P.M. none took any particular notice of her *.

I thought, however, that it would be desirable to make some systematic observations on the subject. The results were as follows.

Sept. 10, at 6 P.M., a number of *Lasius flavus* from one of my captive nests were out feeding on some honey. I chloroformed four of them and also four from a nest in the park, at some distance from the place where the first had been originally procured, and put them close to the honey. Up to 8.20 the ants had taken no notice of their insensible fellow creatures. At 9.20 I found that

* Dead ants, I may add, are always brought out of the nest, and I have more than once found a little heap on one spot, giving it almost the appearance of a burial-ground.

four friends were still lying as before, while the four strangers had been removed. Two of them I found had been thrown over the edge of the board on which the honey was placed. The other two I could not see.

Again, on the 14th Sept., at 8.40, I put in the same way four friends marked white, and four strangers marked red, close to where my *L. flavus* were out feeding on honey placed on a slip of glass over water. For some hours they took no notice of them. At length one took a friend, and after carrying her about some time, at 12.40, dropped her into the water. Some time after another took up a stranger and carried her into the nest at 2.35. A second stranger was similarly carried into the nest at 2.55, a third at 3.45, while the fourth was thrown over the edge of the board at 4.20. Shortly after this two of the strangers were brought out of the nest again and thrown away. A second friend was thrown away, like the first, at 4.58, the third at 5.17, and the fourth at 5.46. I could not ascertain what happened to the last stranger, but have little doubt that she was brought out of the nest and thrown away like the rest.

On the following day at 6.45 I tried the same experiment again, only marking the friends red and the strangers white. At 7 one of the strangers was carried off and dropped over the edge of the glass into the water, and at 8 a second. At 8.45 a friend was taken up and, after being carried about some time, was thrown into the moat. At 9.45 a friend was picked up and carried into the nest, but brought out again and throwu away about 3 in the afternoon. The other four remained where they were placed until 8 P.M., and though the other ants often came up and examined them, they did not carry them off.

Sept. 29. Again placed nine chloroformed ants, five friends and four strangers, close to where a number were feeding. There was a continual stream of ants to the honey, ten or fifteen being generally there at once.

A stranger	was picked up	at	10.20	and dropped at	10.32
,,	,,	,,	10.22	,,	10.35
A friend	,,	,,	11.22	,,	11.42
A stranger	,,	,,	11.35	,,	11.50
,,	,,	,,	11.41	,,	11.45

Shortly after the others were picked up and carried away to the edge of the board, where they were dropped, but none were taken into the nest.

Oct. 2. Again at 10 A.M. placed ten chloroformed ants, five friends and five strangers, close to where some were feeding. They were picked up and carried off as before in the following order:—

At 11. 5	a stranger	was picked up and dropped at	11.15
11.12	a friend	„ „	11.50
11.25	a stranger	„ „	11.36
12. 7	„	„ „	12.45
12.10	a friend	„ „	12.16
1.10	a stranger	„ „	2. 6
1.42	a friend	„ „	1.46
1.52	„	„ „	1.56
2. 6	„	„ „	3.10

Only one of them, and that one a stranger, was carried into the nest at 12.45, but brought out again at 1.10.

Oct. 6. At 9 A.M. again tried the same experiment with four strangers and five friends.

At 9.25	a friend	was picked up and dropped at	9.31
9.32	„	„ „	9.38
9.35	a stranger	„ „	9.45
9.45	a friend	„ „	9.52
10. 8	a stranger	„ „	10.17
10.17	a friend	„ „	10.20
10.22	a stranger	„ „	10.25
10.28	„	„ „	10.40
10 25	a friend	„ „	10.31

None of them were carried into the nest.

These experiments seem to prove that under such circumstances ants, at least those belonging to this species, do not carry their friends (when thus rendered insensible) off into a place of safety.

It may, however, be said that in this experiment, the ants being to all intents and purposes dead, we could not expect that any difference would be made between friends and strangers. I therefore repeated the same experiment, only instead of chloroforming the ants I intoxicated them. This experiment is more difficult, as it is not in all cases easy to hit off the requisite degree of intoxication. The numbers therefore of friends and strangers are not quite the same, because in some cases the ants recovered too quickly and had to be removed. In such cases I have latterly replaced the ant so removed by another, so as to keep the number of friends and strangers about equal. I must make more obser-

vations; but so far as they have gone they are as follows. The sober ants seemed somewhat puzzled at finding their intoxicated fellow creatures in such a condition, took them up, and carried them about for a time in a somewhat aimless manner.

Nov. 20. I experimented with six friends and six strangers, beginning at 11.

At 11.30 a friend was carried to the nest.

11.50 a stranger was dropped into the water.

12.30 „ „ „

12.31 a friend „ „

1.10 a stranger „ „

1.18 „ „ „

1.27 „ „ „

1.30 a friend (partly recovered) was taken to the nest.

2.30 „ was taken up and carried about till 2.55; she was then taken to the nest, but at the door the bearer met two other ants, which seized the intoxicated one, carried her off, and eventually dropped her into the water.

At 3.35 a friend was carried to the nest.

Out of these 12, 5 strangers and 2 friends were dropped into the water; no stranger but 3 friends were taken to the nest. None of the friends were brought out of the nest again.

Nov. 22. Experimented in the same way on four friends and four strangers, beginning at 12.

At 12.16 a stranger was taken and dropped into the water.

12.21 „ „ „

12.23 „ „ „

12.40 „ „ „

I then put 4 more strangers.

3.10 a stranger was taken and dropped as before.

3.30 „ „ „

3.35 „ „ „

3.44 a friend (partly recovered) was taken back to the nest.

4.10 a stranger was taken and dropped into the water.

4.13 a friend (partly recovered) was taken back to the nest.

In this case 8 strangers were dropped into the water, and none were taken to the nest; 2 friends, on the contrary, were taken to the nest, and none were dropped into the water.

Dec. 1. Experimented with five friends and five strangers, beginning at 2.15.

At 2.30 a stranger was dropped into the water.

3. 2 „ „ „

At 3.20 a friend was taken into the nest.
3.35 a stranger „ „
3.52 „ „ „
4. 5 I put out four more friends and as many strangers.
4.45 a stranger was dropped into the water.
5.10 „ taken into the nest.
5.24 „ „ „
5.55 a friend was thrown into the water.
6. 4 a stranger „ „
6. 4 „ „ „
6. 8 a friend was taken into the nest.
6.20 „ „ „
6.23 „ „ „
6.30 a stranger was dropped into the water.
6.50 a friend „ „ „
8. 5 a friend was taken into the nest.

In this case 2 friends were thrown into the water and 7 taken into the nest; while 6 strangers were thrown into the water and 3 were taken into the nest (*all of these, however, were afterwards brought out again and thrown away*).

Dec. 8. Experimented with six friends and six strangers, beginning at 11.30.

At 11.30 a friend was carried to nest.
11.47 „ „
11.50 „ „
11.52 „ „
11.56 a friend was dropped into water.
11.58 a stranger „ „
11.58 „ „ „
12 a stranger was carried to nest.
12. 2 „ „ „
12. 3 „ „ „

I then put four more of each, replacing her by another as each was carried off.

At 12.42 a friend to water.
12.58 a stranger to water.
1 a friend to nest.
1 „ „
1 „ „
1.58 „ „
1.59 „ „
2.30 a stranger to water.
2.30 „ „
2.35 a stranger to nest.

At 2.42 a stranger to water.
2.48 „ „
2.51 „ „
2.52 „ „
2.55 a friend to nest.
2.55 a stranger to water.
2.55 „ „
3. 2 a friend to water.
3. 6 a stranger to water.
3.12 a friend to water.

At 3.15 a friend to water.	At 3.25 a friend to nest.
3.16 a friend to nest.	3.35 a stranger to water.
3.22 a stranger to water.	3.50 a friend to nest.
3.25 „ „	3.50 „ „

All these ants appeared quite insensible; 16 friends were then taken to the nest and 5 to the water, while of the strangers only 3 were taken to the nest, while 15 were thrown into the water. Moreover, as in the preceding observation, even the three strangers which were at first taken to the nest were soon brought out again and thrown away; while this was not the case with any of the friends as far as we could ascertain, though we searched diligently for them also. In this case also all the intoxicated ants were motionless and apparently insensible.

Jan. 15. Repeated the same experiment, beginning at 12.20. Up to 7 P.M. not one of the intoxicated ants had been moved. At 8.20 we found a stranger in the water, at 9.30 another, and at the following morning a third. The others were untouched.

Jan. 17. Repeated the same experiment, beginning at 11.30.

At 12 a friend was carried to the nest.
12.20 a stranger was carried to the water.
12.34 a friend was carried to the nest.
12.40 a stranger was carried to the water.
12.45 a friend was carried to the nest.
1 a stranger „ „
1 „ „ „ water.
(Stopped observing till 2.)
2.30 a stranger was carried to the water.
2.30 „ „ nest.
4.10 „ „ „
4.30 a friend „ „
6.20 a stranger was carried to the water.
6.35 „ „ „

Thus, then, the general results were that the ants removed thirty-eight friends and forty strangers. Of the friends, twenty-seven were carried into the nest and seven were thrown into the water. Of the strangers, on the contrary, thirty were thrown into the water; only nine were taken into the nest, and seven of these were shortly afterwards brought out again and thrown away. Indeed I fully believe that the other two were treated in the same manner, though we could not satisfy ourselves of the fact. But it was only by very close observation that the seven were detected, and the other two may well have escaped observation.

Tabular View.—Experiments on Ants under Chloroform and Intoxicated.

Chloroformed Ants.

	Friends.			Strangers.		
	To nest.	To water.	Unremoved.	To nest.	To water.	Unremoved.
Sept. 10...	.....	...	4	.....	4	
„ 14...	.....	4	...	2 and brought out again.	2	
„ 15...	1 and brought out again.	1	...	.....	2	2
„ 29...	.....	5	...	.....	4	
Oct. 2...	.....	5	...	1 and brought out again.	4	
„ 6...	.....	5	...	.....	4	
	1	20	4	3	20	2
	Intoxicated Ants.					
Nov. 20...	3	2	...	.....	5	1
„ 22...	2	...	2	.....	8	

In these cases some of the Ants had partly recovered; in the following they were quite insensible.

Dec. 1...	7 none brought out again.	2	...	3 all these brought out again.	6	
„ 8...	16 none brought out again.	5	...	3 all these brought out again.	15	
Jan. 15...	...	...	4	.....	3	1
„ 17...	4 none brought out again.	...	...	3 one brought out again.	6	
	27	7	4	9	30	1

Recollection of Friends.

In my previous paper* I recorded some facts tending to show not only that ants belonging to the same nest know one another, but also that they recollect one another after being separated for some months.

This was made evident by separating a colony into halves, registered as Nests No. 4 and 5, and then from time to time introducing an ant from one division into the other. As the nests under observation consisted of a thin stratum of earth between two glass plates, I was able to see exactly how the ant thus introduced behaved herself, and how she was treated by the others.

One of the colonies thus separated belonged to *Formica fusca*, and was divided on the 4th Aug., 1875. The observations made in the same year have been already recorded. On the 15th March following, I put in a stranger and one of the old companions from the other half of the nest at 7 A.M., and watched them longer than those previously experimented on. The stranger was very soon attacked; the friend seemed quite at home.

4th June. 8 A.M. Put into the nest a stranger and an old friend. The stranger was at once attacked, and dragged about by one of her antennæ. 9 A.M. The stranger was being attacked; the friend, though not attacked, kept rather away from the other ants. 10.30 A.M. The stranger was attacked, not the friend. 12.30 P.M. do., 1 P.M. do., 1.30 P.M. do., 2 P.M. do., 2.30 P.M. do. 4 P.M. do., 4.30 P.M. do. 5 P.M. The stranger was dead.

5th June. Put in a stranger and a friend at 9.30. At 10 the stranger was being attacked, not the friend. 10 A.M. do., 10.30 A.M. do.

At 11 I put in another stranger and another old friend, when nearly the same thing was repeated. At 11.30 A.M. the stranger was being dragged about by an antenna; the friend was not attacked. 12. The stranger was by herself in a corner of the nest. The friend was almost cleaned from the paint by which she was marked. I therefore put in another friend. At 2 the stranger was being dragged about by an antenna, the friend was being cleaned. 2.30 do., 3 do. At 3.30 the friend was almost clean; the stranger is being dragged about. 6 do.

10th June. Repeated the same observation at 10 A.M., but transposed the colours by which they were distinguished, so that

* See vol. xii. p. 494, lines 17 and 18 from the top, and under the head of Nov. 7, I unfortunately transposed the words "former" and "latter."

there might be no question whether perhaps the difference of treatment was due to the difference of colouring. At 11 A.M. the friend was all right, the stranger was being dragged about by an antenna. 11.30 A.M. the friend all right, the stranger being dragged about by one leg. 12 do. 12.30 P.M. the friend all right, the stranger being dragged about by an antenna. 1 P.M. do., 2 P.M. do., 3 P.M. do.

3rd July. Put in a friend and a stranger at 11 A.M. At 11.30 A.M. the stranger was being dragged about, the friend was being cleaned. 12 do. 12.30 A.M. both were now being attacked. 1 do.

This seems to show that some, at least, of the ants have forgotten their old friends.

16th July. Put in two friends at 7.45 A.M. At 8 A.M. each was being dragged about by an antenna. 8.30 A.M. one was being dragged about by both antennæ, the other by both antennæ and one leg. 10 A.M. both were still attacked, but it is curious that at the same time others were cleaning off the paint. 12.30, both still attacked.

17th July. Put in a friend at 8.15 A.M. At 8.30 they were cleaning her. At 9 A.M. she was almost clean. 9.30 she seemed quite at home, and had only one spot of paint on her. 10.20 do.

20th July. Put in a friend and stranger at 9 A.M. At 9.30 A.M. the friend seemed all right; the stranger was in a corner by herself. At 10 A.M. the friend was being cleaned; the stranger had come out of her corner and was being fiercely attacked. At 11 A.M. the friend seemed quite at home and was almost cleaned; the stranger was being dragged about, but was almost cleaned. At 12 the same thing was going on, and also at 12.30. At 1.30 the stranger was still being pulled about; but what struck me as remarkable, the friend also had hold of one of the ants by an antenna. At 2 P.M. the friend was by herself, the stranger was being attacked. At 4 P.M. the friend again had hold of an ant by an antenna; the stranger was being pulled about. At 5 the friend seemed quite at home in the nest; the stranger was killed. The following morning I was still able to distinguish the friend; she seemed quite at home.

6th Aug. Put in a stranger and a friend at 8 A.M. At 8.30 both were attacked. 9 do., 9.30 do., 10 do., 11 do., 12.30 do.

6th Aug. Repeated the experiment at 2. Both ants hid themselves in corners. At 3.30 the stranger was being attacked; the friend was in a corner by herself. At 4.30 both were attacked. 5.30 do.

7th Aug. Put in a stranger and a friend at 8.30 A.M. At 8.45 both were being attacked. 9.30 do., 10 do.

Aug. 8. Put in a friend at 7 A.M. At 8 she seemed quite at home with the others. At 9 they had almost cleaned her. 9.30 she seemed quite at home with the others. 10 do.

12th Aug. Put in a friend and a stranger at 7 P.M. Both were immediately attacked. 7.15 they were being dragged about. 7.45 do, 8 do., 8.15 do.

13th Aug. Put in a friend at 6.30 A.M. At 7.50 two attacked her. At 8 she was being attacked by one ant, but another was cleaning her. 8.15 do. 8.45. Two were attacking her, one dragging at her by an antenna. 9 do., 9.30 do., 10 do., 10.30 do. Others had almost entirely cleaned off the paint.

At 5 P.M. put a friend and a stranger into the other nest. At 5.15 the friend seemed quite at home, and had been nearly cleaned; the stranger was being attacked. 5.30 do., 8.15 do. 7.15. Two of the ants were dragging the stranger out of the nest; the friend had been quite cleaned.

14th Aug. At 8.15 A.M. I put an ant from each half of the nest into the other. At 8.30 one was alone in the corner, the other was being attacked. At 9 both were being attacked. 9.30 do., 10.30 do.; 11.30 do., both, however, being almost cleaned.

Aug. 19. At 8 A.M. I put into each nest one from the other. The one was received amicably and cleaned, so that I lost sight of her. It was clear, however, that she was received in a friendly manner, because no fighting was going on. At 11 I put into the same nest another friend: at 11.30 she was all right, and, being cleaned at 12, I could no longer distinguish her.

The ant put into the other nest was not so well received. At 9.30, 12.30, and 11.30 she was being dragged about, but she was also being cleaned, and after 11.30 I lost sight of her.

Aug. 21. At 10.15 I again put into each nest an ant from the other. One was at once cleaned, and I could not find her. I should, however, certainly have seen her if she had been attacked.

The other was at first attacked by one of the ants; but this soon ceased, and they began to clean her. By 11.30 she was quite at her ease among the other ants and almost clean. After 12 I could not see her any more. At 1.40 P.M. I again put into each nest an ant from the other, accompanied, however, in both cases by a stranger. The contrast was most marked, and no one who saw it could have doubted that the friends and strangers were re-

spectively recognized as such, or that they themselves were fully aware of their position.

In the first nest the friend at once joined the other ants, who began to clean her. The stranger ran about in evident alarm, was pursued by the others, and took refuge in a corner. At 2 the friend was with the other ants, the stranger alone in a corner. At 2.25 the friend was almost cleaned, and after 2.30 we could no longer distinguish her: the stranger was still alone. At 3.40 she came out of her hiding-place and was attacked; after a while she escaped from the nest. At 5.30 she met one of the ants, and a battle at once began. I separated the combatants and put the stranger back near her own nest, which she at once entered, and where she was soon cleaned by her own friends.

I will now describe the adventures of the other couple. The friend immediately joined the other ants; the stranger was hunted about and soon seized. At 2 the friend was all right, the stranger being dragged about. At 2.30 ditto. The stranger was soon killed. The friend, whom I watched at intervals till 6.30, continued on the best terms with the others; it was quite clear, therefore, that they did not regard her as a stranger. She herself was not afraid of, and did not avoid them. Still for some time she apparently wished to return home. She came out of the nest and tried to find her way home to her own nest. I put her back again, however, and by the evening she seemed to have accustomed herself to the change. I opened the door of the nest soon after 5; but she showed no wish to leave her newly acquired friends.

Sept. 1. At 11 A.M. I again put into each nest an ant from the other and a stranger. In the one nest the friend joined the other ants, and seemed quite at home; the stranger, on the contrary, endeavoured to conceal herself, and at length, at 4 in the afternoon, escaped from the nest.

In the other division the friend also appeared quite at home. The stranger, on the contrary, endeavoured to escape, but in the course of the afternoon was attacked and killed.

Oct. 15. At 8 A.M. I repeated the same experiment. In the first nest, up to 10 A.M., neither ant was attacked; and it is curious that the stranger was licked and, indeed, almost cleaned. Soon afterwards, however, the ants began to attack her, and at 3 P.M. she was dead, the friend, on the contrary, being quite at home. Still the following day at midday I found her out of the nest (all the rest being within). This almost looks as if, though safe, she

did not feel at home; and I accordingly put her back to the other nest, which she at once entered.

In the other division the friend was soon nearly cleaned, and the stranger partly so. The friend seemed quite at home. At 12.30 the stranger was being dragged about by three ants; but after this I lost sight of her.

Nov. 10. At 11.30 put into one of the divisions a friend and a stranger. At 12 the friend was all right, the stranger was being dragged about by an antenna. From this time till 7 P.M. the stranger was continually being dragged about or held a prisoner, while the friend was quite at home.

Nov. 11. At 10.15 I put into the other division a friend and a stranger. At 11 the friend was quite at home, and the colour with which I had marked her had been almost cleaned off. The stranger, on the contrary, was being dragged about by two of the ants. After this, however, I could not find her. She had, I think, escaped from the nest.

Nov. 12. I therefore, the following day at 11.30, again put a friend and a stranger into this division of the nest. The friend seemed quite at home. One of the ants at once seized the stranger by an antenna and began dragging her about. I will give this observation in detail out of my note-book.

At 11.45. The friend is quite at home with the rest; the stranger is being dragged about.

At 12. The friend is all right. Three ants now have hold of the stranger by her legs and an antenna.

At 12.15, 12.30, 12.45, and at 1 similarly occupied.

At 1.30 similarly engaged. One now took hold of the friend, but soon seemed to find out her mistake and left go again.

At 1.45. The friend is all right. The stranger is being attacked. The friend also has been almost cleaned, while on the stranger the colour has been scarcely touched.

At 2.15. Two ants are licking the friend, while another pair are holding the stranger by her legs.

At 2.30. The friend is now almost clean; so that I could only just perceive any colour. The stranger, on the contrary, is almost as much coloured as ever. She is now near the door and, I think, would have come out, but two ants met her and seized her.

At 3. Two ants are attacking the stranger. The friend was no longer distinguishable from the rest.

At 3.30, 3.40, and 5 engaged as above.

At 6.0. The stranger now escaped from the nest, and I put her back among her own friends.

The difference of behaviour to these two ants was therefore most marked.

The friend was gradually licked clean, and except for a few moments, and that evidently by mistake, never attacked. The stranger, on the contrary, was not cleaned, was at once seized, was dragged about for hours with only a few minutes' interval, by one, two, or three assailants, and at length made her escape from the nest at the time when no other ant was out.

Dec. 11. At 10 A.M. I again put in a friend and a stranger. The friend was not attacked, and consorted peaceably with the rest. I found her again all right on the following morning. The stranger, on the contrary, was soon attacked and killed.

Dec. 22. Repeated the same experiment. The stranger was attacked and driven out of the nest. The friend was received quite amicably.

Dec. 26. Ditto. The friend was received as usual. I lost sight of the stranger, who probably escaped.

Dec. 31. Ditto. In this case the stranger, after being dragged about some time in the nest, made her escape. But even outside, having met with an ant accidentally, she was viciously attacked.

Jan. 15. Ditto.

Jan. 16. I put in two friends; but thinking the preceding experiments sufficient, I did not on this occasion add a stranger. Neither of the friends was attacked.

Jan. 19. Put in two friends at 11 A.M. Neither was attacked, and the following morning they were all right amongst the rest.

Jan. 22. Put in three friends, with the same result.

Jan. 24. „ two „ „

Jan. 26*. „ three „ „

These details are, I fear, tedious, but they may be worth giving, because a mere statement of the general facts without particulars would not convey so clear an idea of the result. The following table shows it in a condensed form:—

* Since this paper was read, I have continued these observations, viz.:—On Feb. 11 put in two friends, on Feb. 12 three, on Feb. 13, 15, and 19, and on March 11 and 12, one friend, on March 18 two, on April 21 one, and on April 22 and 23 two friends; but in none of these instances were the friends attacked

Experiments with Ants of different Nests.

Separation of Nest, Aug. 4, 1875.

	F. fusca.		*Myrmica ruginodis.*	
	Friend.	Enemy.	Friend.	Stranger.
8 Aug. ...	.....	Attacked.		
13 „ ...	.....	„		
16 „ ...	Not attacked.			
20 „ ...	.....	Attacked.		
22 „ ...	Not attacked.	„		
3 Sept. ...	„	Not attacked.	Not much watched.	
17 „ ...	„	„		
3 Oct. ...	„	„		
15 Mar. ...	„	Attacked.	3 Oct.	Not attacked. Attacked.
4 June ...	„	Killed.	18 „	„ Killed.
5 „ ...	„	Attacked.	19 „	„ „
5 „ ...	„	„	20 „	„ „
10 „ ...	„	Attacked and killed.	24 „	„ „
				„ „
3 July ...	Attacked.	Attacked.	31 „	„ „
10 „ ...	„	.....	7 Nov.	„ „
16 „ ...	„			
16 „ ...	„			
17 „ ...	Not attacked.			
17 „ ...	„			
20 „ ...	„	Attacked and killed.		
6 Aug. ...	Attacked.	Attacked.		
6 „ ...	„	„		
7 „ ...	„	„		
8 „ ...	Not attacked.			
12 „ ...	Attacked.	„		
13 „ ...	„			
13 „ ...	Not attacked.	„		
14 „ ...	Attacked?			
14 „ ...	„ ?			
19 „ ...	„			
21 „ ..	Not attacked.			
21 „ ...	„			
21 „ ...	„			
21 „ . .	„	Attacked and killed.		
1 Sept. ...	Not attacked.	Ran away.		
1 „ ...	„	Attacked.		

	F. fusca.	
	Friend.	Enemy.
15 Oct. ...	Not attacked.	„
15 „ ...	„	„
10 Nov. ...	„	„
11 „ ...	„	„
12 „ ...	„	„
11 Dec. ...	„	„
22 „ ...	„	Escaped from the nest.
26 „ ...	„	Not attacked.
31 „ ...	„	Attacked.

Some further observations on *F. fusca*, viz. on January 15th, when a stranger was attacked, whereas twice on the 16th, twice on the 19th, thrice on the 22nd, twice on the 24th, and thrice on the 26th of the same month, all resulted in a friend not being attacked*.

I must, however, point out one thing which must be taken into consideration. As I sometimes transposed ants from one division of the nest to the other, it might be said that some of the friends were among those which had been brought more recently from the other half of the nest.

Of the ants thus transposed, there were, however, altogether, in nest No. 5, only thirteen, in nest No. 4 twenty-three, some of which, moreover, must certainly have been among those that died. After the beginning of November all the transfers were made from No. 4 into No. 5. Since December last thirty-one ants have been so transferred; even assuming then that I had unluckily hit upon all the transferred ants (which is of course little short of an impossibility), eight of them, at any rate, had not been in nest No. 5 since August 1875.

Thus, then, for more than a year these ants remembered their old companions, as is shown by the fact that they received them amicably while they attacked strangers. It is surprising that the ants of a nest should all know one another; but that this should be still the case after more than a year's separation seems to me not the least marvellous point connected with them.

* The following cases have been added since the reading of this paper:—Feb. 11 two friends, Feb. 12 three, Feb. 13, 15, and 19, and March 11 and 12, one friend, March 18 two friends, April 21 one, and April 22 and 23 two friends, none of which when introduced were attacked.

Contrast in Behaviour of different kinds of Ants.

The behaviour of *Lasius flavus* offers a surprising contrast to that of *F. fusca*. I was anxious to see whether the colonies of this species, which are very numerous round my house, were in friendly relations with one another. With this view, I kept a nest of *L. flavus* for a day or two without food and then gave them some honey, to which they soon found their way in numbers. I then put in the midst of them an ant of the same species from a neighbouring nest; the others did not attack, but, on the contrary, cleaned her—though, from the attention she excited and the numerous communications which took place between her and them, I am satisfied that they knew she was not one of themselves. After a few minutes she accompanied some of the returning ants to the nest. They did not drag nor apparently guide her; but she went with them quite freely. This I repeated several times with the same result.

I then took four ants, two from a nest about 500 yards from the first in one direction, the other from an equal distance in another. In all cases the result was the same. I then got a few from a colony about half a mile off. These also were most amicably received, and in every case the stranger went of her own accord to the nest. One of the strangers was, indeed, dragged about half way to the entrance of the nest, but was then left free and might have run away if she had liked. She, however, after wandering about for about half a minute, voluntarily entered the nest. In one or two cases the stranger ran as quickly and straight to the nest as if she had been there over and over again. This, I suppose, can only have been by scent; and certainly no hounds in full cry could have pursued their game more directly or with less hesitation. In other cases, however, they were much longer before they went in. To satisfy myself that these facts were not owing to the nest having been taken from that of colonies or allies, I subsequently procured some ants of the same species from a nest in Hertfordshire; and they also behaved in a similar manner. In one or two cases they seemed to be attacked, though so feebly that I could not feel sure about it; but in no case were the ants killed.

The following fact surprised me still more. I put an ant (Aug. 31) at 9 A.M. on a spot where a number of *F. flava* (belonging to one of my nests of domesticated ants) had been feeding some hours previously, though none were there, or, indeed, out at all, at the

moment. The entrance to the nest was about 8 inches off; but she walked straight to it and into the nest. A second wandered about for four or five minutes and then went in; a third, on the contrary, took a wrong direction, and, at any rate for three quarters of an hour, did not find the entrance.

Very different is the behaviour of *L. niger* under similar circumstances. I tried the same experiment with them. There were no communications with the antennæ, there was no cleaning; but every ant which the stranger approached flew at her like a little tigress. I tried this experiment four times; each stranger was killed and borne off to the nest.

Suspected Cannibalism.

Ants have even been suspected of cannibalism by some writers *, because those which are found dead are generally more or less shrivelled, and the large females even are reduced almost to an empty shell. Huber's statement that their affection for their queens induces them to brush and lick them for days after death, has even been regarded as evidence of cannibalism rather than of affection. On this point, however, further evidence is required.

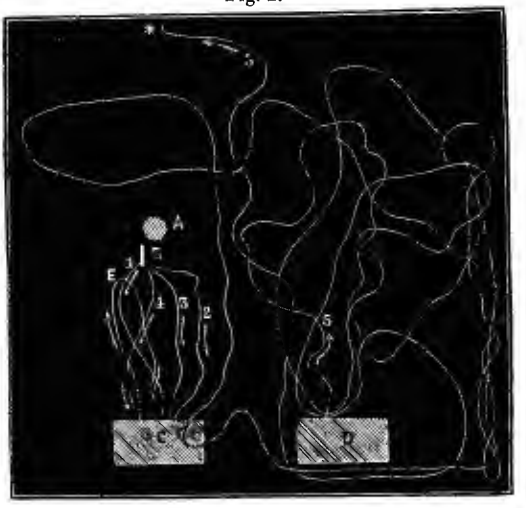
Experiments Testing the Senses. Sight.

In order to test how far they are guided by sight, I made various observations and experiments, the accompanying woodcuts being reduced copies of tracings of some of the tracks made by the ants during the course of the observations. I shall commence with experiment No. 2.

Exp. 2.—Feb. On the parade-ground (see fig. 2) I placed upright a common cylindrical lead-pencil $\frac{1}{4}$ inch in diameter and 7 inches long, fastened with sealing-wax to a penny-piece. Close to the base of the pencil (A) I brought the end of a paper bridge (B) leading to the nest, and then placed a shallow glass with larvæ at C, 4 inches from the base of the pencil. I then put an ant to the larvæ; when she had become acquainted with the road, she went very straight, as is shown in the woodcut (fig. 2). In one case, at the point E, she dropped her larva and returned for another. When she returned on the next journey and was on the glass, I moved it 3 inches, to D, so that the end of the glass was 6 inches from the base of the pencil. If she were much guided by sight, then she would have little or no difficulty in finding her way

* See, for instance, an interesting communication by Mr. Elwin, Sci. Gossip, Nov. 1870, p. 243.

Fig. 2.



Routes followed in experiment No. 2, as detailed above.

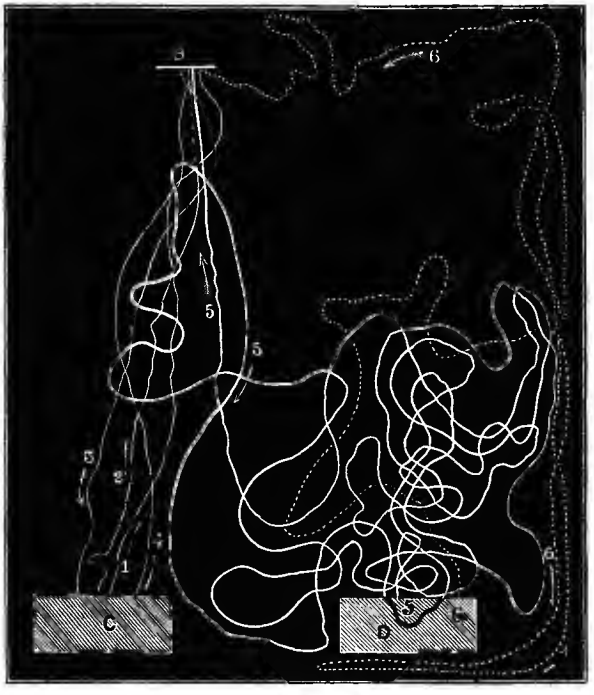
A, position of pencil. B, paper bridge. C and D, glass with larvæ. E, point where larva dropped, the opposite arrow and loop marking return route. 1, 2, 3, 4, comparatively straight paths to the glass. 5, 5, circuitous route on shifting of glass. * different access to nest.

back. Her pathway, however, which is traced on the paper, shows that she was completely abroad; and, after all, she got back to the nest by a different route (5).

I then varied the experiment as subjoined, and as shown in the woodcut (fig. 3).

*Exp.*3.—I connected the parade-ground with the nest by a paper bridge, the end of which is shown at B (fig. 3), and which came down about an inch from the pole supporting the nest. This pole rises 18 inches above the parade-ground. I then put the glass tray (C) with larvæ as before, 12 inches from the base of the pole, and put an ant to the larvæ. When she had learnt her way I traced four of her routes, as shown in the thin lines 1, 2, 3, 4. I then on her next journey (5, thick white line), when she was on the tray (C), moved it three inches to D, as shown in the figure, and again traced her routes. The contrast is very striking between the relatively straight thin white lines 1, 2, 3, 4 of the four journeys when familiar with the road; whereas in the broad white line No. 5 the zigzag twistings show how much difficulty the ant

Fig. 3.



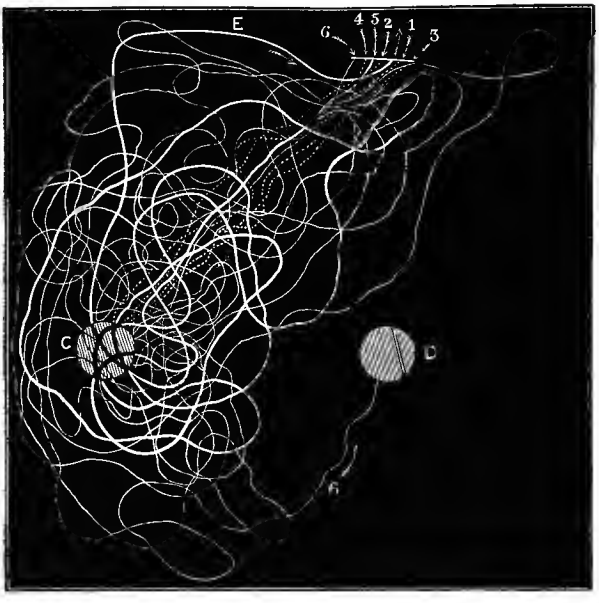
Routes followed in experiment No. 3, as mentioned in text.

B, paper bridge. C, glass tray with larvæ, its first position; and D its position when shifted. 1, 2, 3, 4, thin white lines indicating the comparatively straight routes. 5, thick white line, and 6, dotted line showing tortuous paths when glass had been altered in position. The arrows indicate directions travelled.

experienced in finding her way. Again the dotted sinuous white line (6) shows the course adopted on a subsequent journey.

Exp. 4.—I then again varied the experiment as follows:—I placed the larvæ in a small china cup on the top of the pencil, which thus formed a column $7\frac{1}{2}$ inches high. The cross line close to the arrows (fig. 4) is as before, the base of the paper bridge going to the nest. C shows the position of the penny on which the pencil is supported. The dotted white lines 1, 2, 3, 4 show the routes of a marked ant on four successive journeys from the nest to the base of the pencil. I then moved the pencil 6 inches to D, and the two following routes are marked 5 and 6. In one of them, 5 (thick white line), the ant found a stray

Fig. 4.



Routes followed in experiment No. 4, as described in text.

Cross line at the six arrows represents paper bridge going to nest. C, china cup on top of pencil. D, pencil moved. E, where stray larvæ were found. 1, 2, 3, 4, dotted lines show the nearly direct journeys. 5, thick white line (crossing C in black) of route returning to nest E, being position of larva in the course. 6, very circuitous thin white line of track from nest to pencil D.

larva at E, with which she returned to the nest, without finding the pencil at all. On the following journey, shown in fine white zigzag line (6), she found the pencil at last, but, as will be seen, only after many meanderings.

Exp. 5.—I then repeated the observation on three other ants (see figs. 5–7) with the same result: the second was 7 minutes before she found the pencil, and at last seemed to do so accidentally; the third actually wandered about for no less than half an hour, returning up the paper bridge several times.

Other experiments somewhat similar to the preceding, the results of which are shown in the figures 6 and 7, seem to prove that this species of ant, at any rate, guides itself but little by sight. This, which I had not at all anticipated, seems to follow from the fact that after the pencil and tray of larvæ had been

Fig. 5.

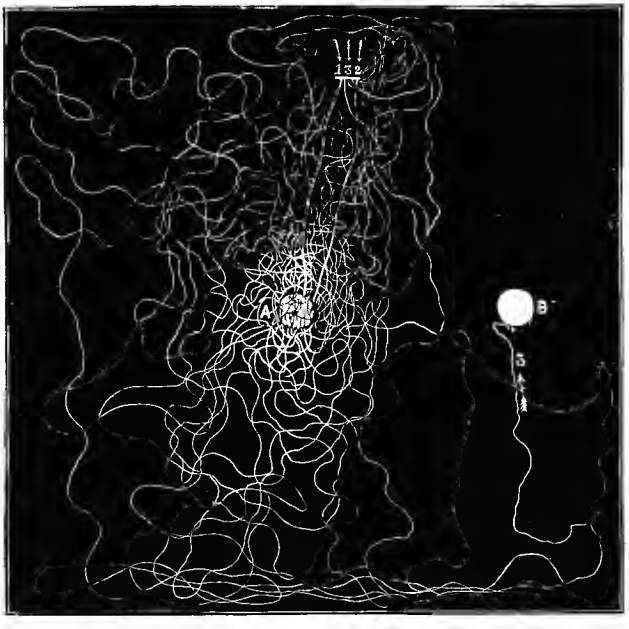


Diagram of complex path traversed in experiment 5.

A, first position of pencil. B, second position of pencil. 1, 2, straight lines of two tracks of the observed ant. 3, winding narrow white line, showing course pursued by the same ant before arriving at B, when the position of the pencil was unchanged.

removed but a short distance to the right or left, the ants on their journey to the shifted object travelled very often backwards and forwards and around the spot where the coveted object first stood. Then they would retrace their steps towards the nest, wander hither and thither from side to side between the nest and the point A, and only after very repeated efforts around the original site of the larvæ reach, as it were, accidentally the object desired at B.

Another evidence of this consists in the fact that if when *L. niger* were carrying off larvæ placed in a cup on a piece of board, I turned the board round so that the side which had been turned towards the nest was away from it, and *vice versâ*, the ants always returned over the same track on the board, and, in consequence, directly away from home.

If I moved the board to the other side of my artificial nest, the

Fig. 6.

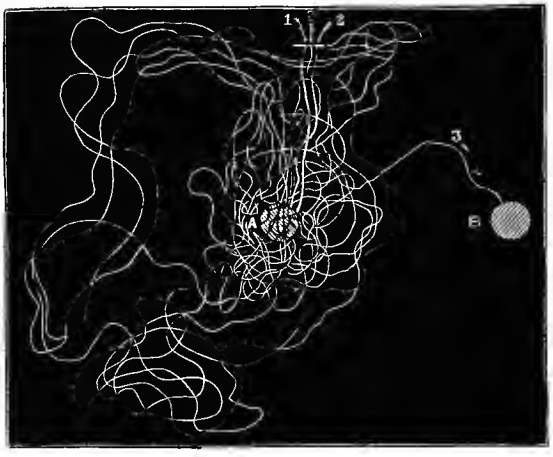


Diagram representing three tracks of an ant in another experiment.

A, the first position of pencil and the food, towards which and from the base-line of nest 1 and 2 lead by nearly direct broadish white lines to A. When the latter was removed to B the ant, in its effort to reach this, pursued the narrow white winding line ending in 3 ⟹

result was the same. Evidently they followed the road, not the direction.

I may here note that the diagrams figs. 2–7 are careful reductions of large tracings made during the experiments. Though not absolutely correct in every minute detail of contour, they are exact for all practical purposes. As the ants pursued their way, pencil-markings in certain instances, and coloured lines in others, were made so as to follow consecutively the paths pursued.

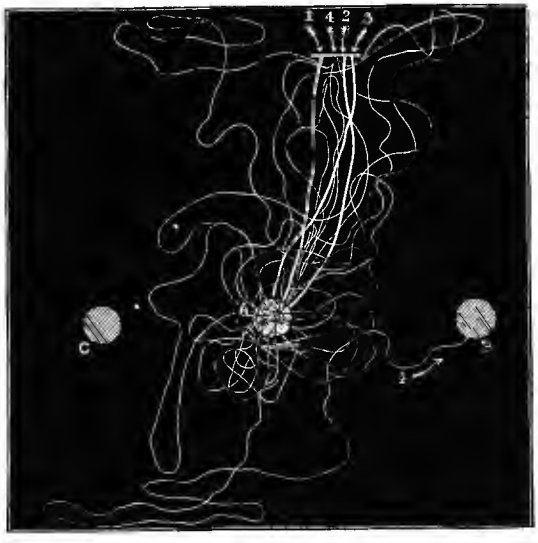
Hearing.

As regards their sense of hearing, I have in my previous paper recorded my unsuccessful experiments in this direction. Approaching an ant which was standing quietly, I have over and over again made the loudest and most shrill noises I could—using a penny pipe, a dog-whistle, a violin, as well as the most piercing and startling sounds I could produce with my own voice, but without effect. At the same time I by no means would infer from this that they are really deaf, though it certainly seems that their range of sounds is very different from ours. We know that certain

allied insects produce a noise by rubbing one of their abdominal rings against another. Landois is of opinion that ants also make sounds in the same way, though these sounds are inaudible to us. Our range is, however, after all very limited, and the universe is probably full of sounds which we cannot perceive. There are, moreover, in the antennæ of ants certain curious organs which may be of an auditory character.

These consist of three parts, a small spherical cup, opening to the outside, a long narrow tube, and a hollow body, shaped

Fig. 7.



Another tracing showing a similar Experiment. 1, 2, 3, the direct broad lines towards A; and 4, the complicated track made when reservoir of larvæ was removed to B.

like an elongated clock-weight. They are about 10 in number, and may serve to increase the resonance of sounds, acting, in fact, to use the words of Prof. Tyndall, who was good enough to look at them with me, like microscopic stethoscopes. Several of the other segments of the antenna also contain one of these curious organs.

Dependence on Slaves.

Huber mentions that the Amazon ants (*Polyergus rufescens*) are

so dependent on their slaves as to perish in two or three days if separated from them. That this is the case, has been shown by subsequent observers. It is no use giving them food—say honey; they will not touch it. Or rather, they walk carelessly over it, smear their legs, and die, if a slave is not put in to clean and dry them. I found, however, that I could keep even a single *Polyergus* alive for more than three months by giving her a slave for about an hour a day to attend on and feed her. I have one at this moment which has been so treated since November, and which is still alive and well*.

Division of Labour.

I mentioned in my last paper that in the autumn of 1875 I noticed an ant belonging to one of my nests of *F. fusca* out feeding alone. The next day the same ant was again out by herself, and for some weeks no other ant, so far as I observed, came out to the food. I did not, however, watch her with sufficient regularity. This winter, therefore, I have kept two nests under close observation, having arranged with my daughters and their governess, Miss Wendland (most conscientious observers), that one of us should look at them once an hour during the day. One of the nests contained about 200 individuals, the other, a nest of *P. rufescens* with the usual slaves, about 400. The mistresses themselves never come out for food, leaving all this to the slaves.

We began watching on the first of November, but did not keep an hourly register till the 20th, after which date the results are given in the following tables. Table No. 1 relates to the nest of *F. fusca*, and the ants are denoted by numbers. The hours at which we omitted to record an observation are left blank; when no ant was at the honey, the square is marked with an 0. An ant, marked in my register as No. 3, was at this time acting as feeder to the community.

The only cases in which other ants came to the honey were at 2 P.M. on the 22nd Nov., when another ant came out, whom we registered as No. 4, another on the 28th, registered as No. 5. Other ants came out occasionally, but not one came to the honey (except the above mentioned) from the 28th Nov. till the 3rd Jan., when another (whom we registered as No. 6) began feeding. After this a friend visited the honey once on the 4th, once on the 11th, and again on the 15th, when she was registered as No. 7.

* April 15. She is still well.

Table No. 2 is constructed in the same way, but refers to the nest of *Polyergus* and *F. fusca*. The feeders in this case were, at the beginning of the experiment, those known to us as Nos. 5, 6, and 7. On the 22nd Nov. a friend, registered as No. 8, came to the honey, and again on the 11th Dec.; but with these two exceptions the whole of the supplies were carried in by Nos. 5 and 6, with a little help from No. 7.

Thinking now it might be alleged that possibly these were merely unusually active or greedy individuals, I imprisoned No. 6 when she came out to feed on the 5th. As will be seen from the table, no other ant had been out to the honey for some days; and it could therefore hardly be accidental that on that very evening another ant (then registered as No. 9) came out for food. This ant, as will be seen from the table, then took the place of No. 6, and (No. 5 being imprisoned on the 11th Jan.) took in all the supplies, again with a little help from No. 7. So matters continued till the 17th, when I imprisoned No. 9, and then again, *i. e.* on the 19th, another ant (No. 10) came out for the food, aided on and after the 22nd by another, No. 11. This seems to me very curious. From the 1st Nov. to the 5th Jan., with two or three casual exceptions, the whole of the supplies were carried in by three ants, one of whom, however, did comparatively little. The other two are imprisoned, and then, but not till then, a fresh ant appears on the scene. She carries in the food for a week, and then, she being imprisoned, two others undertake the task. On the other hand, in Nest 1, where the first foragers were not imprisoned, they continued during the whole time to carry in the necessary supplies.

The facts therefore certainly seem to indicate that certain ants are told off as foragers, and that during winter, when little food is required, two or three are sufficient to provide it.

Tables illustrating Experiments on Division of Labour.

TABLE I.—*Slaves of* Formica fusca. Nest No. 1.

Date.	6.30.	7.	8.	9.	10.	11.	12.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Nov. 20.	0	0	0	0	0	0	0	N 3	0	0	0	0	0	0			
21...	...	N 3	0	0	0	0	0	0	Friend marked	..	...	0	0	0			
22...	0	0	N 3	0	0	N 3	N 3	0	N 4	N 4	0	0	N 4	0	...	...	0
23...	N 4	0	N 4	0	0	0	0	0	0	...	...	0	...	0			
24...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
25...	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
26...	0	0	0	0	0	0	0	...	0	...	0	0	0	0	...	...	0
27...	0	0	0	0	0	N 4	0	N 3	0	0	0	0	0	0	N 3	0	0
28...	0	0	0	0	0	0	0	N 5	0	0	N 5	N 5					
29...	0	0	N 4	0	0	0	0	N 3	0	N 3	0	0	0	0	0	0	
30...	0	0	N 3	N 3	0	0	0	0	0	0	0	0	0	0	0	0	
Dec. 1..	...	0	0	0	0	0	0	0	0	0	0	0	N 4	0	0	0	
2...	0	0	N 4	N 4	0	0	N 3, 4	0	0	0	0	0	0	N 4	N 4	0	
3...	0	0	N 4	0	0	0	0	0	0	N 3	0	0	N 3	0	N 3	N 3	0
4...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	...	0
5...	...	0	N 3	0	0	0	0	0	...	0	0	0	0	0	0	...	0
6...	...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
7...	...	0	0	0	N 4	0	0	0	0	0	0	0	...	0			
8...	0	0	0	0	0	0	0	0	0	0	0	0	0				
9...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	...	0	0

10...	...	0	0	0	0	...	...	0	0	0	0	0	0	0			
11...	0	0	0	0	0	0	0	0	0	...	0	0	0	0	0	0	
12...	0	0	0	0	0	0	0	N 3	N 4	N 3	0	0	0	0	0	N 3	
13...	N 3	0	0	0	0	0	0	0	0	N 3	0	0	0	0	0	N 3	
14...	...	0	0	0	0	0	0	0	0	0	0	0	0	0	...	0	0
15...	...	0	N 3	0	0	0	0	0	0	0	0	0	0	...	0	0	0
16...	0	0	0	0	0	0	N 3	0	0	...	0	0	0	0	...	0	0
17...	...	0	...	0	0	...	0	0	0	0	0	0	0	0	0	0	0
18...	...	0	0	0	0	0	0	0	...	0	0	0	0	0	0	0	0
19...	...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
20...	...	0	0	0	0	0	N 3	N 3	0	0	0	0	0	0	0	0	0
21...	0	0	0	0	0	0	0	0	0	0	0	N 3	0	0	0	0	0
22...	0	0	0	0	0	0	0	0	0	0	0	0	0				
23...	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
24...	0	0	0	0	0	0	0	0	0	0	N 3	0	0	0	0	0	N 3
25...	0	0	0	0	0	...	...	0	0	0	0	0	0	...	0	0	
26...	0	0	0	0	0	0	0	0	0	...	0	0	0	0	0	0	0
27...	0	0	0	0	0	0	0	0	0	0	N 3	...	0	...	0	0	N 3
28...	0	...	N 5	0	0	0	0	0	0	0	0	0	0	0	0	0	0
29...	...	...	0	0	N 3	0	0	0	0	0	...	0	0	0	0	0	0
30...	...	0	0	N 3	0	0	0	0	0	0	0	0	0	0	0	0	0
31...	...	0	0	0	0	0	0	0	0	...	0	0	0	0	0	0	0
Jan. 1...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
2...	0	0	0	0	0	0	0	0	0	...	0	0	0	0	0	0	0
3...	0	0	...	Friend N 6	0	0	0	0	0	0	0	0	0	0	0	0	0
4...	0	N 6	N 6	N 6	0	0	0	...	...	...	...	...	...	N 6	...	N 6	0
5...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
6...	N 6	0	N 6	0	0	0	0	0	0	0	...	...	...	...	N 6	0	0
7...	0	0	0	0	0	...	...	N 6	0	0	N 6	0	0	...	0	0	0

TABLE I. (*continued*).

Date.	6.30.	7.	8.	9.	10.	11.	12.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Jan. 8...	...	0	0	N 5	N5&6	0	0	0	0	N 3	0	0	0	0	N 5	0	N 5
9...	0	0	0	0	0	0	0	0	...	0	0	0	0	...	0	0	0
10...	0	0	N 3	0	0	0	...	0	N 3	0	0	0	0	0	0	0	0
11...	...	0	N 6	N 3	0	0	0	0	Friend feeding	0	0	0	0	0	0	0	0
12...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
13...	0	0	0	0	0	...	0	0	0	0	0	0	0	0	0	0	0
14...	0	N 3	0	0	0	0	0 Friend marked	0	0	0	0	0	0	0	0	0	0
15...	0	0	0	0	0	0	N 7	0	0	0	0	0	0	0	0	0	0
16...	0	0	0	N7&3	0	0	N 3	0	0	0	0	N 7	0	0	0	0	0
17...	...	0	N 7	0	0	N 7	N 7	0	0	0	0	0	N 6	0	0	0	0
18...	0	N 6	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
19...	0	0	N 6	0	0	0	0	0	0	0	0	0	0	0	0	N 6	0
20...	...	0	0	0	0	0	0	0	0	N 6	0	0	0	...	0	0	0
21...	0	N 6	0	N 3	0	...	...	0	0	0	0	0	0	0	0	0	0
22...	0	N 6	0	0	0	0	0	0	N 6	0	N 6	0	0	...	0	0	0
23...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
24...	0	0	0	0	0	0	0	0	0	0	0	0	0	...	0	0	0
25...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
26...	0	0	0	0	0	N 3	0	0	0	...	0	N 3	0	0	0	0	0
27...	...	0	0 Friend	0	0	0	0	0	0	0	0	0	0	0	0	0	0
28...	0	0	N 8	0	0	0	0	0	0	0	0	0	0	0	0	0	0
29...	0	0	N 8	0	0	0	0	0	0	N 8	0	0	0	0	0	0	0

30...	0	0	0	0	Friend, marked her N 9	0	0	0	0	0	0	0	0	0	0	0	0	0
31...	0	0	0	0	0	0	0	0	0	0	0	0	N 6	0	0	0	0	0

(With the permission of the Council I have added the following, so as to extend the observations to three months.

Feb. 1...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
2...	0	0	0	0	0	0	0	0	0	...	0	0	0	N 8	0	N 8	0	
3...	...	N 9	0	0	N 9	0	0	0	0	0	0	0	...	0	0	0	0	
4...	0	0	0	0	0	0	0	0	0	N 6	0	N 6	0	0	0	0	0	
5...	0	0	0	0	0	0	0	0	0	...	0	0	0	N 6	0	0	0	
6...	0	0	0	0	0	0	0	0	0	0	0	0	0	N 8	0	0	0	
7...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
8...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	N 8	0	0	
9...	0	0	0	0	0	0	0	0	0	0	0	0	N8&9	0	N 10	0	0	
10...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
11...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
12...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
13...	0	0	0	0	0	N 11	0	0	0	0	0	0	0	0	0	0	0	
14...	...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
15..	0	0	0	0	0	0	0	0	0	0	Friend	0	0	0	0	0	0	
16...	...	0	0	0	0	0	0	0	0	0	feeding.	0	N 8	N 9	0	N 8	0	
17...	...	0	0	0	0	...	...	0	0	0	0	0	0	0	0	0	0	
18...	0	0	0	...	..	0	0	0	0	0	0	0	0	0	0	0	0	
19 ..	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
20...	...	0	0	0	0	0	0	0	0	0	0	0	0	N 8	0	0	0	
1...	0	0	0	0	0	0	0	0	0	0								
22...	...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
23...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
24...	...	...	0	0	0	N 8	0	0	0	0	0	0	0	0	0	0	0	

TABLE II.—*Slaves of* Polyergus.

Date.	6.30.	7.	8.	9.	10.	11.	12.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Nov. 20...	N 5	0	0	0	0	0	N 5	0	0	N 5	N 5	N 5	0	N 6	N 7		
21...	0 Friend marked	0	0	N 5	0	0	N 5	0	...	...	...	0	0	0			
22...	N 8	N 5	N 7	0	0	N 5	N 7	N 5	0	0	N 5	0	N 5	0			
23...	0	0	N 5	0	0	0	0	0	0	...	...	0	...	0			
24...	0	N 5	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
25...	0	N 5	0	0	N 5	0	N 5	N 5	N 5	N 5	N 5	0	0	0			
26...	0	0	0	0	0	0	0	...	0	...	0	N 6	0	0	...	...	N 6
27...	0	N 6	N 5	0	N 5	0	N 5	N 5	N 6	N 5	0	N 6	0	N 5	0	N 6	N 6
28...	0	0	0	0	0	0	0	N 5	0	0	N 5	N 5					
29...	0	0	N 5	0	0	0	0	0	0	0	0	0	0	0	0	0	
30...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	N 6	0	
Dec. 1...	...	0	0	0	0	0	N 6	0	0	0	N 6	0	0	N 6	N 6	0	
2...	0	N 6	0	0	0	N 6	N 6	0	0	N 6	0	0	0	N 6	0	N 6	0
3...	0	0	0	0	0	...	N 7	0	0	N 6	0	0	0	0	0	...	0
4...	0	N 6	0	0	0	0	0	0	0	0	N 6	0	0	0	0	0	0
5...	...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	...	0
6...	...	0	0	0	0	0	0	0	...	0	0	0	0	0	0	0	0
7...	0	0	0	0	0	0	0	0	0	0	0	0	...	0			
8...	0	N 6	N 6	N 6	0	0	N 6	N 6	0	0	N 6	0	N 6				
9...	N 6	N 6	0	0	0	0	0	0	...	0	0	N 6	0	0	...	0	0
10...	...	0	0	0	0	...	...	0	0	0	0	0	0	0			
11...	0	0	0 Friend.	0	0	0	0	0	...	0	0	0	0	N 6	0	0	

12...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	N 6
13...	N 6	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
14...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	...	0	0
15...	...	0	0	0	0	0	0	0	0	0	0	N 6	0	...	0	0	0
16...	0	0	0	0	0	0	0	0	0	...	0	0	0	0	...	0	0
17...	...	0	...	0	N 6	0	0	N 7	0	0	0	0	N 7	0	0	0	0
18...	...	0	0	0	0	0	0	0	...	0	0	0	0	0	0	0	0
19...	...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
20...	...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
21...	N 6	0	N 6	N 6	0	0	N 6	0	0	0	N6&5	0	N 5	N5&6	0	N5&6	N 5
22...	0	0	0	0	N 6	0	0	N 5	N 5	0	N 5	0	0				
23...	0	0	N 5	0	0	0	0	N 5	0	0	0	N 5	0	0			
24...	0	0	N 5	0	0	0	N 5	0	0	0	0	N 5	0	0	N 5	0	0
25...	0	0	0	N 5	0	...	...	N 5	0	0	0	N 5	0	...	0	0	
26...	0	0	0	N 5	0	0	N 5	0	0	N 5	0	0	0	0	0	0	0
27...	0	0	0	0	0	0	0	0	0	0	0	...	0	0	0	0	0
28...	0	0	N 6	0	N 5	0	0	0	0	0	0	0	N 6	0	N 6	0	0
29...	...	...	0	N 6	0	N 6	0	0	0	N 6	0	N 5	0	0	N 5	0	0
30...	...	N 5	N 5	0	0	0	N 5	N 5	0	0	0	0	N 5	0	0	0	0
31...	...	N 5	0	0	0	0	0	0	0	0	N 6	0	0	0	0	0	0
Jan. 1...	0	0	0	0	0	0	0	0	0	0	0	0	N 6	0	0	0	0
2...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
3...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
4 ..	0	0	N 6	N 6	0	N 6	0	...	...	...	...	...	...	0	0	0	0

TABLE II. (*continued*).

Date.	6.30	7.	8.	9.	10.	11.	12.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Jan. 5...	0	0	0	0	0	0	0	Impris. N 6	0	0	0	0	0		0	0	0
6...	0	0	0	0	0	0	0	0	0	0	...	...	...	0	0	Friend feeding	0
7...	0	0	0	0	0	...	...	0	0	0	0	0	0	0	0	0	0
8...	...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
9...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
10...	Friend marked N 9	0	0	0 Impris.	0	0	...	0	0	0	0	N 9	N 7	N 9	0	N5&9	0
11...	...	0	0	N 5	0	0	0	0	0	0	0	0	0	0	0	0	0
12...	0	N 9	0	0	0	0	0	0	0	0	0	0	N 9	0	0	0	N 9
13...	0	N 9	0	0	0	...	0	0	0	0	0	0	0	0	0	0	N 9
14...	0	0	0	0	0	0	0	0	0	0	N 9	0	0	0	0	N 9	0
15...	0	0	0	0	0	0	0	0	0	0	0	N 9	0	N 9	0	N 9	0
16...	N 9	0	0	0	0	0	0	0 Impris.	0	0	0	N 9	N 9	N 9	0	0	N 9
17...	...	0	0	0	0	0	0	N 9	0	0	0	0	0	0	0	0	0
18...	0	0	0 Friend marked	0	0	0	0	0	0	0	0	0	0	0	0	0	0
19...	0	0	N 10	0	0	0	N 10	0	N 10	0	N 10	0	0	0	0	0	0

20...	...	0	0	0	0	0	0	0	0	0	0	N 7	0 Impris.	...	0	0	0
21...	0	0	0	0	0	...	...	0	0 Friend. marked	0	0	0	N 7	...	N 10	0	0
22...	0	0	0	0	0	0	0	0	N 11	0	0	0	0	N 11	0	0	N 11
23...	0	0	N 11	0	0	0	0	0	0	0	0	N 11	0	Friend marked N 12	N 10	0	0
24...	N 11	0	0	0	0	0	0	0	0	0	N 11	0	0	..	N 11	0	0
	I now put back No. 7.																
25...	0	N 11	0	N 7	0	0	N 11	0	0	0	N 11	0	N 11	N 11	0	0	0
26...	0	0	0	0	0	0	0	0	0	...	0	0	N 11	0	0	0	0
27...	...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	I put back Nos. 5 and 6.																
28...	N 11	0	0	0	0	0	0	0	0	0	0	0	N 5	0	0	N 12	0	0
29...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
30...	0	N 6	0	0	N 6	0	N 6	0	N 6	0	0	N 6	0	0	0	0	0	0
31...	0	0	0	0	0	0	0	0	0	0	0	0	N 6	0	0	0	0	0

With the permission of the Council I have in this case also added the following, so as to extend the observations to three months.

TABLE II (*continued*).

Date.	6.30	7.	8.	9.	10.	11.	12.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Feb. 1...	0	0	0	0	0	0	0	0	0	0	0	0	N 11	0	N 6	0	0
2...	0	0	0	0	0	0	0	0	0	...	0	0	N 11	0	0	0	0
3...	...	0	0	0	0	0	0	0	0	0	0	0	N 6	...	0	0	0
4...	0	0	0	0	0	0	0	0	0	0	N 6	0	0	0	0	0	0
5...	0	0	0	0	0	0	0	0	0	...	0	0	0	0	0	0	N 11
6...	0	N 11	0	0	0	0	N 11	0	0	0	0	0	0	0	0	0	0
7...	0	0	N 11	0	0	0	0	N 11	0	0	N 11	N12&6	N 7	N 11	0	0	N 6
8...	N 11	0	0	N 12	N 11	0	N 12	0	N 11	0	0	0	0	N 7	0	0	0
9...	0	N 11	0	0	N 11	0	N 11	0	0	0	0	0	0	0	N 12	0	0
10...	0	0	N 11	0	0	0	0	0	0	0	0	0	0	0	0	0	N 11&12
11...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
12...	0	N 11	0	N 11	0	N 11	0	0	N 11	0	N 11	0	0	0	N 10	N 11	0
13...	0	0	0	0	N 11	N 12	0	0	0	0	0	0	N 11	0	N 10	0	0
14...	...	N 11	0	0	0	0	0	0	0	0	0	0	N 11	0	0	0	N 11
15...	0	0	0	0	0	0	0	0	N 11	0	0	0	0	0	0	0	0
16...	...	0	N 11	0	0	0	0	0	0	0	0	0	0	0	0	N 11	0
17...	..	0	0	0	0	0	0	...	0	0	0	0	0	0	0	N 11	0
18...	0	0	0	0	0	...	...	0	0	0	0	0	0	0	0	N 11	0
19...	0	0	N 11	0	N 11	0	0	N 11	0	0	0	0	0	0	0	0	0
20...	...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
21...	...	0	0	0	0	0	0	0	0								
22...	...	0	0	0	0	0	0	...	0	0	0	0	0	0	0	0	0
23...	...	0	0	0	0	0	0	0	0	0	...	0	0	0	0	0	0
24...	...	...	0	0	0	0	0	0	0	0	0	0	0	N 12	0	0	0

Parthenogenesis in Ants.

Although the workers rarely lay eggs, still they do so occasionally, among ants as well as among bees and wasps. In the two latter groups these virgin eggs always produce drones; and the same will probably be found to be the case with ants also. I have a nest of *Formica cinerea* which I brought from Castellamare in December 1875, and which has no queen: nevertheless eggs were laid in it last spring, and these eggs produced winged individuals only, all, I believe, males; but unfortunately they emerged one day when I was away from home, and I lost the opportunity of examining them carefully. None of the eggs, however, produced workers.

Parasites of, and on, Ants.

The curious blind Woodlouse (*Platyarthrus Hoffmanseggii*) is very common in ants' nests in my neighbourhood. I have, however, never seen the ants take the slightest notice of them. Moreover, when my ants migrate from one nest to another, if the nests are at a little distance apart, the *Platyarthri* remain behind. I am disposed to think that they are mere scavengers.

On the 14th of October I observed that one of my ants had a mite attached to the underside of its head. The mite was almost as large as the head, and must have been very inconvenient. The ant could not remove it herself. She never came out of the nest, so that I could not do it for her; and none of her own companions from that day to this (1 Feb.) have thought of performing this kind office. I have also observed specimens of a minute red Mite, which I believe to be new, in nests of *Lasius flavus*.

Certain species of Diptera, belonging to the family Phoridæ, are also parasitic on ants. I have forwarded them to Mr. Verral, who finds that some of them are a new species of the genus *Phora* and that among them is also the type of a new genus, which he proposes to call *Platyphora*, doing me the honour of naming the species after me. I subjoin his descriptions as a separate paper or appendix to my own.

In conclusion I wish to acknowledge the valuable assistance which I have received from my wife and daughters and their governess, Miss Wendland. Without their aid I could not have carried out the continuous observations above recorded.

Having had some enlarged drawings made, for my own convenience, of several specimens of the ants which I had been

watching, it was suggested to me that figures of these same, though not new to entomologists, might nevertheless be desirable to those interested in the subject. In the Plate each figure is considerably enlarged, but the actual dimension is expressed by scale alongside.

EXPLANATION OF PLATE XVII.

Fig. 1. *Polyergus rufescens.*
2. *Formica sanguinea.*
3. —— *fusca.*
4. *Atta barbara* (worker major).
5. Do. (worker minor)
6. *Strongylognathus testaceus.*

Description of a new Genus and Species of Phoridæ parasitic on Ants. By G. H. VERRAL, Esq., Memb. Entom. Soc.

THROUGH my friend Mr. Frederick Smith, of the British Museum, Sir John Lubbock has kindly forwarded for my examination and determination certain specimens of Dipterous insects said to have been found parasitic on species of ants, which latter he has been studying with care as to their habits. Having given considerable attention to the family Phoridæ, I was agreeably surprised to find the parasitic specimens to be forms new to science. One of these is a new species of the genus *Phora*; the other I regard as possessing characters *sui generis*, and hence define it under the generic title *Platyphora*, at the same time bestowing on the species the name of the discoverer, who worthily pursues entomological researches, spite of many pressing public engagements.

The subjoined descriptions embrace the diagnostic peculiarities of the insects in question.

PHORA FORMICARUM, n. sp. Nigro-cinerea, fronte setosa, caniculata; antennis mediocribus, cinereis; palpis magnis, flavis; halteribus flavidis; pedibus totis pallide flavis, inermibus, tibiis intermediis unicalcaratis, posticis modice dilatatis; alis subhyalinis, nervo secundo simplici, nervulis vix undulatis. Long. vix $\frac{1}{2}$ lin.

Frons broad, grey, bristly, two large bristles being close to the eye-margin; down the centre is a deep impressed channel, which at its lower

end joins a channel above the antennæ, and at its upper end a channel round the raised vertical triangle; the space between these two latter channels (comprising the true *frons*) is about once and a half broader than deep; on the vertical triangle are two bristles; the third joint of the antennæ is moderately large, ovate, grey; the arista short, somewhat yellowish, almost naked; the palpi conspicuous, all pale yellow, with a few short black bristles at the tip; on the cheeks are some short black bristles.

The thorax is grey or brownish grey, broad, not much arched, the disk being nearly flat, and on the hinder part absolutely concave; on the disk there are no long bristles, but a dense clothing of rather short, black bristles; along the side of the thorax between the humeri, the base of the wing, and the scutellum are some long black bristles, and two on the thorax just before the scutellum; on each side of the scutellum are two long bristles; halteres dirty pale yellow; abdomen bare, dull black, with slightly yellowish incisures; ovipositor polished black, long, slightly incurved and grooved.

Legs pale yellow, including the coxæ, clothed with minute black bristles; all the coxæ with two or three black bristles at the tips, the legs otherwise bare excepting the spurs; femora flattened and widened, especially the hind pair, the hind tibiæ also slightly flattened and widened on the apical half; middle tibiæ with a long spur inside at the tip, and hind tibiæ with a small one inside and a very minute one outside; tarsi longer than the tibiæ, joints gradually diminishing in length.

Wings very slightly smoky, broad; second thick vein not extending half the length of the wing, thickened, but not forked at its tip; first veinlet with a steady curve; second very slightly curved at base, otherwise straight; third very slightly undulated; fourth hardly visible at base, evident towards tip, very slightly undulated; costa bristly up to end of second thick vein.

This species is readily distinguished by its simple second thick vein, channelled frons, small size, and by the absence of bristles on the tibiæ.

It is parasitic on *Lasius niger*.

Platyphora, n. gen.

Lata, plana, tota absque setis. Frons latissima. Thorax transversus. Abdomen parvus. Alarum vena cubitalis simplex, subcostali parallela; venulæ undulatæ; costa ad basin subciliata.

Distinguished from all the existing genera of Phoridæ by its flat and broad shape, which resembles that of the small species of *Sphærocera*. The absence of strong bristles on the frons, thorax,

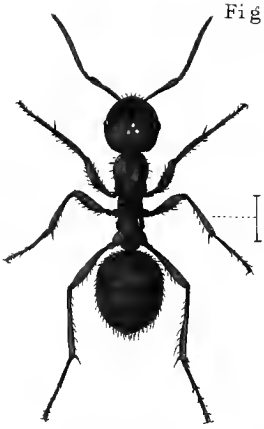
and legs also distinguishes it from all the genera except *Gymnophora*, which, however, is of the usual arched *Phora*-shape and has the cubital vein forked, costa bare, &c.

PLATYPHORA LUBBOCKII. Nigra, nitida; abdomine triangulari, segmento tertio parvo; femoribus posticis basi flavidis; alis apice latis, flavido-hyalinis, costa ad basin subciliata, vena cubitali ad medium costæ extensa subcostali parallela, venulis undulatis. Long. $\frac{2}{3}$ lin.

Broad, flat, shining; frons very broad, the eyes scarcely occupying each one sixth the width of the head; it is moderately shining, gently arched, and pretty densely clothed with minute bristles; the three ocelli visible slightly luteous; anteunæ with the third joint rather large, somewhat rounded; thorax broad, flat, rather broader than the head, angles tolerably rounded, disk shining (in appearance suggesting a small *Sphærocera*), beset with very minute bristles, which become rather scarcer towards the hinder part; scutellum rather dull, margined, nearly four times as broad as long: abdomen black, narrower and shorter than the thorax (again suggestive of *Sphærocera*); each segment after the second successively narrower, the last one being almost triangular; the third segment is very short, contracted under the second; the hind margins form a curve convex towards the thorax, the first segment being slightly emarginate in the middle; the sixth (last) is much the longest. Legs stoutish, blackish, basal two thirds of hind femora yellowish; middle tibiæ with two small spines at the tip. Wings considerably overlapping the abdomen, yellowish hyaline, darker about the basal half of the costa, blunt at the tip, cubital vein extending about half the length of the wing, and the costa slightly ciliate up to its end, subcostal vein running parallel to it and ending just before it; both veins a little thickened at their ends; first veinlet curved S-like, considerably at its base, slightly at its end, vanishing distinctly before the tip of the wing; second veinlet also S-like, diverging at its end from the first, and ending distinctly below the tip of the wing; third veinlet slightly undulated, ending very wide from the second; fourth faint, not reaching the end of the wing.

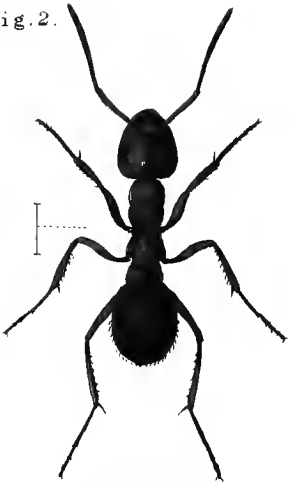
This description having been made from a specimen gummed down on card, though in very good condition, I am unable to decide on the sex, or to examine the face, palpi, base of antennæ, or coxæ.

Fig. 1.



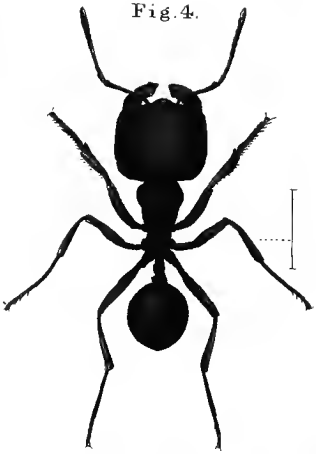
Polyergus rufescens

Fig. 2.



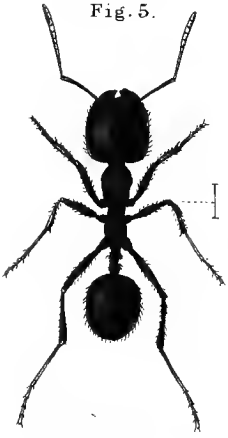
Formica sanguinea.

Fig. 4.



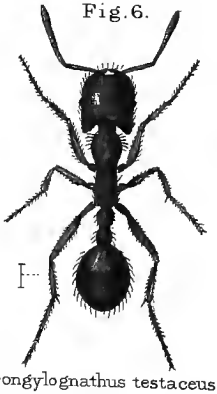
Atta barbara.
(worker major.)

Fig. 5.



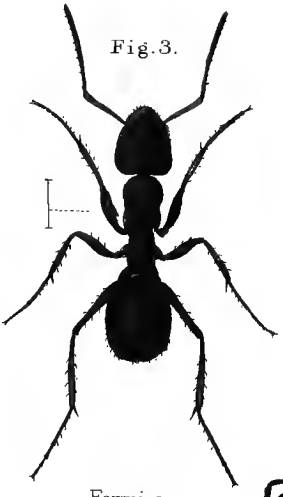
Atta barbara.
(worker minor)

Fig. 6.



Strongylognathus testaceus.

Fig. 3.



Formica fusca.

E A Smith.

Mintern Bros imp

SPECIES OF ANTS.

[*Extracted from the* Linnean Society's Journal—Zoology, vol. xiv.]

OBSERVATIONS ON

ANTS, BEES, AND WASPS.

Part V. ANTS.

BY

SIR JOHN LUBBOCK, Bart., M.P., F.R.S., F.L.S., D.C.L.,

VICE-CHANCELLOR OF THE UNIVERSITY OF LONDON.

Ants.

Recollection of Friends.

In my last paper I recorded some experiments made on a nest of *Formica fusca* which I had divided into two parts. I found that while a stranger introduced into the nest was attacked, driven out, or even killed ; and while strangers so introduced showed every sign of fear and an unmistakable desire to escape, friends, on the other hand, put back among their old companions, even after months of separation, were amicably received, and made themselves quite at home. Since my last paper I have continued the observations, as follows:—

The nest was divided on the 4th Aug. 1875.

Feb. 11, 1877. I put in two friends from the other division at 10 A.M. I looked at 10.15, 10.30, 11, 11.30, 12, 2, 4, and 6 P.M. They were on every occasion quite at home amongst the others.

Feb. 12. Put in three from the other division at 12. They were quite at home. I looked at them at 12.30, 1, 2, 4, and 6. Only for a minute or two at first one appeared to be threatened.

Feb. 13. Put in one friend from the other division. The ant was put in at 9.15 A.M., and visited at 9.30, 10, 11, 12, and 1.

Feb. 15. Do. The ant was put in at 10.15 A.M., and visited at 10.30, 11, 12, 1, 2, 3, and 4.

Feb. 19. Do. The ant was put in at 10 A.M., and visited at 10.15, 10.30, 11, 12, 1, and 2.

Mar. 11. Do. Do. at 9.30 A.M., visited at 10.30, 12.30, 2.30 and 5.30.

Mar. 12. Do. Do. at 10 A.M., visited at 12, 2, and 4.

Mar. 18. Put in two friends at 1 P.M., visited at 2 and 4.

April 21. Put in one friend at 9.30 A.M. At 10 she was all right, also at 12 and 4.

April 22. Put in two friends at 8.30. Visited them at 9 and 10, when they were almost cleaned. After that I could not find them; but I looked at 2, 4, and 6, and must have seen if they were being attacked.

April 23. Put in two friends at 12.32. Visited them at 1, 2, 3, 4, and 6. They were not attacked.

May 13. Put in two friends and a stranger at 7.45. At 9 the two friends were with the rest. The stranger was in a corner by herself. 11 do., 12 do. At 1 the friends were all right; the stranger was being attacked. 2, the friends all right; the stranger had been killed and dragged out of the nest. The next morning I looked again; the two friends were all right.

May 14. Put in the remaining three friends at 10. Visited them at 11, 12, 1, 2, 4, and 6. They were not attacked, and seemed quite at home.

This completed the experiment, which had lasted from Aug. 4, 1875, till May 14, 1877, when the last ones were restored to their friends. In no case was a friend attacked.

Though the above experiment seemed to me conclusive, I thought it would be well to repeat it with another nest.

I therefore separated a nest of *Formica fusca* into two portions on the 20th Oct. 1876.

On the 25th Feb. 1877, at 8 A.M. I put an ant from the smaller lot back among her old companions. At 8.30 she was quite comfortably established among them. At 9 do., at 12 do., and at 4 do.

June 8. I put two specimens from the smaller lot back as before among their old friends. At 1 they were all right and among the others. At 2 do. After this I could not distinguish them amongst the rest; but they were certainly not attacked.

June 9. Put in two more at the same hour. Up to 3 in the afternoon they were neither of them attacked. On the contrary, two strangers, from different nests, which I introduced at the same time, were both very soon attacked.

July 14. I put in two more of the friends at 10.15. In a few minutes they joined the others, and seemed quite at home. At 11 they were among the others. At 12 do., and at 1 do.

July 21. At 10.15 I put in two more of the old friends. At

10.30 I looked; neither were being attacked. At 11 do., 12 do., 2 do., 4 do., and 6 do.

Oct. 7. At 9.30 I put in two, and watched them carefully till 1. They joined the other ants and were not attacked. I also put in a stranger from another nest. Her behaviour was quite different. She kept away from the rest, running off at once in evident fear, and kept wandering about, seeking to escape. At 10.30 she got out; I put her back, but she soon escaped again. I then put in another stranger. She was almost immediately attacked. In the mean time the old friends were gradually cleaned. At 1.30 they could scarcely be distinguished; they seemed quite at home, while the stranger was being dragged about. After 2 I could no longer distinguish them. They were, however, certainly not attacked. The stranger, on the contrary, was killed and brought out of the nest.

This case, therefore, entirely confirmed the preceding, in which strangers were always attacked; friends were amicably received, even after a year of separation.

Thus, therefore, in these experiments, as in those previously recorded, the old acquaintances were evidently recognized. This is clear, because they were never attacked; while any ant from a different nest, even of the same species, would be set on and killed if she did not succeed in escaping from the nest. This recognition of old friends seems to me very remarkable. In one case the ants had not seen one another for more than a year.

Intelligence tested by Experiments with Honey.

To test their intelligence I made the following experiments:— I suspended some honey over a nest of *Lasius flavus* at a height of about $\frac{1}{2}$ an inch, and accessible only by a paper bridge more than 10 feet long. Under the glass I then placed a small heap of earth. The ants soon swarmed over the earth on to the glass, and began feeding on the honey. I then removed a little of the earth, so that there was an interval of about $\frac{1}{3}$ of an inch between the glass and the earth; but, though the distance was so small, they would not jump down, but preferred to go round by the long bridge. They tried in vain to stretch up from the earth to the glass, which, however, was just out of their reach, though they could touch it with their antennæ; but it did not occur to them to heap the earth up a little, though if they had moved only half a dozen

particles of earth they would have secured for themselves direct access to the food. This, however, never occurred to them. At length they gave up all attempts to reach up to the glass, and went round by the paper bridge. I left the arrangement for several weeks, but they continued to go round by the long paper bridge.

Further Test Experiments with Glycerine.

Again I varied the experiment as follows:—Having left a nest without food for a short time, I placed some honey on a small wooden brick surrounded by a little moat of glycerine about $\frac{1}{2}$ an inch wide and about $\frac{1}{10}$ of an inch in depth. Over this moat I then placed a paper bridge, one end of which rested on some fine mould. I then put an ant to the honey, and soon a little crowd was collected round it. I then removed the paper bridge; the ants could not cross the glycerine, they came to the edge and walked round and round, but were unable to get across, nor did it occur to them to make a bridge or bank across the glycerine with the mould which I had placed so conveniently for them. I was the more surprised at this on account of the ingenuity with which they avail themselves of earth for constructing their nests. For instance, wishing, if possible, to avoid the trouble of frequently moistening the earth in my nests, I supplied one of my ant-nests of *Lasius flavus* with a frame containing, instead of earth, a piece of linen, one portion of which projected beyond the frame and was immersed in water. The linen then sucked up the water by capillary attraction, and thus the air in the frame was kept moist. The ants approved of this arrangement, and took up their quarters in the frame. To minimize evaporation I usually closed the frames all round, leaving only one or two small openings for the ants, but in this case I left the outer side of the frame open. The ants, however, did not like being thus exposed; they therefore brought earth from some little distance, and built up a regular wall along the open side, blocking up the space between the upper and lower plates of glass, and leaving only one or two small openings for themselves. This struck me as very ingenious. The same expedient was, moreover, repeated under similar circumstances by the slaves belonging to my nest of *Polyergus*.

On the Origin of new Communities.

It is remarkable that, notwithstanding the labours of so many excellent observers, and though ants' nests swarm in every field and every wood, we do not yet know how their nests commence.

Three principal modes have been suggested. After the marriage-flight the young queen may either—

1. Join her own or some other old nest;

2. Associate herself with a certain number of workers, and with their assistance commence a new nest; or

3. Found a new nest by herself.

The question can of course only be settled by observation, and the experiments made to determine it have hitherto been indecisive.

Blanchard, indeed, in his work on the 'Metamorphoses of Insects' (I quote from Dr. Duncan's translation, p. 205), says:—"Huber observed a solitary female go down into a small underground hole, take off her own wings, and become, as it were, a worker; then she constructed a small nest, laid a few eggs, and brought up the larvæ by acting as mother and nurse at the same time."

This, however, is not a correct version of what Huber says. His words are:—"I enclosed several females in a vessel full of light humid earth, with which they constructed lodges, where they resided, some singly, others in common. They laid their eggs and took great care of them; and, notwithstanding the inconvenience of not being able to vary the temperature of their habitation, they reared some, which became larvæ of a tolerable size, but which soon perished from the effect of my own negligence" *.

It will be observed that it was the eggs, not the larvæ, which, according to Huber, these isolated females reared. It is true that he attributes the early and uniform death of the larvæ to his own negligence, but the fact remains that in none of his observations did an isolated female bring her offspring to maturity.

Other entomologists, especially Forel and Ebrard, have repeated the same observations with similar results; and as yet in no single case has an isolated female been known to bring her young to maturity. Forel even thought himself justified in concluding, from his observations and from those of Ebrard, that such a fact could not occur.

Lepeletier de St. Fargeau † was of opinion that ants' nests originate in the second mode indicated above, and it is, indeed, far

* 'Natural History of Ants,' Huber, p. 121.

† Hist. Nat. des Ins. Hyménoptères, vol. i. p. 143.

from improbable that this may occur. No clear case has, however, yet been observed.

Under these circumstances I made the following experiments:—

1 *a*. I took an old, fertile, queen from a nest of *Lasius flavus*, and put her to another nest of the same species. The workers became very excited and killed her.

b. I repeated the experiment, with the same result.

c. Do. do. In this case the nest to which the queen was transferred was without a queen; still they would not receive her.

d and *e*. Do. do. do.

I conclude, then, that, at any rate in the case of *L. flavus*, the workers will not adopt an old queen from another nest.

2. I took an old, fertile queen of the same species and placed her by herself with damp earth, food, and water. In a few days, however, she died.

The following, however, shows that whether or not ants' nests sometimes originate in the two former modes or not, at any rate in some cases isolated queen ants are capable of giving origin to a new community.

On the 14th Aug. 1876, I isolated two pairs of *Myrmica ruginodis* which I found flying in my garden. I placed them with damp earth, food, and water, and they continued perfectly healthy through the winter. In April, however, one of the males died, and the second in the middle of May. The first eggs were laid between the 12th and 23rd April. They began to hatch the first week in June, and the first turned into a chrysalis on the 27th; a second on the 30th; a third on the 1st July, when there were also seven larvæ and two eggs. On the 8th there was another egg. On the 8th July a fourth larva had turned into a pupa. On the 11th July I found there were six eggs, and on the 14th about ten. On the 15th one of the pupæ began to turn brown, and the eggs were about 15 in number. On the 16th a second pupa began to turn brown. On the 21st a fifth larva had turned into a pupa, and there were about 20 eggs. On the 22nd July the first worker emerged, and a sixth larva had changed. On the 25th I observed the young worker carrying the larvæ about when I looked into the nest; a second worker was coming out. On July 28 a third worker emerged, and a fourth on the 5th Aug. The eggs appeared to be less numerous, and some had probably been devoured.

This experience shows that the queens of *Myrmica ruginodis*

have the instinct of bringing up larvæ and the power of founding communities. The workers remained about six weeks in the egg, a month in the state of larvæ, and 25–27 days as pupæ.

Communication between Ants.

Every one knows that if an ant or a bee in the course of her rambles has found a supply of food, a number of others will soon make their way to the store. This, however, does not necessarily imply any power of describing localities. A very simple sign would suffice, and very little intelligence is implied, if the other ants merely accompany their friend to the treasure which she has discovered. On the other hand, if the ant or bee can describe the locality, and send her friends to the food, the case is very different. This point, therefore, seemed to me very important; and I have made a number of observations bearing on it, some of which are recorded in my previous papers read before the Society.

The following may be taken as a type of what happens under such circumstances. On June 12 I put a *Lasius niger*, belonging to a nest which I had kept two or three days without food, to some honey. She fed as usual, and then was returning to the nest, when she met some friends, whom she proceeded to feed. When she had thus distributed her stores, she returned alone to the honey, none of the rest coming with her. When she had a second time laid in a stock of food, she again in the same way fed several ants on her way towards the nest; but this time five of those so fed returned with her to the honey. In due course these five would no doubt have brought others, and so the number at the honey would have increased.

Some species, however, act much more in association than others—*Formica fusca*, for instance, much less than *Lasius niger*. I have already given an illustration of what happens when a *L. niger* finds a store of food. The following is a great contrast. On the 28th March I was staying at Arcachon. It was a beautiful and very warm spring day, and numerous ants were coursing about on the flagstones in front of my hotel.

At about 10.45 I put a *F. fusca* to a raisin. She fed till 11.2, when she went almost straight to her nest, which was about 12 feet away. In a few minutes she came out again, and returned to the fruit, after a few small wanderings, at about 11.18. She then fed till 11.30, when she returned to the nest.

At 11.45 another ant accidentally found the fruit. I imprisoned her.

At 11.50 the first returned, and fed till 11.56, when she went off to the nest. On the way she met and talked with three ants, none of whom, however, came to the fruit. At 12.7 she returned, again alone, to the fruit.

On March 29 I repeated the same experiment. There were perhaps even more ants about than on the previous day.

At 9.45 I put one (N 1) to a raisin.	At 9.50 she went to the nest.
9.55 I put another (N 2) to the raisin.	10.0 „ „
10.0 N 1 came back.	10.2 „ „
10.7 „ „	10.9 „ „
10.11 N 2 „	10.13 „ „
10.12 N 1 „	10.14 „ „
10.13 put another (N 3) to the raisin.	10.18 „ „
10.16 N 1 back.	10.17 „ „
10.22 N 2 „	10.24 „ „
(I here overpainted N 2, and she returned no more.)	
10.24 N 1 back.	At 10.26 went to the nest.
10.30 N 1 „	10.32 „ „
10.33 N 3 „	10.35 „ „
10.35 N 1 „ (She met with an accident. At first she seemed a good deal hurt, but gradually recovered.)	
10.40 N 3 back.	At 10.46 she went to the nest.
10.46 a stranger came; I bottled her.	
10.47 „ „ „	
10.52 N 1 back.	10.54 „ „
10.57 N 3 „	11.2 „ „
11.8 N 3 „	11.13 „ „
11.10 a stranger came; I removed her to a little distance.	
11.11 „ „ marked her N 4.	

11.16 N3 back.	At 11.18 went.		11.48 N1 came.	At 11.49 went.	
11.23 N4 „	11.25 „		11.49 N4 „	11.50 „	
11.24 N3 „	11.26 „		11.51 N1 „	11.53 „	
11.27 N4 „	11.29 „		11.53 N3 „	11.56 „	
11.31 N3 „	11.34 „		11.54 N4 „	11.56 „	
11.32 N4 „	11.35 „		12.0 N3 „	12.2 „	
11.40 N3 came.	11.42 „		„ N4 „	„ „	
11.40 N4 „	„ „		„ N1 „	„ „	
11.45 N3 „	11.47 „		12.5 N4 „	12.7 „	
„ a stranger came.			12.6 N3 „	12.8 „	

12.13 N3 came.	12.15 went.	12.30 N4 came.	12.32 went.
12.14 N4 „	12.15 „	„ a stranger came.	
12.17 stranger came.		„ N3 (was disturbed)	
12.19 N4 came.	12.20 „		12.37 „
12.20 N3 „	12.22 „	12.38 N4 came.	12.40 „
12.21 N1 „	12.25 „	12.42 N3 „	
12.25 N4 „	12.26 „	12.47 N4 „	12.49 „
12.27 N3 „	12.28 „		

Thus during these three hours only six strangers came. The raisin must have seemed almost inexhaustible, and the watched ants in passing and repassing went close to many of their friends; they took no notice of them, however, and did not bring any out of the nest to cooperate with them in securing the food, though their regular visits showed how much they appreciated it.

Again (on the 15th July) an ant belonging to one of my nests of *Formica fusca* was out hunting. At 8.8 I put a spoonful of honey before her. She fed till 8.24, when she returned to the nest. Several others were running about. She returned as follows:—

9.10 to the honey, but was disturbed, ran away, and returned at 10.40. At 10.53 to the nest;
„ 11.30 „ 11.40 „
„ 12.5, but was disturbed; she ran away again, but
„ 1.30. At 1.44 to the nest;
„ 2.0 „ 2.15 „
„ 3.7 „ 3.17 „
„ 3.34 „ 3.45 „
„ 4.15 „ 4.23 „
„ 4.52 „ 5.3 „
„ 5.56 „ 6.10 „
„ 6.25 „ 6.45 „
„ 7.10 „ 7.18 „
„ 7.45 „ 8.0 „
„ 8.22 „ 8.32 „
„ 9.18 „ 9.30 „
„ 10.10 „ 10.20 „

During the whole day she brought no friend, and only one other ant found the honey, evidently an independent discovery.

Experiments testing Communication by Sound.

To test the power which ants might have of summoning one another by sound, I tried the following experiments. I put out on the board where one of my nests of *Lasius flavus* was usually

fed, six small pillars of wood, about 1½ inch high, and on one of them I put some honey. A number of ants were wandering about on the board itself in search of food, and the nest itself was immediately above and about 12 inches from the board. I then put three ants to the honey, and when each had sufficiently fed, I imprisoned her, and put another; thus always keeping three ants at the honey, but not allowing them to go home. If, then, they could summon their friends by sound, there ought soon to be many ants at the honey. The results were as follows:—

Sept. 8. Began at 11 A.M. Up to 3 o'clock only seven ants found their way to the honey, while about as many ran up the other pillars. The arrival of these seven, therefore, was not more than would naturally result from the numbers running about close by. At 3 we allowed the ants then on the honey to return home. The result was that from 3.6, when the first went home, to 3.30, 11 came, from 3.30 to 4 no less than 43. Thus in four hours only 7 came; while it is obvious that many would have wished to come if they had known about the honey, because in the next three quarters of an hour, when they were informed of it, 54 came.

On the 10th Sept. we tried the same again, keeping as before three ants on the honey, but not allowing any to go home. From 12 to 5.30 only eight came. They were then allowed to take the news. From 5.30 to 6, 4 came; from 6 to 6.30, 4; from 6.30 to 7, 8; from 7.30 to 8 no less than 51.

On the 23rd Sept. we did the same again, beginning at 11.15. Up to 3.45 nine came. They were then allowed to go home. From 4 to 4.30, 9 came; from 4.30 to 5, 15; from 5 to 5.30, 19; from 5.30 to 6, 38. Thus in 3½ hours 9 came; in 2, when the ants were permitted to return, 81.

Again, on Sept. 30, I tried the same arrangement again, beginning at 11. Up to 3.30 seven ants came. We then let them go. From 3.30 to 4.30, 28 came; from 4.30 to 5, 51 came. Thus in four hours and a half only 7 came; while when they were allowed to return, no less than 79 came in an hour and a half.

It seems obvious, therefore, that in these cases no communication was transmitted by sound.

Experiments testing Affection.

To test the affection of ants belonging to the same nest for one another, I tried the following experiments. I took six ants from a nest of *Formica fusca*, imprisoned them in a small bottle, one end of which was left open, but covered by a layer of muslin. I then put the bottle close to the door of the nest. The muslin

was of open texture, the meshes, however, being sufficiently large to prevent the ants from escaping. They could not only, however, see one another, but communicate freely with their antennæ. We now watched to see whether the prisoners would be tended or fed by their friends. We could not, however, observe that the least notice was taken of them. The experiment, nevertheless, was less conclusive than could be wished, because they might have fed at night, or at some time when we were not looking. It struck me, therefore, that it would be interesting to treat some strangers also in the same manner.

On Sept. 2, therefore, I put two ants from one of my nests of *F. fusca* into a bottle, the end of which was tied up with muslin as described, and laid it down close to the nest. In a second bottle I put two ants from another nest of the same species. The ants which were at liberty took no notice of the bottle containing their imprisoned friends. The strangers in the other bottle, on the contrary, excited them considerably. The whole day one, two, or more ants stood sentry, as it were, over the bottle. In the evening no less than twelve were collected round it, a larger number than usually came out of the nest at any one time. The whole of the next two days, in the same way, there were more or less ants round the bottle containing the strangers; while, as far as we could see, no notice whatever was taken of the friends. On the 9th the ants had eaten through the muslin, and effected an entrance. We did not chance to be on the spot at the moment; but as I found two ants lying dead, one in the bottle and one just outside, I think there can be no doubt that the strangers were put to death. The friends throughout were quite neglected.

Sept. 21.—I then repeated the experiment, putting three ants from another nest in a bottle as before. The same scene was repeated. The friends were neglected. On the other hand, some of the ants were always watching over the bottle containing the strangers, and biting at the muslin which protected them. The next morning at 6 A.M. I found five ants thus occupied. One had caught hold of the leg of one of the strangers, which had unwarily been allowed to protrude through the meshes of the muslin. They worked and watched, though not, as far as I could see, with any system, till 7.30 in the evening, when they effected an entrance, and immediately attacked the strangers.

Sept. 24.—I repeated the same experiment with the same nest. Again the ants came and sat over the bottle containing the strangers, while no notice was taken of the friends.

The next morning again, when I got up, I found five ants round the bottle containing the strangers, none near the friends. As in the former case, one of the ants had seized a stranger by the leg, and was trying to drag her through the muslin. All day the ants clustered round the bottle, and bit perseveringly, though not systematically, at the muslin. The same thing happened all the following day.

These observations seemed to me sufficiently to test the behaviour of the ants belonging to this nest under these circumstances. I thought it desirable, however, to try also other communities. I selected, therefore, two other nests. One was a community of *Polyergus rufescens* with numerous slaves. Close to where the ants of this nest came to feed, I placed as before two small bottles, closed in the same way—one containing two slave ants from the nest, the other two strangers. These ants, however, behaved quite unlike the preceding, for they took no notice of either bottle, and showed no sign either of affection or hatred. One is almost tempted to surmise that the war-like spirit of these ants was broken by slavery.

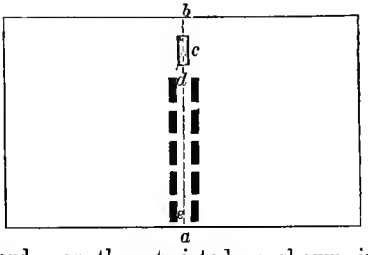
The other nest which I tried, also a community of *Formica fusca*, behaved exactly like the first. They took no notice of the bottle containing the friends, but clustered round and eventually forced their way into that containing the strangers.

It seems, therefore, that in these curious insects hatred is a stronger passion than affection.

Experiments showing the importance of the Sense of Smell to certain Ants.

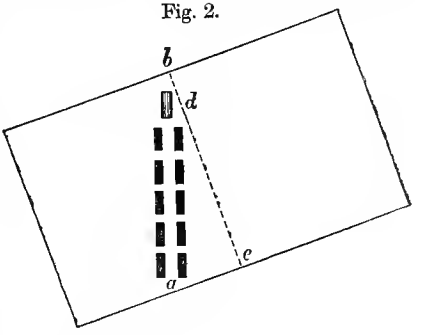
In order further to test how far ants are guided by sight and how much by scent, I tried the following experiment with *Lasius niger*. Some food was put out at the point *a* on a board measuring 20 inches by 12 (fig. 1), and so arranged that the ants in going straight to it from the nest would reach the board at the point *b*, and after passing under a paper tunnel, *c*, would proceed between five pairs of wooden bricks, each 3 inches in length and $1\frac{3}{4}$ in height. When they got to know their way, they went quite straight along the line *d e* to *a*. The board was then twisted as shown in fig. 2. The bricks and tunnel being arranged exactly in the same direction as before, but the board having been moved,

Fig. 1.



the line *d e* was now outside them. This change, however, did not at all discompose the ants; but instead of going, as before, through the tunnel and between the rows of bricks to *a*, they walked exactly along the old path to *e*.

Fig. 2.



I then arranged matters as before, but without the tunnel and with only three pairs of bricks (fig. 3). When an ant had got quite used to the path *d* to *e*, I altered the position of the bricks and food to *f* (fig. 4), making a difference of 8 inches in the position of the latter. The ant came as before, walked up to the first brick, touched it with her antennæ, but then followed her old line to *a*. From there she veered towards the food, and very soon found it. When she was gone, I altered it again, as shown in fig. 5; she returned after the usual interval, and went again straight to *a*; then, after some wanderings, to *f*, and at length, but only after a lapse of 25 minutes, found the food at *g*. These experiments were repeated more than once, and always with similar results. I then varied matters by removing the bricks, which, however, did not seem to make any difference to the ants.

Fig. 3.

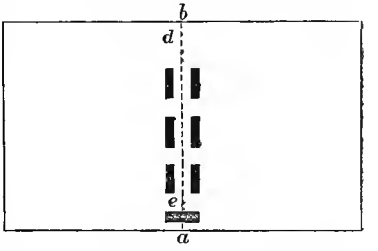


Fig. 4.

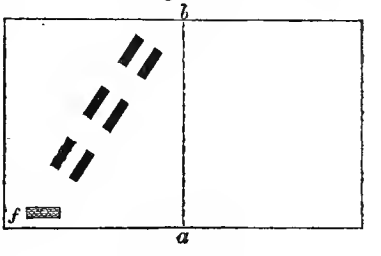
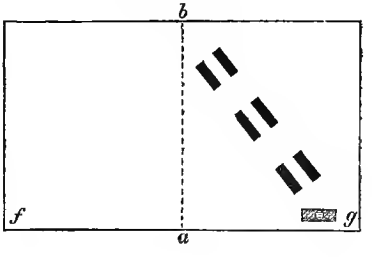


Fig. 5.



Experiments showing how Ants are affected by different coloured Lights and Media.

From the observations of Sprengel there could of course be little, if any, doubt, that bees are capable of distinguishing colours; but I have in my previous papers read before the Linnean Society recorded some experiments which put the matter beyond a doubt. Under these circumstances, I have been naturally anxious to ascertain, if possible, whether the same is the case with ants. I have, however, found more difficulty in doing so, because, as shown in the observations just recorded, ants find their food so much more by smell than by sight.

I tried, for instance, placing food at the bottom of a pillar of coloured paper, and then moving both the pillar and food. The pillar, however, did not seem to help the ant (*Lasius niger*) at all to find her way to the food. I then, as recorded in my previous paper, placed the food on the top of a rod of wood 8 inches high, and when the ant knew her way perfectly well to the food so that she went quite straight backwards and forwards to the nest, I found that if I moved the pillar of wood only six inches, the ant was quite bewildered, and wandered about backwards and forwards, round and round, and at last only found the pillar, as it were, accidentally.

Under these circumstances, I could not apply to ants those tests which had been used in the case of bees. At length, however, it occurred to me that I might utilize the dislike which ants, when in their nests, have to light. Of course they have no such feeling when they are out in search of food; but if light is let in upon their nests, they at once hurry about in search of the darkest corners, and there they all congregate. If, for instance, I uncovered one of my nests and then placed an opake substance over one portion, the ants invariably collected in the shaded part.

I procured, therefore, four similar strips of glass, coloured respectively green, yellow, red, and blue, or, rather, violet. The yellow was rather paler in shade, and that glass consequently rather more transparent than the green, which, again, was rather more transparent than the red or violet. I then laid the strips of glass on one of my nests of *Formica fusca* containing about 170 ants. These ants, as I knew by many previous observations, seek darkness, and would certainly collect under any opake substance.

I then, after counting the ants under each strip, moved the colours gradually at intervals of about half an hour, so that each

should by turns cover the same portion of the nest. The results were as follows—the numbers indicating the approximate numbers of ants under each glass (there were sometimes a few not under any of the strips of glass):—

1.	Green.	Yellow.	Red.	Violet.
	50	40	80	0
2.	Violet.	Green.	Yellow.	Red.
	0	20	40	100
3.	Red.	Violet.	Green.	Yellow.
	60	0	50	50
4.	Yellow.	Red.	Violet.	Green.
	50	70	1	40
5.	Green.	Yellow.	Red.	Violet.
	30	30	100	0
6.	Violet.	Green.	Yellow.	Red.
	0	14	5	140
7.	Red.	Violet.	Green.	Yellow.
	50	0	40	70
8.	Yellow.	Red.	Violet.	Green.
	40	50	1	70
9.	Green.	Yellow.	Red.	Violet.
	60	35	65	0
10.	Violet.	Green.	Yellow.	Red.
	1	50	40	70
11.	Red.	Violet.	Green.	Yellow.
	50	2	50	60
12.	Yellow.	Red.	Violet.	Green.
	35	55	0	70

Adding these numbers together, there were, in the twelve observations, under the red 890, under the green 544, under the yellow 495, and under the violet only 5. The difference between the red and the green is very striking, and would doubtless have been more so, but for the fact that when the colours were transposed the ants which had collected under the red sometimes remained quiet, as, for instance, in cases 7 and 8. Again, the difference between the green and yellow would have been still more marked but for the fact that the yellow always occupied the position last held by the red, while, on the other hand, the green had some advantage in coming next the violet. In considering the differ-

ence between the yellow and green, we must remember also that the green was decidedly more opake than the yellow.

The case of the violet glass is more marked and more interesting. To our eyes the violet was as opake as the red, more so than the green, and much more so than the yellow. Yet, as the numbers show, the ants had scarcely any tendency to congregate under it. There were nearly as many under the same area of the uncovered portion of the nest as under that shaded by the violet glass.

Lasius flavus also showed a marked avoidance of the violet glass.

I then experimented in the same way with a nest of *Formica fusca*, in which there were some pupæ, which were generally collected in a single heap. I used glasses coloured dark yellow, dark green, light yellow, light green, red, violet, and dark purple. The colours were always in the preceding order, but, as before, their place over the nest was changed after every observation.

To our eyes the purple was almost black, the violet and dark green very dark and quite opake; the pupæ could be dimly seen through the red, rather more clearly through the dark yellow and light green, while the light yellow were almost transparent. There were about 50 pupæ, and the light was the ordinary diffused daylight of summer. (See Table, p. 281.)

These observations show a marked preference for the greens and yellows. The pupæ were $6\frac{1}{2}$ times under dark green, 3 under dark yellow, $3\frac{1}{2}$ under red, and once each under light yellow and light green, the violet and purple being altogether neglected.

I now tried the same ants under the same colours, only in the sun; and placed a shallow dish containing some 10 per cent. solution of alum sometimes over the yellow, sometimes over the red. I also put four thicknesses of violet glass, so that it looked almost black. (See Table, p. 282.)

Under these circumstances, the pupæ were placed under the red $7\frac{1}{2}$ times, dark yellow $5\frac{1}{2}$, and never under the violet, purple, light yellow, dark or light green.

The following day I placed over the same nest, in the sun, dark green glass, dark red and dark yellow (two layers of each). In nine observations the pupæ were carried 3 times under the red and nine times under the yellow.

I then tried a similar series of experiments with *Lasius niger*, using part of a nest in which were about 40 pupæ, which were generally collected in a single heap all together. As before, the glasses were moved in regular order after each

1.	Dark yellow.	Dark green.	Light yellow.	Light green.	Red.	Violet.	Purple.	The pupæ were placed under		dark green.
2.	Purple.	Dark yellow.	Dark green.	Light yellow.	Light green.	Red.	Violet.	,,	,,	,,
3.	Violet.	Purple.	Dark yellow.	Light yellow.	Dark green.	Light green.	Red.	,,	,,	light yellow.
4.	Red.	Violet.	Purple.	Dark yellow.	Light yellow.	Dark green.	Light green.	,,	,,	dark green.
5.	Light green.	Red.	Violet.	Purple.	Dark yellow.	Light yellow.	Dark green.	,,	,,	dark yellow.
6.	Dark green.	Light green.	Red.	Violet.	Purple.	Dark yellow.	Light yellow.	,,	,,	,,
7.	Light yellow.	Dark green.	Light green.	Red.	Violet.	Purple.	Dark yellow.	,,	,,	light green.
8.	Dark yellow.	Light yellow.	Light yellow.	Light green.	Red.	Violet.	Purple.	,,	,,	dark green.
9.	Purple.	Dark yellow.	Dark yellow.	Dark green.	Light green.	Red.	Violet.	,,	,,	,,
10.	Violet.	Purple.	Purple.	Light yellow.	Dark green.	Light green.	Red.	,,	,,	red.
11.	Red.	Red.		Dark yellow.	Light yellow.	Dark green.	Light green.	,,	,,	dark green.
12.	Light yellow.	Purple.	Light green.	Dark yellow.	Dark green.	Violet.	Red.	,,	,,	dark green 30; red 30.
13.	Red.	Light yellow.	Purple.	Light green.	Dark yellow.	Dark green.	Violet.	,,	,,	,, ,,
14.	Violet.	Red.	Light yellow.	Purple.	Light green.	Dark yellow.	Dark green.	,,	,,	,, ,,
15.	Dark green.	Violet.	Red.	Light yellow.	Purple.	Light green.	Dark yellow.	,,	,,	dark yellow 20; red 40.

1. Dark yellow.	Dark green.	Violet.	Red.	Light yellow.	Purple.	Light green.	Pupæ under red.	
2. Light green.	Dark yellow.	Dark green.	Violet.	Red.	Light yellow.	Purple.	,,	,,
3. Purple.	Light green.	Dark yellow.	Dark green.	Violet.	Red.	Light yellow.	,,	,,
4. Light yellow.	Purple.	Light green.	Dark yellow.	Dark green.	Violet.	Red and alum solution.	,,	dark yellow.
5. Red.	Light yellow.	Purple.	Light green.	Dark yellow.	Dark green.	Violet.	,,	red 20, dark yellow 40.
6. Violet.	Red.	Light yellow.	Purple.	Light green.	Dark yellow and alum solution.	Dark green.	,,	dark yellow.
7. Dark green.	Violet.	Red.	Light yellow.	Purple.	Light green.	Dark yellow and alum solution.	,,	,,
8. Dark yellow and alum solution.	Dark green.	Violet.	Red.	Light yellow.	Purple.	Light green.	,,	,,
9. Light green.	Dark yellow and alum solution.	Dark green.	Violet.	Red.	Light yellow.	Purple.	,,	,,
10. Purple.	Light green.	Dark yellow.	Dark green.	Violet.	Red and alum solution.	Light yellow.	,,	red.
11. Red and alum solution.	Light yellow.	Light green.	Dark yellow.	Dark green.	Violet.	Purple.	,,	,,
12. Purple.	Red and alum solution.	Light yellow.	Light green.	Dark yellow.	Dark green.	Violet.	,,	,,
13. Dark green.	Violet.	Red and alum solution.	Light yellow.	Light green.	Dark yellow.	Purple.	,,	,,

experiment; and I arranged them so that the violet followed the red. As far, therefore, as position was concerned, this gave violet rather the best place. The glasses used were dark violet, dark red, dark green, and yellow, the yellow being distinctly the most transparent to our eyes.

Experiment.			Experiment.		
1.	Pupæ under	yellow.	17.	Pupæ under	yellow.
2.	„	„	18.	„	„
3.	„	„	19.	„	red.
4.	„	„	20.	„	„
5.	„	„	21.	„	yellow.
6.	„	„	22.	„	„
7.	„	green.	23.	„	„
8.	„	„	24.	„	red.
9.	„	red.	25.	„	yellow.
10.	„	yellow.	26.	„	red.
11.	„	red.	27.	„	„
12.	„	yellow.	28.	„	„
13.	„	„	29.	„	„
14.	„	red.	30.	„	yellow.
15.	„	green.	31.		red.
16.	„	„	32.	„	green.

I now put two extra thicknesses of glass over the red and green.

33.	Pupæ under	red.	37.	Pupæ under	red.
34.	„	yellow.	38.	„	„
35.	„	red.	39.	„	yellow.
36.	„	yellow.	40.	„	red.

The result is very striking, and in accordance with the observations on *Formica fusca*. In 40 experiments the pupæ were carried under the yellow 19 times, under the red 16 times, and under the green 5 times only, while the violet was quite neglected. After the first twenty observations, however, I removed it.

I then tried a nest of *Cremastogaster scutellaris* with violet glass, purple glass, and red, yellow, and green solutions, formed respectively with fuchsine, bichromate of potash, and chloride of copper. The purple looked almost black, the violet very dark; the red and green, on the contrary, very transparent, and the yellow even more so. The yellow was not darker than a tincture of saffron. The latter indeed, to my eye, scarcely seemed to render the insects under them at all less apparent; while under

the violet and purple I could not trace them at all. I altered the relative positions as before. The nest contained about 50 larvæ and pupæ.

Observation.						
1.	Violet gl.	Purple gl.	.....	Bichr. pot.	Fuchsine.	Pupæ and larvæ half under the yellow and half under the green.
2.	.....	Bichr. pot.	Fuchsine.	Violet gl.	Purple gl.	
3.	Fuchsine.	Violet gl.	Purple gl.	.....	Bichr. pot.	
4.	Green.	Bichr. pot.	Violet gl.	Purple gl.	Fuchsine.	
5.	Violet gl.	Purpl. gl.	Fuchsine.	.....	Bichr. pot.	
6.	Fuchsine.	.....	Bichr. pot.	Purple gl.	Violet gl.	
7.	Violet gl.	Fuchsine.	.....	Bichr. pot.	Purple gl.	
8.	Purple gl.	Violet gl.	Fuchsine.	.....	Bichr. pot.	
9.	Bichr. pot.	Purple gl.	Violet gl.	.....	Bichr. pot.	
10.	Fuchsine.	.....	Bichr. pot.	Purpl. gl.	Violet gl.	

I then poured out half the yellow and green solutions and filled them up with water, making them even lighter in colour as before.

Observation.						
12.....	Purple gl.	.....	Violet gl.	Fuchsine.	Bichr. pot.	Pupæ and larvæ half under the yellow and half under the green.
13.....	Bichr. pot.	Purpl. gl.	.....	Violet gl.	Fuchsine.	

Thus in every case the larvæ and pupæ were brought under the yellow or the green.

Aug. 20.—Over a nest of *Formica fusca* containing about 20 pupæ I placed violet glass, purple glass, a weak solution of fuchsine (carmine), the same of chloride of copper (green), and of bichromate of potash (yellow, not darker than saffron).

1.	Violet.	Purple.	Green.	Yellow.	Red.	The pupæ were placed under the yellow.		
2.	Red.	Violet.	Purple.	Green.	Yellow.	,,	,,	,,
3.	Yellow.	Red.	Violet.	Purple.	Green.	,,	,,	,,
4.	Green.	Yellow.	Red.	Violet.	Purple.	,,	,,	,,
5.	Purple.	Green.	Yellow.	Red.	Violet.	,,	,,	,,
6.	Violet.	Purple.	Green.	Yellow.	Red.	,,	,,	,,
7.	Red.	Violet.	Purple.	Green.	Yellow.	,,	,,	green and yellow.
8.	Yellow.	Red.	Violet.	Purple.	Green.	,,	,,	yellow.
9.	Green.	Yellow.	Red.	Violet.	Purple.	,,	,,	green and yellow.
10.	Purple.	Green.	Yellow.	Red.	Violet.	,,	,,	yellow.
11.	Violet.	Purple.	Green.	Yellow.	Red.	,,	,,	,,

Here, again, in every case the pupæ were brought under the yellow or the green.

I then tried a nest of *Lasius flavus* with the purple glass, violet glass, the very weak bichromate of potash, and chloride of copper as before.

Observation.					[and green.
1.	Yellow.	Green.	Purple.	Violet.	The pupæ were brought under the yellow
2.	Violet.	Yellow.	Green.	Purple.	,, ,, green.
3.	Purple.	Violet.	Yellow.	Green.	,, ,, ,,
4.	Green.	Purple.	Violet.	Yellow	,, ,, ,,
5.	Yellow.	Green.	Purple.	Violet.	,, ,, yellow.
6.	Violet.	Yellow.	Green.	Purple.	,, ,, green.

The results, then, were the same as in the previous cases.

In these experiments, then, the violet and purple affected the ants much more strongly than the yellow and green.

It is curious that the coloured glasses appear to act on the ants (speaking roughly) as they would, or, I should rather say, inversely as they would, on a photographic plate. It might even be alleged that the avoidance of the violet glass by the ants was due to the chemical rays which are transmitted. From the habits of these insects such an explanation is very improbable. If, however, the preference for the other coloured glasses to the violet was due to the transmission and not to the absorption of rays—that is to say, if the ants went under the green rather than the violet because the green or red transmitted rays which were agreeable to the ants, and which the violet glass, on the contrary, stopped—then, if the violet was placed over the other colours, they would become as distasteful to the ants as the violet itself. On the contrary, however, whether the violet glass was placed over the others or not, the ants equally readily took shelter under them. Obviously, therefore, the ants avoid the violet glass because they dislike the rays which it transmits.

Mr. Busk suggested that as the red glass stops the chemical rays more effectually than the yellow or green, while the violet is most transparent to them, and as the ants prefer the red glass to the yellow or green, and these, again, to the violet, possibly the explanation might be that the chemical rays were peculiarly distasteful to them. To test this, therefore, I made some experiments with fluorescent liquids which Mr. Hanbury was kind enough to procure for me from Mr. Benger, of Manchester. They were prepared by M. Caro, of Manheim. One was opake grass-green by reflected light and orange by transmitted; one violet by transmitted light and red by reflected; and a third green by transmitted and red by reflected light. I believe their exact chemical composition is not known, but that, in all cases, fluorescine is the principal ingredient. They stop the chemical rays, or rather turn them into visible rays. The action takes

place altogether at the surface of the liquids, so that it is not necessary to use any large quantity. I poured them into shallow glass cells about $\frac{1}{2}$ inch deep, which I put, as before, over the ants. If now they were affected mainly by the chemical rays, it must appear to them to be dark under these solutions. This, however, was not the case. The solutions seemed to make no difference to them. I also tried quinine and uranium glass with the same effect.

In order to ascertain what colours were transmitted by these several media, I then tested them with the spectroscope, and with the following results :—

The violet glass			transmitted violet, blue, some green and yellow to about the line D in the spectrum or a trifle beyond.
„	„	(double)	transmitted violet and blue with tinge of red.
green glass		„	most of blue, and about to line "*a*."
„	„ (double)	„	green, yellow, and red to about line "C."
„	„ (dark)	„	green and some yellow.
yellow	„	„	the spectrum from red end to about halfway between "F" and "S."
„	„ (dark)	„	from red to end of green about "F."
red	„	„	red with a touch of orange.
„	„ (double)	„	only red.
purple	„	„	a little violet, a little yellow, orange, and red.

Amm. sulph. of copper (blue)			transmitted violet and blue only.
Chloride of copper ...	(green)	„	green, an edging of blue, and faint yellow with an edging of orange.
Saffron.....		„	every thing except violet and blue.
Bichromate of potash	(orange)	„	red, orange, yellow, and very little green.
„	(very pale yellow)	„	red, orange, yellow, and green.
Fuchsine	(carmine)	„	red only.
Solution of carmine ..	„	„	„
Solution of iodine	„	„	red, orange, and a very little yellow.

But though the ants so markedly avoided the violet glass, still, as might be expected, the violet glass certainly had some effect, because if it was put over the nest alone, the ants preferred being under it to being under the plain glass only.

I then compared the violet glass with a solution of ammonio-sulphate of copper, which is very similar, though perhaps a little more violet, and arranged the depth of the fluid so as to make it as nearly as possible of the same depth of colour as the glass.

Approx. number of Ants under the	Exp. 1.	Exp. 2.	Exp. 3.	Exp. 4.	Exp. 5.	Exp. 6.	Exp. 7.	Exp. 8.	Exp. 9.	Exp. 10.	Total.
Glass.....	0	0	0	2	0	2	0	2	3	0	9
Solution	40	80	100	80	50	70	60	40	90	100	710

In another experiment with *Lasius niger* I used the dark yellow glass, dark violet glass, and a violet solution of 5 per cent. ammonio-sulphate of copper, diluted so as to be, to my eye, of exactly the same tint as the violet glass ; in 8 observations the pupæ were three times under the violet solution, and 5 times under the yellow glass. I then removed the yellow glass, and in 10 more observations the pupæ were always brought under the solution.

It is interesting that the glass and the solution should affect the ants so differently, because to my eye the two were almost identical in colour. The glass, however, was more transparent than the solution.

To see whether there would be the same difference between red glass and red solution as between violet glass and violet solution, I then (Aug. 21) put over a nest of *Formica fusca* a red glass and a solution of carmine, as nearly as I could make it of the same tint. In 10 experiments, however, the ants were, generally speaking, some under the solution and some under the glass, in, moreover, as nearly as possible equal numbers.

Aug. 20.—Over a nest of *Formica fusca* containing 20 pupæ, I placed a saturated solution of bichromate of potash, a deep solution of carmine, which let through scarcely any but the red rays, and a white porcelain plate.

Observation					
1.	Under the bichromate of potash were	0	pupæ, carmine	18,	porcelain 2.
2.	„ „	0	„ „	6 „	14.
3.	„ „	6	„ „	3 „	11.
4.	„ „	0	„ „	5 „	18.
5.	„ „	6	„ „	4 „	10.
6.	„ „	0	„ „	19 „	1.
7.	„ „	0	„ „	0 „	20.
8.	„ „	4	„ „	15 „	1.
9.	„ „	2	„ „	4 „	14.
10.	„ „	0	„ „	4 „	16.
11.	„ „	0	„ „	3 „	17.
	Total.....	18		81	124

I then put over another nest of *Formica fusca* four layers of red glass (which, when examined with the spectroscope, let through red light only), four layers of green glass (which, examined in the same way, transmitted nothing but a very little green), and a porcelain plate. Under these circumstances the ants showed no marked preference, but appeared to feel equally protected, whether they were under the red glass, the green glass, or the porcelain.

Thus, though it appears from other experiments that ants are affected by red light, still the quantity that passes through dark red glass does not seem to disturb them. I tested this again by placing over a nest containing a queen and about 10 pupæ a piece of opake porcelain, one of violet, and one of red glass, all of the same size. The result is shown below.

Observation.							
1.	Queen went under	red glass.	5	pupæ were taken under	red glass,	2	under porcelain.
2.	,,	porcelain.	0	,,	,,	7	,,
3.	,,	red glass.	0	,,	,,	7	,,
4.	,,	,,	6	,,	,,	2	,,
5.	,,	,,	6	,,	,,	2	,,
6.	,,	,,	3	,,	,,	7	,,
7.	,,	,,	10	,,	,,	0	,,
8.	,,	,,	4	,,	,,	6	,,
9.	,,	,,	1	,,	,,	0	,,
10.	,,	porcelain.	0	,,	,,	10	,,
11.	,,	red glass.	10	,,	,,	0	,,
12.	,,	porcelain.	4	,,	,,	6	,,
13.	,,	red glass.	7	,,	,,	3	,,
14.	,,	porcelain.	4	,,	,,	6	,,
15.	,,	red glass.	4	,,	,,	6	,,
16.	,,	porcelain.	0	,,	,,	10	,,
17.	,,	red glass.	10	,,	,,	0	,,
18.	,,	,,	8	,,	,,	2	,,
19.	,,	porcelain.	7	,,	,,	3	,,
20.	,,	,,	1	,,	,,	9	,,
			90			88	

Obviously, therefore, the ants showed no marked preference for the porcelain. On one, but only on one occasion (Obs. 9), most of the pupæ were carried under the violet glass, but generally it was quite neglected.

I now tried a similar experiment with two layers of yellow glass.

Obs.							
1.	Queen went under the porcelain.		8	pupæ were taken under yellow,		2	under porcelain.
2.	„ „	„	2	„	„	8	„
3.	„ „	„	8	„	„	2	„
4.	„ „	yellow glass.	5	„	„	5	„
5.	„ „	porcelain.	3	„	„	8	„
6.	„ „	yellow glass.	8	„	„	3	„
7.	„ „	porcelain.	6	„	„	5	„
8.	„ „	„	0	„	„	7	„
9.	„ „	„	0	„	„	10	„
10.	„ „	yellow glass.	9	„	„	1	„
11.	„ „	porcelain.	8	„	„	2	„
12.	„ „	„	3	„	„	7	„
13.	„ „	yellow glass.	10	„	„	0	„
14.	„ „	porcelain.	0	„	„	10	„
15.	„ „	yellow glass.	10	„	„	0	„
16.	„ „	„	7	„	„	3	„
17.	„ „	„	10	„	„	0	„
18.	„ „	porcelain.	1	„	„	9	„
19.	„ „	„	0	„	„	10	„
			98			92	

I then put two ants on a paper bridge, the ends supported by pins, the bases of which were in water. The ants wandered backwards and forwards, endeavouring to escape. I then placed the bridge in the dark and threw the spectrum on it, so that successively the red, yellow, green, blue, and violet fell on the bridge.

The ants, however, walked backwards and forwards without (perhaps from excitement) taking any notice of the colour.

I then allowed some ants (*Lasius niger*) to find some larvæ, to which they obtained access over a narrow paper bridge. When they had got used to it, I arranged so that it passed through a dark box, and threw on it the principal colours of the spectrum, namely, red, yellow, green, blue, and violet, as well as the ultra-red and ultra-violet; but the ants took no notice.

At the suggestion of Prof. Stokes, I then tried the following experiments. Mr. Spottiswoode not only most kindly placed the rich resources of his laboratory at my disposal, but he and his able assistant Mr. Ward were good enough to arrange the apparatus for me.

We tried the ants with coloured lights in a Bunsen's burner, using chloride of strontium and carbonate of lithia for red, chloride of barium for green, and chloride of sodium for yellow. The lithium gives an almost pure red, the strontium and barium give a little yellow, but so little that I do not think it would affect the ants.

The ants on which we experimented were *Formica fusca* and *F. cinerea* and *Cremastogaster scutellaris*; but it was rather too late in the season, and they were somewhat torpid.

The yellow of the soda-flame certainly affected the *Formica cinerea*, but the others seemed to take no notice of it.

The barium also affected the *F. cinerea*, but neither of the others; I could not feel sure whether it was the green or the accompanying yellow which disturbed them. The red of the lithium was not so brilliant, still the *F. cinerea* seemed to perceive it.

The strontium-flame did not seem to have any effect on the ants.

It is obvious that these facts suggest a number of interesting inferences. I must, however, repeat the observations and make others; but we may at least, I think, conclude from the preceding that:—(1) ants have the power of distinguishing colour; (2) that they are very sensitive to violet; and it would also seem (3) that their sensations of colour must be very different from those produced upon us.

As to the Longevity of Ants.

I have been much surprised at the longevity of my ants. I have still two queens of *Formica fusca** which have been with me since 1874. They must therefore now be at any rate four years old; but as they were probably a year old when I captured them, they would now be not less than five years old. As regards workers, I have some specimens of *Formica sanguinea* and *F. fusca* which M. Forel was so good as to send me from Munich in the beginning of September 1875, some *F. cinerea* which I brought back from Castellamare in Nov. 1875, and a great many belonging to various species which have been with me since 1876.

* These ants are still alive, Aug. 1878.

[*Extracted from the* Linnean Society's Journal—Zoology, vol. xiv.]

Observations on the Habits of Ants, Bees, and Wasps.—Part VI. 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 February 6, 1879.]

Ants.

Plant-hairs and Fur preventative against Ants climbing.

When I first began keeping ants, I surrounded the nests by moats of water. This acted well; but the water required continually renewing, especially, of course, in summer, just when the ants were most active. At length, however, in considering the habits of ants and their relations to flowers, another plan suggested itself to me. The hairs by which plants are clothed are of various forms, and fulfil various functions. One is, I believe, to prevent ants and other creeping insects from climbing up the plants so as to obtain access to the flowers, and thus rob them of their honey.

It occurred to me, therefore, that instead of water I might use fur arranged so that the hairs pointed downwards. This I have found to answer perfectly; and I mention it specially because the same arrangement may be found practically useful in hot climates. It is, of course, very possible that in hot countries the fur might be open to objections which do not occur in England; and, again, the tropical species might be able to climb up the fur; but at any rate the experiment would be worth trying.

Workers frequently fertile as well as Queens, but produce usually Males.

It is generally stated that among ants the queens only lay eggs. This, however, is not correct.

Denny* and Lespès† have shown that the workers also are capable of producing eggs; but the latter asserted that these eggs never come to maturity. Forel, however, has proved‡ that this is not the case, but that in some cases, at any rate, the eggs do produce young. Dewitz even maintains§ that the workers

* Ann. & Mag. Nat. Hist. 2nd ser. vol. i. p. 240.

† Ann. des Sci. Nat. 1863.

‡ Fourmis de la Suisse, p. 329.

§ Zeit. f. wiss. Zool. vol. xxviii. p. 536.

habitually lay eggs, and explains the difference which on this view exists between the workers of ants and those of bees, on the ground that (as he supposes) the majority of ants die in the autumn, so that the eggs laid by the queens alone would not be sufficient to stock the nest in the spring; while among bees the majority survive the winter, and consequently the eggs laid by the queen are sufficient to maintain the numbers of the community. In reply to this argument, it may be observed that among wasps the workers all perish in the autumn, while, on the contrary, among ants I have proved that, at least as regards many species, this is not the case. Moreover, although eggs are frequently laid by workers, this is not so often the case as Dewitz appears to suppose. Forel appears to have only observed it in one or two cases. In my nests the instances were more numerous; and, indeed, I should say that in most nests there were a few fertile workers.

Among bees and wasps also the workers are occasionally fertile; but, so far as our observations go, it is a curious fact that their eggs never produce females, either queens or workers, but always males. The four or five specimens bred by Forel from the eggs of workers were, moreover, all males.

It would therefore be interesting to know whether the same is the case generally among ants; and my nests have supplied me with some facts bearing on the question. Most of my nests contained queens; and in these it would be impossible, or at least very difficult, to distinguish and follow the comparatively few eggs laid by the workers. Some of my nests, however, contained no queen; and in them therefore all the eggs must have been laid by workers.

One of these was a nest of *Formica cinerea*, which I brought back from Castellamare in November 1875. At that time it contained no eggs or larvæ. In 1876 a few eggs were laid, of which fifteen came to maturity, and were, I believe, all males. Last year there were fourteen pupæ, of which twelve came to maturity and were all males.

Again, in a nest of *Lasius niger*, kept in captivity since June 1875, there were in 1876 about 100 young; and these were, as far as I could ascertain, all males. At any rate there were about 100 males, and I could not find a single young female. In 1877 there were again some pupæ; but none of them came to maturity. Last year fifteen have come to maturity; and fourteen

were males. The other I could not find; but I have no doubt, from the appeacance of the pupa, that it was also a male.

Another nest of *Lasius niger*, captured in November 1875, in 1878 brought only one young ant to maturity; and this was a male.

Again, in a nest of *Formica fusca* captured in 1875, though in 1876 and 1877 eggs were laid and a few arrived at the pupa-state, none came to maturity. They were all, however, either males or queens, and, I have little doubt, were males. This year one came to maturity, and it was a male.

Another nest of *F. fusca*, captured in 1876, did not bring up any young in 1877. This year three larvæ came to maturity; and they all proved to be males. A nest of *F. fusca*, captured in 1877, in 1878 brought only one young one to maturity. This was a male.

In these species, then, as far as the evidence goes, it would seem that, as in bees, the workers can produce males only. On the other hand, I ought to add that in a nest of *Lasius flavus* captured in 1876 a number of workers were produced this year. In this species the larvæ live through the winter; but I can hardly believe they take two years in coming to maturity. Nor do I think the ants had access to any other larvæ; still I would not attach too much importance to this isolated case. In the case of bees we know that the queen is brought up on different food from the workers. Whether this is the case among ants, we have no knowledge. I am, however, disposed to believe it; because while hundreds, I might say thousands, of workers have been bred in my nests, and a large number of males, not a single queen has been produced in any one of them.

As to the Relations of Ants and their Domestics.

M. Lespès has given a short but interesting account of some experiments made by him on the relations existing between ants and their domestic animals, from which it might be inferred that even within the limits of a single species some communities are more advanced than others. He found that specimens of the curious blind beetle *Claviger*, which always occurs with ants, when transferred from a nest of *Lasius niger* to another which kept none of these domestic beetles, were invariably attacked and eaten. From this he infers that the intelligence necessary to keep Clavigers is not coextensive with the species, but belongs

only to certain communities and races, which, so to say, are more advanced in civilization than the rest of the species.

With reference to the statements of Lespès, I have more than once transferred specimens of *Platyarthrus* from one nest to another, and always found them received amicably. I also transferred specimens from a nest of *Lasius flavus* to one of *Formica fusca* with the same result. I even brought from the South of France some specimens of a different species, I believe *Typhlopone*, and put them in a nest of *Formica fusca*, where they are still living, and have brought up more than one brood of young. These species, however, occur in most ants' nests, while Clavigers are only found in some.

Longevity of Ants.

In my previous paper I have called attention to the longevity of ants, which has proved to be much greater than I had expected. One of my nests of *Formica fusca* was brought from the woods in December 1874. It then contained two queens, both of which are (May 1879) 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.

In the following nests, however—viz. another nest of *Formica fusca*, which I brought in on the 6th June, 1875, one of *Lasius niger* on the 25th July, 1875, of *Formica sanguinea* (kindly sent me by M. Forel from Munich) on the 12th September, 1875, and of *F. 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 therefore now be between three and four years old.

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

On the mode of recognition of Friends.

It is clear, from the experiments recorded in the present and in my former papers, that the ants recognize their fellows in the same nest; but it is very difficult to understand how this can be effected. The nests vary very much in size; but in some species 100,000 individuals may be by no means an unusual number, and in some instances even this is largely exceeded. Now it seems almost incredible that in such cases every ant knows

every other one by sight. Neither does it seem possible that all the ants in each case should be characterized from those of other nests by any physical peculiarity.

It has been suggested that ants may recognize one another by scent. This, however, I think, cannot be the explanation. For instance, when intoxicated, ants were recognized; surely the whiskey must have obliterated any natural odour. Again, I have kept ants for forty-eight hours in an atmosphere strongly scented with musk; but when returned to the nest they were recognized without the least doubt or hesitation.

It has been suggested in the case of bees that each nest might have some sign or password.

The whole subject is full of difficulty. It occurred to me, however, that experiments with pupæ might throw some light on the subject. Although the ants of every nest, say of *Formica fusca*, are deadly enemies, still if larvæ or pupæ from one nest are transferred to another, they are kindly received, and tended with apparently as much care as if they really belonged to the nest. In ant-warfare, though sex is no protection, the young are spared, at least when they belong to the same species. Moreover, though the habits and disposition of ants are greatly changed if they are taken away from their nest and kept in solitary confinement, or only with a few friends, still, under such circumstances, they will carefully tend any young which may be confided to them. Now if the recognition were effected by means of some signal or password, then, as it can hardly be supposed that the larvæ or pupæ would be sufficiently intelligent to appreciate, still less to remember it, the pupæ which were intrusted to ants from another nest would have the password, if any, of that nest; and not of the one from which they had been taken. Hence, if the recognition were effected by some password or sign with the antennæ, they would be amicably received in the nest from which their nurses had been taken, but not in their own.

In the first place, therefore, I put, on the 2nd of September 1877, some pupæ from one of my nests of *Formica fusca* with a couple of ants from the same nest. On the 27th I put two ants, which in the meantime had emerged from one of these pupæ, into the nest at 8.30 A.M., marking them with paint as usual. At 9 they seemed quite at home; at 9.30, ditto; at 10, ditto; and they were nearly cleaned. After that I could not distinguish them.

On the 29th another ant came out of the pupa-state; and on

the 1st October at 7.45 I put her into the nest. She seemed quite at home, and the others soon began to clean her. We watched her from time to time, and she was not attacked; but, the colour being removed, we could not recognize her after 9.30.

On the 14th July last year (1878) I put into a small glass some pupæ from another nest of *Formica fusca* with two friends.

On the 11th August I put four of the young ants which had emerged from these pupæ into the nest. After the interval of an hour, I looked for them in vain. The door of the nest was closed with cotton-wool; so that they could not have come out; and if any were being attacked, I think we must have seen it. I believe, therefore, that in the meantime they had been cleaned. Still, as we did not actually watch them, I was not satisfied. I put in, therefore, two more at 5 P.M. At 5.30 they were all right; at 5.45, ditto, but one was almost cleaned. At 6 one was all right; the other was no longer recognizable, having been quite cleaned. At 6.30 also one was quite at home; the other could not be distinguished. At 7 both had been completely cleaned.

The following day I marked another, and put her in at 6 A.M. At 6.15 she was all right among the others, and also at 6.30, 7, 7.30, 8, and 9.30, after which I could no longer distinguish her.

Again, on the following day I put in another at 6.45 A.M. At 7 she was quite at home, and also at 7.15, 7.30, 8, and to 9.30, after which I did not watch her.

To test the mode in which the ants of this nest would behave to a stranger, I then, though feeling no doubt as to the result, introduced one. The difference was very striking. The stranger was a powerful ant; still she was evidently uncomfortable, started away from every ant she met, and ran nervously about, trying to get out of the nest. She was, however, soon attacked.

Again, on the 1st October some pupæ of *Lasius niger* were placed in a glass with five ants from the same nest.

On the 8th December I took three of the ants which had emerged from these pupæ, and at midday put them back into their old nest, having marked them by nicking the hind leg. Of course, under these circumstances we would not watch the ants. I examined the nest, however, every half hour very carefully, and am satisfied that there was no fighting. The next morning there was no dead ant; nor was there a death in the nest for more than a fortnight.

December 21. Marked three more in the same manner, and

put them in at 11.15 A.M. Looked at the usual intervals, but saw no fighting. The next morning there was no dead one outside the nest; but I subsequently found one of these ants outside, and nearly dead. I am, however, disposed to think that I had accidentally injured this ant.

Dec. 23. Painted three, and put them in at 10 A.M. At 11 they were all right, 12 do., 1 do., 2 do., 3 do., 4 do., 5 do. At 3 I put in three strangers for comparison: two of them were soon attacked; the other hid herself in a corner. The next morning I found two ants outside the nest; and they were two of the strangers. On the 31st I found the third stranger dead outside the nest. Found no other dead ant for some days.

Dec. 29. Painted three more of the ants from the pupæ separated on the 1st Oct., and put them in at 10.30 A.M. At 11 they were all right, 12 do., 1 do., 2 do. During the afternoon they were once or twice attacked for a minute or two, but let go again almost directly. The next morning I found one dead ant, but had no reason to suppose that she was one of the above three. The following morning there was again only one dead ant outside the nest; she was the third of the strangers put in on the 23rd as mentioned above. Up to the 23rd Jan. found no other dead one.

Jan. 3, 1879. Painted three more and put them in at 11.30 A.M. At 12 two were all right: we could not see the third; but no ant was being attacked. 12 do. 1, all three are all right; 2, do.; 5 do.

As already mentioned, for some days there was no dead ant brought out of the nest.

Jan. 5. Painted three more and put them in at 11.30 A.M. At 12 two were all right among the others; I could not find the third; but no ant was being attacked. 12.30 do., 1 do., 2 do., 4 do.

Jan. 6. Found two of them all right among the others. There was no dead ant.

Jan. 13. Painted three more and put them in at 12.30. At 1 they were all right. 2 do. 4, two were all right; I could not see the third, but she was not being attacked. The next morning, when I looked at the nest, one was being just carried, not dragged, out. The ant carried her about 6 inches and then put her down, apparently quite unhurt. She soon returned into the nest and seemed to be quite amicably received by the rest. Another one of the three also seemed quite at home. The third

I could not see; but up to the 23rd Jan. no dead one was brought out of the nest.

Jan. 19. Marked the last three of these ants and put them into the nest at 9.30 A.M. They were watched continuously up to 1. At that time two of them had been almost completely cleaned. One was attacked for about a minute soon after 11, and another a little later; but with these exceptions they were quite amicably received, and seemed entirely at home among the other ants.

Thus every one of these 32 ants was amicably received.

These experiments, then, seem to prove that ants removed from a nest in the condition of pupæ, but tended by friends, if reintroduced into the parent nest, are recognized and treated as friends. Nevertheless the recognition does not seem to have been complete. In several cases the ants were certainly attacked, though only by one or two ants, not savagely, and only for a short time. It seemed as if, though recognized as friends by the great majority, some few, more ignorant, or more suspicious, than the rest, had doubts on the subject, which however, in some manner still mysterious, were ere long removed. The case in which one of these marked ants was carried out of the nest, may perhaps be explained by her having been supposed to be ill, in which case, if the malady is considered to be fatal, ants are generally brought out of the nest.

It now remained to test the result when the pupæ were confided to the care of ants belonging to a different nest, though, of course, the same species.

I therefore took a number of pupæ out of some of my nests of *Formica fusca* and put them in small glasses, with ants from another nest of the same species. Now, as already mentioned, if the recognition were effected by means of some signal or password, then, as we can hardly suppose that the larvæ or pupæ would be sufficiently intelligent to appreciate, still less to remember it, the pupæ which were intrusted to ants from another nest, would have the password, if any, of that nest and not of the one from which they had been taken. Hence, if the recognition were effected by some password or sign with the antennæ, they would be amicably received in the nest from which their nurses had been taken, but not in their own.

I will indicate the nests by the numbers in my note-book.

On the 26th August last year, I put some pupæ of *Formica fusca*

from one of my nests (No. 36) with two workers from another nest of the same species. Two emerged from the chrysalis-state on the 30th; and on the 2nd September I put them, marked as usual, into their old nest (No. 36) at 9.30 A.M. At9.45 they seemed quite at home, and had already been nearly cleaned. At 10·15 the same was the case, and they were scarcely distinguishable. After that I could no longer make them out; but we watched the nest closely, and I think I can undertake to say that if they had been attacked we must have seen it.

Another one of the same batch emerged on the 18th August, but was rather crippled in doing so. On the 21st I put her into the nest (No. 36). This ant was at once attacked, dragged out of the nest, and dropped into the surrounding moat of water.

Again, on the 14th July last year (1878) I put some pupæ of *Formica fusca* from No. 36 into a glass with three ants of the same species from nest No. 60.

On the 22nd I put an ant which had emerged from one of these pupæ into her old nest (No. 36) at 9.30 A.M. She was at once attacked. 10, she is being dragged about. 10.30 do.

Aug. 8. Put another ant which had emerged from one of these pupæ into her old nest (No. 36) at 7.45 A.M. At 8 she seemed quite at home among the others. 8.15 do., 8.30 do., 9 do. 9.30 do.

Aug. 9. Put two other young ants of this batch into their old nest (No. 36) at 7 A.M. At 7.30 they were all right. At 7.30 one of them was being dragged by a leg, but only, I think, to bring her under shelter, and was then let go. Young ants of this species, when the nest is disturbed, are sometimes dragged to a place of safety in this way. At 8.30 they were all right and nearly cleaned. After this I could not distinguish them; but if they had been attacked, we must have seen it.

Aug. 11. Put in another one as before at 8.30 A.M. At 8·45 she was all right. At 9 she was dragged by a leg, like the last, but not for long; and at 9.30 she was quite comfortable amongst the others. 10 do., 10.45 do., 12 do., 5 do.

Aug. 24. Put in the last two ants of this lot as before at 9.15 A.M. At 9.30 they were all right. 9.45 do. At 10 they were almost cleaned. At 10.30 I could only distinguish one; and she had only a speck of colour left. She appeared quite at home; and though I could no longer distinguish the other, I must have seen it if she had been attacked.

Thus, then, out of seven ants of this batch put back into their

old nest, six were amicably received. On the other hand, I put one into nest No. 60, from which the three nurses were taken. She was introduced into the nest at 8.15 A.M., and was at once attacked. 8.45, she is being dragged about. 9, do.; 9.15, do.; 9.30, do. Evidently therefore she was not treated as a friend.

Again, on the 14th July last year (1878) put some pupæ of *Formica fusca* from nest No. 60 with three ants from nest No. 36.

On the 5th August at 4 P.M. I put an ant which had emerged from one of these pupæ, into her old nest (No. 60). At 5.15 she seemed all right. They were already cleaning her; and by 4.30 she was no longer distinguishable. We watched the nest, however, carefully for some time; and I feel sure she was not attacked.

Aug. 6. Put another of this batch into nest No. 60 at 7.15 A.M. At 7.30 she is not attacked. At 8, one of the ants was carefully cleaning her. At 8.15 she was quite at home among the others. At 8.30 do., she was nearly cleaned. 9.30 do.

Aug. 8. Put in another as before at 7.45. At 8 she is all right. 8.30 do., 9.30 do., 9.45 do.

Aug. 9. Put in another as before at 7 A.M. At 7.30 she is quite at home among the others, and already nearly cleaned. At 8 I could no longer distinguish her; but certainly no ant was being attacked. 9 do.

Aug. 11. Put in another as before at 8 A.M. At 8.15 she is quite at home. 8.30 do., 9 do., 9.30 do., 10 do., 12.30 do.

Aug. 13. Lastly, I put in the remaining young ant as before at 7 A.M. At 7.15 she was all right. At 7.30 do. and nearly cleaned. At 8 I could no longer distinguish her; but no ant was being attacked.

Thus, then, as in the preceding experiment, these six ants when reintroduced into the nest from which they had been taken as pupæ, were received as friends. On the other hand, on the 5th August I put a young ant of the same batch into nest No. 36, from which the three nurses had been taken. She was introduced at 11 and was at once attacked. At 11.30 she was being dragged about, and shortly after was put to death. I then introduced a second; but she was at once attacked like the first.

Aug. 22. I put some pupæ of *Formica fusca* from nest No. 64 under the charge of three ants from No. 60. By the 7th September several young ones had emerged. I put two of them into nest No. 64 at 8.15 A.M. They were amicably received, as in the pre-

ceding experiments, and the ants began to clean them. At 8.30 they were all right. 8.45 do. At 9 they had been completely cleaned so that I could not distinguish them; but there was no fighting going on in the nest.

On the same day, at 9.45 A.M. I put into nest 64 two more as before. At 10 they were both quite at home among the other ants. 10.15 do., 10.30 do., 11 do., 12 do., 1 do. I then put in a stranger; and she was at once fiercely attacked.

Sept. 8. Put in two more of the ants which had emerged from the pupæ, as before, at 9.30 A.M. At 9.45 they were all right. 10 do., 10.30 do., 11 do., 11.30 do., 12 do., 1 do.

On the other hand, on September 14, I put one of these ants in the same manner into nest No. 60 at 6.30 A.M. She was at once attacked. At 6.45 she was being dragged about by an antenna. 7 do. At 7.30 she was by herself in one corner. At 8.30 she was again being dragged about. 9.30 do. The difference, therefore was unmistakable.

Lastly, on July 29 I put some pupæ of *Formica fusca* from out of doors under the charge of three ants from nest No. 36.

Aug. 3. Several had come out, and I put two of them into the nest of their nurses (No. 36) at 2 P.M. Both were at once attacked. At 2.45 they were being dragged about. 3 do. 3.30 one was being dragged about. 4, both were being attacked. Eventually one was turned out of the nest. The other I lost sight of.

Aug. 4. Put two more of this batch into nest No. 36. at 12.30. One was at once attacked. 1, one is being dragged about by an antenna. 2.30, both are being attacked. At 2.45 one was dragged out of the nest.

I then put back one of the old ones; as might have been expected, she was received quite amicably.

I then tried the same experiment with another species, *Lasius niger*. I took some pupæ from two of my nests, which I knew not to be on friendly terms, and which I will call 1 and 2, and confided each batch to three or four ants taken from the other nest. When they had come to maturity I introduced them into the nests as before.

They were taken from their nest on the 20th Sept.; and the results were as follows.

Pupæ from nest 1 confided to ants from nest 2.

Sept. 20. Put one of the young ones into nest 2 at 7.15 A.M.

Several at once threatened her. At 7.25 one of the ants seized her by an antenna, and began dragging her about. 7.30, she was still being dragged about. 8, do. 8.15, she is now being dragged about by three ants. 8.30, she is still attacked. 9, do. At 9.15 she was dragged out of the nest.

Sept. 23. Put two of the young ants into nest 1 at 9.15 A.M. One was at once attacked, and the other a few minutes afterwards. 9.45, both are attacked. 10, do. One is now dead and hanging on to a leg of assailant. 10.15, do. 10.45, both are still being dragged about.

At 11 A.M. I put into nest 2 three more very young ones. At 11.10 one was attacked. At 11.20 all three were being viciously attacked, and yet one was nearly cleaned. At 12 one was being attacked, one was alone in a corner, the other we could not find. At 12.10 one was dragged out of the nest and then abandoned, on which, to my surprise, she ran into the nest again, which no old ant would have done. She was at once again seized by an antenna. At 12.30 she was still being dragged about; the second was being cleaned. In this instance, therefore, I think two out of the three were eventually accepted as inmates of the nest.

Sept. 25. Put two of the young ones into nest 1 at 2.30 P.M. At 2.45 one was attacked, but not viciously. 3 do., 3.15 do. No notice was taken of the other, though several ants came up and examined her. 3.30, the first is not attacked, the second is almost cleaned. 4, the first has been again attacked, but not viciously, and moreover has been partly cleaned. The second is evidently received as a friend, and is almost cleaned. 4.30, they are both comfortably among the others and are almost clean. At 5 I could no longer distinguish them.

I now pass to the other batch, namely, pupæ from nest 2 with ants from nest 1.

Sept. 25. Put three of the young ants into nest 1 at 9.30 A.M. At 9.45 two were attacked, the third was by herself. 10 do. At 10.15 one made her escape from the nest. At 10.20 the third was attacked. At 10.30 one of them was dragged out of the nest, and then abandoned. At 10·50 the third also was dragged out of the nest.

I then put two of these ants and a third young one into nest 2. At 11.15 A.M. they seemed quite happy; but at 11.30 two were being dragged about; the third, who was very young, was, on the contrary, being carefully cleaned. At 12 this last one was undis-

tinguishable; of the other two, one was being attacked, the second was taken no notice of, though several ants came up to her. At 12.5 the first was dragged out of the nest and then abandoned; the second was being carefully cleaned. This went on till 12.20, when the paint was entirely removed.

Sept. 27. I put in three more of these young ants into nest 1, at 7.45 A.M. At 8 o'clock they seemed quite at home among the other ants. A few minutes after, one was being held by a leg; the other two seemed quite at home. At 8.30 one was almost cleaned, one I could not see. At 9 two of them were quite at home, but I could not see the third. At 9.30 they were both all but cleaned; and after that we were no longer able to distinguish them.

Thinking the results might be different if the ants were allowed to become older before being returned into their nests, I made no further observations with these ants for two months. I then took two of the ants which had emerged from the pupæ separated on the 20th of September, and which had been brought up by ants from nest 2, and on the 22nd of November I put them back at 12 in their old nest (that is to say, in nest 1), having marked them as usual, with paint. They showed no signs of fear, but ran about among the other ants with every appearance of being quite at home. At 12.15 do. At 12.30 one was being cleaned. At 12.45 both were being cleaned; and by 1 o'clock they could scarcely be distinguished from the other ants. There had not been the slightest symptom of hostility. After this hour we could no longer identify them; but the nest was carefully watched throughout the afternoon, and I think I can undertake to say that they were not attacked. When we left off watching, the nest was enclosed in a box. The next morning I examined it carefully to see if there were any dead bodies. This was not the case; and I am satisfied, therefore, that neither of these two ants was killed. To test these ants, I then, on the 24th of November, at 8.30 A.M., put into the nest two ants from nest 2. At 8.40 one was attacked; the other had hid herself away in a corner. At 9.15 both of the ants were being dragged about. At 9.35 one was dragged out of the nest and then released, and the other a few minutes afterwards. After watching them for some time to see that they remained outside, I restored them to their own nest. The contrast, therefore, was very marked.

Again, on Nov. 25, I took two ants which had emerged from

pupæ belonging to nest 2, removed on the 20th September, and brought up by ants from nest 1, and put them back into their old nest at 2 P.M. They were watched continuously until 4 P.M., but were not attacked, nor even threatened. The following morning one of them was quite well, the other one had probably been cleaned. We could not distinguish her; but if she had been killed, we must have found her dead body. I then at 10 A.M. put in two more. At 10.30 one of them was attacked for a moment, but only for a moment. With this exception neither of them was attacked until 2 o'clock, when one of them was again seized and dragged about for a minute or two, but then released again. We continued watching them till half-past 4, when they seemed quite at home amongst the others. On the other hand a stranger, put in as a test at 12, was at once attacked. It was curious, however, that although she was undoubtedly attacked, yet at the very same time another ant began to clean her.

The next morning we found one ant lying dead in the box outside the nest; and this turned out to be the stranger of yesterday. She had been almost cleaned; but there were one or two infinitesimal particles of paint still remaining, so that there could be no doubt of her identity.

The next day, Nov. 27, I put in three more of the ants derived from these pupæ at 10 A.M. At 10.30 they were all right, running about amongst the others. At 11 o'clock the same was the case; but whilst I was looking again shortly afterwards, one of them was seized by an antenna and dragged a little way, but released again in less than a minute. Shortly afterwards one of the others was also seized, but let go again almost immediately. At 1 o'clock they were all right, and also at 2. They had, however, in the meantime been more than once threatened, and even momentarily seized, though they were never dragged about as strangers would have been. At 3 o'clock I found one of them dead; but I think I must have accidentally injured her, and I do not believe that she was killed by the other ants, though I cannot speak quite positively about it. The other two were all right, and had been partly cleaned. At 6 one of them was running about comfortably amongst the rest; the other I could not distinguish; but certainly no ant was being attacked.

Nov. 28. I put in the last two ants from the above-mentioned batch of pupæ at noon. Like the preceding, these ants were occasionally threatened, and even sometimes attacked

for a moment or two; but the other ants soon seemed to find out their mistake, and on the whole they were certainly treated as friends, the attacks never lasting more than a few moments. One of them was watched at intervals of half an hour until 5 P.M.; the other we could not distinguish after 3, the paint having been removed; but we should certainly have observed it had she been attacked.

On the whole, then, all the 32 ants belonging to *Formica fusca* and *Lasius niger*, removed from their nest as pupæ, attended by friends and restored to their own nest, were amicably received.

What is still more remarkable, of 22 ants belonging to *F. fusca*, removed as pupæ, attended by strangers, and returned to their own nest, 20 were amicably received. As regards one I am doubtful; the last was crippled in coming out of the pupa-case; and to this perhaps her unfriendly reception may have been due.

Of the same number of *Lasius niger* developed in the same manner from pupæ tended by strangers belonging to the same species, and then returned into their own nest, 19 were amicably received, three were attacked, and about two I feel doubtful.

On the other hand, 15 specimens belonging to the same two species, removed as pupæ, tended by strangers belonging to the same species, and then put into the strangers' nest, were all attacked.

The results may be tabulated as follows:—

	Pupæ brought up by friends and replaced in their own nest.	Pupæ brought up by strangers.	
		Put in own nest.	Put in strangers' nest.
Attacked.....	0	7*	15
Received amicably..	32	37	0

I propose next season to make some more experiments of this nature; but even the above results seem to me very interesting. The differences cannot be referred to any difference of temperament in different nests. For instance, any idea that the specimens of *Formica fusca* experimented with in August and September, and amicably received, were so on account of the peaceable character of the nests, is disposed of by the facts. Thus specimens of *F. fusca* experimented with in August and September last were taken principally from two nests, numbered respectively 36 and 60. Now, while nest 36, in most cases, amicably received ants bred from its own pupæ but tended by ants from 60, it showed itself fatally hostile to ants from pupæ

* About three of these I do not feel sure.

born in nest 60, even when these had been tended by ants from nest 36. Nest 60, again, behaved in a similar manner, as a general rule, amicably receiving its own young, even when tended by ants from 36; and refusing to receive ants born in nest 36, even when tended by specimens from 60.

These experiments seem to indicate that ants of the same nest do not recognize one another by any password. On the other hand, if ants are removed from a nest in the pupa-state, tended by strangers, and then restored, some at least of their relatives are certainly puzzled, and in many cases doubt their claim to consanguinity. I say some, because while strangers under the circumstances would have been immediately attacked, these ants were in every case amicably received by the majority of the colony, and it was sometimes several hours before they came across one who did not recognize them.

Suggestions as to the Relation &c. of second "Knot" and Sting.

I have elsewhere suggested* that the existence of a second "knot" in the Myrmicidæ stands perhaps in relation with their possession of a sting. The late Fred. Smith indeed, describes *Œcophylla*, which has only one knot, as having a sting; and I have the above-cited memoir admitted that this would be a difficulty, though not, I think, a conclusive argument against the suggestion. Forel† has since pointed out that the sting of *Œcophylla* is rudimentary. He rejects my view, however, on the ground that some ants which have two knots have only a rudimentary sting, such as *Pheidole*; while some of the Poneridæ have a well developed sting and yet only one knot.

In does not, however, seem to me that these cases are conclusive. The stings of ants are obviously homologous with those of Bees and other Hymenoptera. The sting may therefore be said to be more ancient than the ant; and as we may also assume that the ancestors of ants at one time had an abdomen of the more usual type, *i. e.* without a knot, the existence of ants with a sting and only one knot, so far from being inexplicable, is just what might have been expected. They represent in this respect an archaic phase through which the ancestors of *Myrmica* must have passed. The existence of a second knot, giving

* Monthly Micros. Journ. Sept. 1877.

† Zeit. f. wiss. Zool. 1878, vol. xxx. p. 30.

greater mobility to the sting, might have been an advantage, and thus gradually produced in certain cases, without necessarily being developed in others, in which, perhaps, some other advantage was enjoyed.

The so-called stingless ants, as Forel and Dewitz* have clearly shown, possess in reality a rudimentary sting; and their ancestors obviously had a more developed one. Such cases, therefore, as *Pheidole* and *Atta*, to which M. Forel refers, represent cases in which, perhaps with reference to the powerful development of the mandibles, the sting has fallen partly into disuse, and consequently has diminished in size. On the other hand, the second knot having once been formed, has retained its existence. It will be observed also that the "knot" in the Formicidæ, where it is single, is much more elevated than in the Myrmicidæ where there are two knots, and consequently two sets of muscles moving the abdomen. Thus, while the Myrmicidæ have two sets of muscles acting on the abdomen, and the Formicidæ only one, the difference is to a certain extent neutralized by the fact that the muscles in the latter family are longer than in the Myrmicidæ. This accounts I think, for the elevation of the knot or scale in *Formica* and the allied genera.

As to Sounds emitted by Ants.

In 'Nature' for December is a letter from Mr. T. S. Tait, who, writing from Baroda, says that by means of the microphone "we have been able to hear the roar of a black ant when attacked by its companion." It is unfortunate that Mr. Tait does not mention the species, because some of the Mutillidæ make a sound which is audible even to the naked ear. Moreover the expression "attacked by its companion" is curious, and does not harmonize with the usual habits of ants. Still I am quite disposed to believe that ants do produce sounds.

In the previous paper I have mentioned that I was never able to satisfy myself that my ants heard any sounds which I could produce. On the other hand, I have tried unsuccessfully various experiments, in order to ascertain whether the ants themselves produced any sounds for the purpose of conveying signs or ideas. Prof. Tyndall was good enough to arrange for me one of his sensitive flames; but I could not perceive that it responded in any

* Zeit. f. wiss. Zool. vol. xxviii.

way to my ants. The experiment was not, however, very satisfactory, as I was not able to try the flame with a very active nest. Prof. Bell most kindly set up for me an extremely sensitive microphone: it was attached to the underside of one of my nests; and though we could distinctly hear the ants walking about, we could not distinguish any other sound.

It is, however, far from improbable that ants may produce sounds entirely beyond our range of hearing. Indeed it is not impossible that insects may possess senses, or rather sensations, of which we can no more form an idea than we should have been able to conceive red or green if the human race had been blind. The human ear is sensitive to vibrations reaching to 38,000 in a second. The sensation of red is produced when 470 millions of millions of vibrations enter the eye in a similar time; but between these two numbers vibrations produce on us only the sensation of heat; we have no special organs of sense adapted to them. But there is no reason in the nature of things why this should be the case with other animals; and the problematical organs possessed by many of the lower forms favour the suggestion. If any apparatus could be devised by which the number of vibrations produced by any given cause could be lowered so as to be brought within the range of our ears, it is probable that the result would be most interesting.

Observations on the Kindness of Ants.

In my previous paper I have given various cases which seem to show that ants are not so uniformly humane as the descriptions of previous writers would seem to imply. Some of those who have done me the honour of noticing my papers have assumed that I disputed altogether the kindly feelings which have been attributed to ants. I should, however, be very sorry to treat my favourites so unfairly. So far as I can observe, ants of the same nest never quarrel. I have never seen the slightest evidence of ill-temper in any of my nests: all is harmony. Nor are instances of active assistance at all rare. Again, indeed I have myself given various cases showing care and tenderness on their part.

In one of my nests of *Formica fusca* was a poor ant which had come into the world without antennæ. Never having previously met with such a case, I watched her with great interest; but she never appeared to leave the nest. At length one day I found her wandering about in an aimless sort of manner, and apparently

not knowing her way at all. After a while she fell in with some specimens of *Lasius flavus*, who directly attacked her. I at once set myself to separate them; but whether owing to the wounds she had received from her enemies, or my rough, though well-meant handling, or both, she was evidently much wounded, and lay helplessly on the ground. After some time another *Formica fusca* from her nest came by. She examined the poor sufferer carefully, then picked her up tenderly and carried her away into the nest. It would have been difficult for any one who witnessed this scene to have denied to this ant the possession of humane feelings.

Again, if an ant is fighting with one of another species, her friends rarely come to her assistance. They seem generally (unless a regular battle is taking place) to take no interest in the matter, and do not even stop to look on. Some species, indeed, in such cases never appear to help one another; and even when this is the case, as for instance in the genus *Lasius*, the truth seems to be that several of them attack the same enemy—their object being to destroy the foe, not to save their friend.

Wasps and Bees.

Further Experiments as to their Knowledge of Colour, &c.

The experiments recorded in one of my previous papers (Journ. Linn. Soc. vol. xii. p. 510) tend to indicate that wasps are less guided by colour than bees. I thought, however, that it would be well to make some more experiments on the subject. On the afternoon, therefore, of the 1st September I put a wasp to some honey on a slip of glass placed over red paper, and, continually supplying fresh honey, allowed her to keep on coming till the 5th. I then moved the paper and the honey about 15 inches, putting another drop of honey on another slip of glass, over green paper, in the old place. She returned to the honey on the green paper. I then replaced the honey and red paper as before, and she came back quite straight to it. I then again moved it, and put honey on blue paper in the old place. She returned, however, quite straight to the honey, without taking any apparent notice of the change of colour. Sept. 7th, I moved the honey and paper about a foot, and put a drop of honey on glass over blue paper in between. She went to the honey on the blue paper. I then let her come again to the honey on the red three or four times, and then as before moved the paper about a foot, and put another drop of

honey over it, placing the old honey on yellow paper in between. She came to the honey on the red paper, but after feeding for about half a minute left it, to try that on the yellow.

I may mention that other observations of the same kind gave similar results; but it is perhaps hardly worth while to give more details.

Indeed, while hive-bees were generally contented with any honey I gave them, wasps showed a very different disposition, and, if there were several drops of honey near one another, flew frequently from one to the other, as if to make sure which they liked best.

Conduct towards their Friends.

With reference to the behaviour as regards comrades, I may observe that the results entirely confirmed those previously arrived at. For instance, a wasp observed and fed from the 7th to the 12th Sept. did not bring more than three or four friends during the whole of that time.

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OBSERVATIONS

ON

ANTS, BEES, AND WASPS.

Part VIII.

BY

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Observations on Ants, Bees, and Wasps.—Part VIII. By Sir John Lubbock, Bart., Pres. Linn. Soc., M.P., F.R.S., D.C.L., LL.D.

[Read June 2, 1881.]

Experiments with Light of different Wave-lengths.

In one of my former papers (Linnean Journ. vol. xiv. p. 278) I have given a series of experiments made on ants with light of different colors, in order, if possible, to determine whether ants have the power of distinguishing colors. For this purpose I utilized the dislike which ants, when in their nest, have for light. Not unnaturally, if a nest is uncovered, they think they are being attacked, and hasten to carry their young away to a darker and, as they suppose, a safer place. I satisfied myself, by hundreds of experiments, that if I exposed to light the greater part of a nest, but left any of it covered over, the young would certainly be con-

veyed to the dark part. In this manner I satisfied myself that the various rays of the spectrum act on them in a different manner from that in which they affect us; for instance, that ants are specially sensitive to the violet rays.

But I was anxious to go beyond this, and to attempt to determine how far their limits of vision are the same as ours. We all know that if a ray of white light is passed through a prism, it is broken up into a beautiful band of colors—the spectrum. To our eyes this spectrum is bounded by red at the one end and violet at the other, the edge being sharply marked at the red end, but less abruptly at the violet. But a ray of light contains, besides the rays visible to our eyes, others which are called, though not with absolute correctness, heat-rays and chemical rays. These, so far from falling within the limits of our vision, extend far beyond it, the heat-rays at the red, the chemical rays at the violet end.

I had already tried various experiments with spectra derived from sunlight; but, owing to the movement of the earth, they were not thoroughly satisfactory. Mr. Spottiswoode was also good enough to enable me to make some experiments with electric light, which have been already recorded; and I have now to bring before the Society some additional and much more complete experiments, which, through the kindness of Prof. Dewar, Prof. Tyndall, and the Board of Managers, to whom I beg to offer my most cordial thanks, I have been enabled to make in the Laboratory of the Royal Institution.

Prof. Dewar was also kind enough to test my glasses and solutions with reference to their power of transmitting color. Taking the wave-length of the extreme visible red as 760 and that of the extreme violet as 397, we have

760 to 647 give red.
647 „ 585 „ orange.
585 „ 575 „ yellow.
575 „ 497 „ green.
497 „ 455 „ blue.
445 „ 397 „ violet.

The result of his examination of my glasses and solutions was as follows:—

The light-yellow glass cut off the high end down to wave-length 442.

The dark-yellow glass cut off the high end down to wave-length 493.

The green glass cut off the high end down to wave-length 465, and also the red to 616.

The red glass cut off the high end down to wave-length 582.

The violet glass cut off the orange and yellow from wave-length 684 to 583, and a band between wave-lengths 543 and 516.

The purple glass cut off the high end down to wave-length 528.

The solution of chromate of potash cut off the high end to 507.

The saffron cut off the high end to about 473.

Blue fluid cut off the low end to 516.

Red fluid cut off the high end to 596.

In my previous experiments with colored spectra, the ants carried the pupæ out of the portion of the nest on which coloured light was thrown and deposited them against the wall of the nest; or, if I arranged a nest of *Formica fusca* so that it was entirely in the light, they carried them to one side or into one corner. It seemed to me, therefore, that it would be interesting so to arrange matters, that on quitting the spectrum, after passing through a dark space, the ants should encounter not a solid obstacle, but a barrier of light. With this object, I prepared some nests 12 inches long by 6 inches wide; and Mr. Cottrell kindly arranged for me at the Royal Institution on the 29th of June, by means of the electric light, two spectra, which were thrown by two glass prisms on to a table at an angle of about 45°. Each occupied about 6 inches square, and there was a space of about 2 inches between the red end of the one and the violet of the other, the more distant spectrum being a good deal the brightest.

Exp. 1.—In the light space I placed a nest of *Formica fusca*, 12 inches by 6, containing about 150 pupæ, and arranged it so that one end was distinctly beyond the limit of the violet visible to us, and all but to the edge of the green given by thalline paper*, and the other just beyond the visible red. The pupæ at first were almost all in or beyond the violet, but were carried into the dark space between the two spectra, the bright thalline band being avoided, but some pupæ being deposited in the red.

Exp. 2.—I then tried the same experiment with a nest of *Lasius niger*, in which there were many larvæ as well as pupæ. They were all at the commencement at the blue end of the nearer spectrum.

* If paper steeped in thalline is placed in the ultra-violet portion of the spectrum, it gives, with rays of a certain wave-length, a distinctly visible green colour, which therefore constitutes a green band.

The larvæ were left by themselves in the violet, while pupæ were ranged from the end of the green to that of the red inclusive.

Exp. 3.—Arranged a nest of *L. niger* as before; at the commencement the pupæ and larvæ were much scattered, being, however, less numerous in the violet and ultra-violet rays. Those in the ultra-violet rays were moved first, and were deposited, the larvæ in the violet, and the pupæ in the red.

Exp. 4.—Made the same experiment with another nest of *L. niger*. At the commencement the larvæ and pupæ were in the violet and ultra-violet portion, extending to double the distance from the visible end to the thalline band. The ants soon began bringing the pupæ to the red. Over part of the red I placed a piece of money. The pupæ were cleared from the ultra-violet first. That the pupæ were not put in the red for the sake of the red light was evident, because the space under the coin was even more crowded. The pupæ were heaped up in the dark as far as the thalline-band of the other spectrum. I then brought the second spectrum nearer to the first. The pupæ which thus found themselves in the thalline band were gradually moved into the dark.

Exp. 5.—Tried the same with another nest of *L. niger*. The pupæ were at first in the violet and ultra-violet about double as far as the thalline line, while most of the larvæ were in the green. The experiment began at 1.15. The furthest part was cleared first; and they were again brought principally into the yellow, red, and dark.

Again, I arranged them pretty equally from double the distance of the thalline from the violet as far as the blue of the other spectrum, most, however, being in the violet and blue and a few scattered all over.

The pupæ in the red were not moved. The others were carried beyond the thalline band into the yellow or red.

Exp. 6.—Repeated the same experiment. Begun it 11.15. Placed some pupæ in the red, some in the yellow, and a few scattered over the second spectrum; there were none in the nearer one.

They were all carried away from the red past the violet, and put down in the dark portion, or in the red and yellow, of the nearer spectrum.

These experiments surprised me much at the time, as I had expected the pupæ to be carried into the space between the two

spectra; but it afterwards occurred to me that the ultra-violet rays probably extended further than I had supposed, so that even the part which lay beyond the thalline band contained enough rays to appear light to the ants. Hence perhaps they selected the red and yellow as a lesser evil.

Exp. 7.—I altered, therefore, the arrangement. Prof. Dewar very kindly prepared for me a condensed pure spectrum (showing the metallic lines) with a Siemens's machine, using glass lenses and a mirror to give a perpendicular incidence when thrown on the nest. I arranged the pupæ again in the ultra-violet as far as the edge of the fluorescent light shown with thalline paper. The pupæ were all again removed, and most of them placed just beyond the red, but none in the red or yellow.

Exp. 8.—Arranged the light as before, and placed the pupæ in the ultra-violet rays. In half an hour they were all cleared away and carried into the dark space beyond the red. We then turned the nest round and placed the part occupied by the pupæ again in the violet and ultra-violet. The light chanced to be so arranged that along one side of the nest was a line of shadow; and into this the pupæ were carried, all those in the ultra-violet being moved. We then shifted the nest a little, so that the violet and ultra-violet fell on some of the pupæ. These were then all carried into the dark, the ones in the ultra-violet being moved first.

It is noticeable that in these experiments with the vertical incidence there was less diffused light, and the pupæ were in no case carried into the red or yellow.

Exp. 9.—I arranged the light and the ants as before, placing the pupæ in the ultra-violet, some being distinctly beyond the bright thalline band. The ants at once began to remove them. At first many were deposited in the violet, some, however, being at once carried into the dark beyond the red. When all had been removed from the ultra-violet, they directed their attention to those in the violet, some being carried, as before, into the dark, some into the red and yellow. Again, when those in the violet had all been removed, they began on the pupæ in the red and yellow, and carried them also into the dark. This took nearly half an hour. As I had arranged the pupæ, and it might be said that they were awkwardly placed, we then turned the nest round, leaving the pupæ otherwise as they had been arranged by the ants; but the result of moving the nest was to bring some of them into the violet, though most were in the ultra-violet; while beyond them

was a space of about an inch, which, in Prof. Dewar's opinion, was beyond the limit of the transparency of glass to the ultra-violet rays, and would therefore be as free from rays as the part beyond the red. They were, as before, all carried into the dark space beyond the red in about half an hour.

We then turned the glass round again, this time arranging the end about the length of the spectrum beyond the end of the violet visible to our eyes. They began clearing the thalline band, carrying some into the violet, but the majority away further from the spectrum. In a quarter of an hour the thalline band had been quite cleared; and in half an hour a band beyond, and equal to the thalline band, those in the violet being left untouched. After the pupæ in the ultra-violet portion had all been moved, those in the violet were also carried away and deposited about twice as far from the edge of the violet as the width of the bright thalline band.

Exp. 10.—Experimented again with the same arrangement as before, using another nest of *Lasius niger* and placing the pupæ in the violet and a little beyond. The ants at once began removing them into the dark, tunnelling into the heap, and then carrying away those in the ultra-violet first, although they were further off. In half an hour they had all been moved out of the violet and ultra-violet, about half being in the dark, and half having been provisionally placed in the red and yellow.

Exp. 11.—Same arrangement as before. The pupæ being placed all along one side of the nest, from the edge of the red to a distance beyond the violet as great as the whole length of the spectrum. I began at 4.15. By degrees they were all cleared away from the spectrum, except those in the violet, where indeed, and immediately outside of which, the others were placed. At 5, however, they began to carry them back into the red. At 5.45 the blue and violet were nearly cleared, the pupæ being placed in the red and yellow. At 6.15 they had all been brought from the violet and ultra-violet into the red and yellow.

I then shook up the pupæ so that they were arranged all along one side of the nest, and extended about an inch beyond the red. This excited them very much, and in less than ten minutes all those in the spectrum, and for about 6 inches beyond the violet, were moved, but at first put down anywhere, so that they were scattered all over the nest. This, however, lasted for a very short time, and they were all carried into the dark beyond the red or

into the extreme end beyond the violet. At 7 they followed the line of the red at one end, coming about $\frac{1}{4}$ inch within it, which was not owing to want of room, as one side of the nest was almost unoccupied; at the other end they were all carried 3 inches beyond the end of the violet.

I then arranged the same ants in a wooden frame consisting of a base and two side walls, between which in the middle was a perpendicular sliding door. The pupæ had been arranged by the ants in the centre of the nest, so that some were on each side of the door. We then, by means of a strong induction-coil, threw a magnesium-spark on the nest from one side, and the light from a sodium-flame in a Bunsen burner on the other, the light being in each case stopped by the door, which was pressed close down on the nest. In this way the first half was illuminated by the one light, the second by the other, the apparatus being so arranged that the lights were equal to our eyes—that, however, given by the magnesium, consisting mainly of blue, violet, and ultra-violet rays, that of the sodium being very yellow and poor in chemical rays. In a quarter of an hour the pupæ were all carried into the yellow. The sodium light being the hotter of the two, to eliminate the action of heat I introduced a water-cell between the ants and the sodium-flame, and made the two sides as nearly as possible equally light to my eye. The pupæ, however, were again carried into the sodium side.

I repeated the same experiment as before, getting the magnesium-spark and the sodium-flame to the same degree of intensity, as nearly as my eye could judge, and interposing a water-screen between the sodium-flame and the ants. The temperature was tested by the thermometer; but I could distinguish no difference between the two sides. Still the ants preferred the sodium side. This I repeated twice. I then removed the magnesium-spark somewhat, so that the illumination on that side was very much fainter than on the other; still the pupæ were carried into the sodium-light. I then turned the nest round so as to bring them back into the magnesium. They were again carried to the sodium side.

Once more I repeated the same experiment. The light on the magnesium side was so faint that I could scarcely see the pupæ, those on the sodium side being quite plain. The thermometer showed no difference between the two sides. The pupæ were carried into the sodium-light. I then turned the nest round

twice; but the pupæ were each time carried out of the magnesium-light.

These experiments seemed strongly to indicate, if not to prove, that ants were really sensitive to the ultra-violet rays. Now to these rays sulphate of quiniue and bisulphide of carbon are extremely opaque, though perfectly transparent in the case of visible rays, and therefore to our eyes entirely colourless and transparent. If, therefore, the ants were really affected by the ultra-violet rays, then a cell containing a layer of sulphate of quinine or bisulphide of carbon would tend to darken the underlying space to their eyes, though to ours it would not do so. It will be remembered that if an opaque substance is placed over a part of a glass nest, other things being equal, the ants always congregate under it; aud that if substances of different opacity are placed on different parts of a nest, they collect under that which seems to them most opaque.

Over one of my nests of *Formica fusca*, therefore, I placed two pieces of dark-violet glass 4 inches by 2 inches; and over one of them I placed a cell containiug a layer of bisulphide of carbon, an inch thick, slightly coloured with iodine. In all these experiments, when I moved the liquids or glasses, I gave the advantage, if any, to the one uuder which experience showed that the ants were least likely to congregate. The ants all collected under the glass over which was the bisulphide of carbon.

I then thought that though no doubt the iodine rendered the bisulphide more completely impervious to the ultra-violet rays, I would try the effect of it when pure and perfectly colourless. I therefore tried the same experiment with pure bisulphide, moving the two glasses from time to time in such a manner that the ants had to pass the first violet glass in order to reach that over which was the bisulphide.

At 8.30 the ants were all under the glass over which was the bisulphide of carbon: I then changed the position.

8.45	ditto	ditto	ditto.
9	ditto	ditto	ditto.
9.15	ditto	ditto	ditto.

Although the bisulphide of carbon is so perfectly transparent, I then thought I would try it without the violet glass. I therefore covered part of the nest with violet glass, a part with a layer

of bisulphide of carbon, moving them from time to time as before.

At 9.45 the ants were all under the bisulphide: I then changed the position.

10.15	ditto	ditto	ditto.
10.45	ditto	ditto	ditto.
11.15	ditto	ditto	ditto.

I then reduced the thickness of the layer of bisulphide to $\frac{4}{10}$ of an inch.

At 1.30 the ants were all under the bisulphide: I then changed the position.

2	ditto	ditto	ditto.
2.30	ditto	ditto	ditto.
3	ditto	ditto	ditto.

Then thinking that possibly it might make a difference, the one shelter being a plate of glass and the other a liquid, I tried two similar bottles, one containing water and the other bisulphide of carbon; but in every case the ants went under the bisulphide of carbon. On the other hand, when I used a solution of ammonio-sulphate of copper so deep in colour that the ants were only just visible through it, the ants went under the coloured liquid.

Oct. 10. I uncovered the nest at 7 A.M., giving the ants an option between the bisulphide and a solution of ammonio-sulphate of copper.

At 7.30 the ants were all under the solution of ammonio-sulphate of copper. Changed the places.

8	ditto	ditto	ditto.
8.15	ditto	ditto	ditto.

I then replaced the solution of sulphate of copper by one of carmine so deep that the ants could only just be seen through it.

At 8.30 they were under the carmine. I shifted the carmine and bisulphide.

8.45	ditto	ditto	ditto.
9	ditto	ditto	ditto.
9.15	ditto	ditto	ditto.
9.30	ditto	ditto	ditto.

I now took a bright-green solution of chlorate of copper:—

At 10 they were under the chlorate of copper. I shifted the liquids.

10.15	ditto	ditto	ditto.
12.30	ditto	ditto	ditto.
12.45	ditto	ditto	ditta.

Subsequently I used saffron instead of the chlorate of copper:—

At 11	they were under the saffron.	I shifted the liquids.	
11.15	ditto	ditto	ditto.
11.25	ditto	ditto	ditto.
11.35	ditto	ditto	ditto.

I now took successively red, yellow, and green glass; but in every case the ants preferred the glass to the bisulphide. Although, therefore, it would seem from the previous experiments that the bisulphide darkened the nests to the ants more than violet glass, it would appear to do so less than red, green, or yellow.

I now made some experiments in order, if possible, to determine whether the reason why the ants avoided the violet glass was because they disliked the colour violet, or whether it was because the violet glass transmitted more of the ultra-violet rays.

For this purpose I placed a layer of the bisulphide of carbon over a piece of violet glass. By this arrangement I got the violet without the ultra-violet rays; and I then contrasted this combination with other coloured media.

First, I took a solution of bichromate of potash (bright orange), and placed it on a part of the nest side by side with the violet glass and bisulphide of carbon. I should add that the bichromate of potash also cuts off the ultra-violet rays. In all the following observations I changed the position after each observation.

At 1.30 P.M.	the ants were	under the	bichromate.
3	„	„	half under the bichromate and half under the violet glass and bisulphide.
8 A.M.	„	„	under the bichromate.
8.30	„	„	under the violet glass and bisulphide.
9	„	„	half under each.
9.30	„	„	some under each, but most under the violet glass and bisulphide.
9.45	„	„	half under each.
10	„	„	„ „

In this case, therefore, though without the layer of bisulphide the violet glass would always have been avoided, the result of

placing the bisulphide over the violet glass was that the ants did not care much whether they were under the violet glass or under the bichromate of potash.

I now took the same solution of carmine which I had already used.

10. The ants were under the carmine.
10.15 „ „ „
10.30 „ most under the carmine, but some under the violet.
10.45 „ under the carmine.
11 „ most under the carmine, but some under the violet.

Here, then, again the bisulphide made a distinct difference, though not so much so as with the bichromate of potash.

I now took the solution of chlorate of copper already used.

1. About half the ants were under each.
1.30. The greater number were under the violet glass and bisulphide.
2. ditto ditto ditto.
2.30. ditto ditto ditto.
3. Almost all were under the violet glass and bisulphide.

Here, then, the addition of the bisulphide caused the violet glass to be distinctly preferred to the chlorate of copper.

I then took a solution of sulphate of nickel, almost exactly the same tint, or a shade paler than, the chlorate of copper.

At 3.45 the ants were under the violet glass and bisulphide.
4 ditto ditto ditto.
5 ditto ditto ditto.

Oct 18.
7 A.M. ditto ditto ditto.
8. About half of the ants were under each.

Here the same result was even more marked.

I then took some saffron 1 inch in thickness and of a deep-yellow colour.

12.45. The ants were about half under each.
1. Most of the ants were under the violet glass and bisulphide.
1.15. ditto ditto ditto.
2. Most of the ants were under the saffron.

Here, again, we have the same result.

I then tried the different-coloured glasses, all of which, as I had

previously found, are unmistakably preferred to the violet. It remained to see what effect placing the bisulphide of carbon on the violet would have.

First, I placed side by side, as usual, a piece of green glass and the violet glass covered with bisulphide of carbon :—

1st exp. Half of the ants were under each.
2nd „ They were under the violet glass and bisulphide.
3rd „ „ „ „
4th „ Most of them „ „
5th „ „ „ „

Next, I tried pale-yellow glass.

1st obs. The ants were almost all under the violet glass and bisulphide.
2nd „ About three quarters were „ „
3rd „ They were all „ „
4th „ About half were under each.

I then took the dark-yellow glass.

1st obs. About half the ants were under the yellow glass and half under the violet glass and bisulphide.
2nd „ Most of them were under the violet glass and bisulphide.
3rd „ „ „ yellow glass.
4th „ „ „ violet glass and bisulphide.
5th „ About half under each.

I now took deep-red glass.

1st obs. The ants were under the red glass.
2nd „ Half of the ants were under each.
3rd „ Most of the ants were under violet glass and bisulphide.
4th „ Half were under each.

It seemed evident, therefore, that while if violet glass alone was placed side by side with red, yellow, or green, the ants greatly preferred any of the latter, on the other hand, if a layer of bisulphide of carbon, which to our eyes is perfectly transparent, was placed over the violet glass, they then went as readily, or even more readily, under it than under other colours.

In order to be sure that it was not the mere presence of a fluid, or the two layers of glass, to which this was due, I thought it would be well to try a similar series of experiments, using, however, a

layer of similar thickness (1 inch) of water coloured light blue by ammonio-sulphate of copper.

I therefore took again the piece of violet glass, over which I placed a flat-sided bottle, about 1 inch thick, containing a light-blue solution of ammonio-sulphate of copper; and, in contrast with it, I used the same coloured glasses as before.

First, I took the red glass.

Observation 1. Some of the ants were under each, but most under the red glass.
„ 2. All under the red glass.
„ 3. Almost all under the red.
„ 4. „ „
„ 5. „ „

I now took the green glass.

Observation 1. Almost all were under the green.
„ 2. All were under the green.
„ 3. Two thirds were under the green.
„ 4. All „ „
„ 5. „ „ „
„ 6. „ „ „

These experiments were made on a gloomy day; so I repeated them on a bright one, when the contrast was more marked.

Observation 7. All were under the green glass.
„ 8. „ „
„ 9. „ „ except two or three.
„ 10. „ „
„ 11. „ „
„ 12. „ „

I now took the dark-yellow glass.

Observation 1. All were under the yellow glass.
„ 2. „ „
„ 3. „ „
„ 4. „ „

I now took the light-yellow glass.

Observation 1. They were all under the light-yellow glass.
„ 2. „ „ „
„ 3. „ „ „
„ 4. „ „ „

These experiments seem to demonstrate that in the previous series the ants were really influenced by some property inherent in the bisulphide of carbon, and which affected their eyes, though it was insensible to ours.

I then thought it would be interesting to use, instead of the bisulphide, a solution of sulphate of quinine ($\frac{1}{2}$ dr. to 4 ounces), which differs from it in many points, but agrees in cutting off the ultra-violet rays. I used, as before, a layer about an inch thick, which I placed over violet glass, and then placed by its side the same coloured glasses as before.

First, I took the red glass.

Obs. 1. About half the ants were under each.
„ 2. Most of them were under the red glass.
„ 3. About half under each; rather more under the violet glass and sulphate of quinine than under the red glass.
„ 4. ditto ditto ditto.

I now took the dark-yellow glass instead of the red.

Obs. 1. Most of the ants were under the violet glass and sulphate of quinine.
„ 2. All „ „ „
„ 3. „ „ „ „
„ 4. „ „ „ yellow glass.
„ 5. „ „ „ „
„ 6. All of the ants were under the violet glass and sulphate of quinine.
„ 7. About half under each.
„ 8. Rather more under the violet glass and sulphate of quinine than under the yellow glass.

I then took the light-yellow glass instead of the dark.

Obs. 1. The ants were all under the violet glass and sulphate of quinine.
„ 2. Rather more than half under the yellow glass.
„ 3. Almost all under the violet glass and sulphate of quinine.
„ 4. All „ „ „

I then took the green glass instead of the yellow.

Obs. 1. They were under the violet glass and sulphate of quinine.
„ 2. „ „ „
„ 3. About half under each.
„ 4. About three quarters under the green glass.
„ 5. Almost all under the violet glass and sulphate of quinine.

I then tried similar experiments with a saturated solution of chrome alum and chromium chloride. These are dark greenish blue, very opaque to the visible light-rays, but transparent to the ultra-violet. I used a layer ¼ inch thick, which was still so dark that I could not see the ants through it; and for comparison, a solution 1 inch thick of bisulphide of carbon, moving them after each observation as before.

Exp. 1. The ants were under the bisulphide of carbon.
„ 2. „ „ „
„ 3. Most „ „ „
„ 4. All but three „ „ „
„ 5. All „ „ „

I now took chromium chloride instead of chrome alum.

Exp. 1. Most were under the bisulphide of carbon.
„ 2. All „ „ „
„ 3. Almost all „ „ „
„ 4. About three fourths were under the chromium chloride.
„ 5. All were under the chromium chloride.
„ 6. About two thirds „ „
„ 7. About one half under each.
„ 8. All under the bisulphide of carbon.
„ 9. About three fourths under the bisulphide of carbon.
„ 10. About half „ „ „
„ 11. All under the chrome alum.
„ 12. „ bisulphide of carbon.

Thus, then, while if the ants have to choose between the violet and other coloured glasses, they will always prefer one of the latter, the effect of putting over the violet glass a layer either of sulphate of quinine or bisulphide of carbon, both of which are quite transparent, but both of which cut off the ultra-violet rays, is to make the violet glass seem to the ants as good a shelter as any of the other glasses. This seems to me strong evidence that the ultra-violet rays are visible to the ants.

Prof. Paul Bert has made ('Archiv de Physiol.' 1869, p. 547) some very interesting experiments on a small freshwater Crustacean belonging to the genus *Daphnia*, from which he concludes that they perceive all the colours known to us, being, however, specially sensitive to the yellow and green, and that their limits of vision are the same as ours.

Nay, he even goes further than this, and feels justified in con-

cluding from the experience of two widely divergent species—Man and *Daphnia*—that the limits of vision would be the same in all cases.

His words are :—

A. "Tous les animaux voient les rayons spectraux que nous voyons."

B. "Ils ne voient aucun de ceux que nous ne voyons pas."

C. "Dans l'étendue de la région visible, les différences entre les pouvoirs éclairants des différents rayons coloriés sont les mêmes pour eux et pour nous."

He adds, that "puisque les limites de visibilités semblent être les mêmes pour les animaux et pour nous, ne trouvons-nous pas là une raison de plus pour supposer que le rôle des milieux de l'œil est tout-à-fait secondaire, est que la visibilité tient à l'impressionnabilité de l'appareil nerveux lui-même?"

Such a generalization would seem to rest on but a slight foundation ; and I may add that I have made some experiments myself on Daphnias which do not agree with those of M. Bert. I hope on some future occasion to have the honour of laying them before the Society.

At any rate, it seems to me that the preceding evidence strongly indicates that ants perceive the ultra-violet rays. Now, as every ray of homogeneous light which we can perceive at all appears to us as a distinct colour, it seems probable that these ultra-violet rays must make themselves apparent to the ants as a distinct and separate colour (of which we can form no idea), but as unlike the rest as red is from yellow, or green from violet. The question also arises whether white light to these insects would differ from our white light in containing this additional colour. At any rate, as few of the colours in nature are pure colours, but almost all arise from the combination of rays of different wave-lengths, and as in such cases the visible resultant would be composed not only of the rays which we see, but of these and the ultra-violet, it would appear that the colours of objects and the general aspect of nature must present to them a very different appearance from what it does to us.

Sense of Direction.

In continuation of the experiments recorded in my last paper (Linnean Journ. vol. xv. p. 177), I caused to be constructed a circular table 18 inches in diameter, the arrangement of which was kindly devised for me by Mr. Francis Galton. It consisted, as shown in figs. 1 and 2, of three concentric pieces—a central F G,

an intermediate D E, H I, and an outer piece B C, K L, each of these three pieces being capable of separate rotation.

Fig. 1.

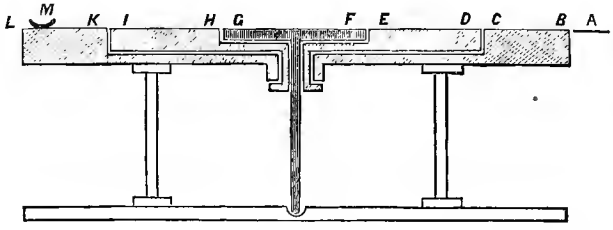
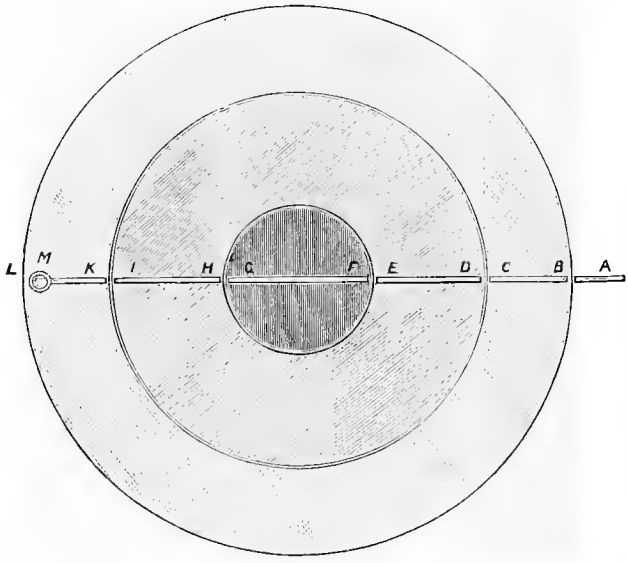


Fig. 2.



I then connected the table with a nest of *Lasius niger* by a paper bridge A, and also made a paper path across the table, as shown in fig. 2, divided into five pieces corresponding to the divisions of the table. This I did because I found that the ants wandered less if they were provided with a paper road than if they walked actually on the wood itself. I then placed a cup containing larvæ on the table at B, and put an ant on the larvæ. She at once picked one up, and, with some little guidance from

me, carried it off to the nest, returning at once for another, bringing some friends with her to help. When she knew her way, I gradually moved the cup across the table along the paper bridge to M. After a while the ants came to know the way quite well, and passed to and fro quite straight along the paper path from the nest to the larvæ at M. Having thus established a service of ants, I tried the following experiments:—

1. I removed the piece of paper G F. This disturbed them; but they very soon reestablished the chain.

2. I turned round the central piece of the table G F, so that the paper G F was reversed, G being where F had been, and *vice versâ*. This did not seem to disconcert the ants at all.

3. When the ants were between I and B, I rotated the outer circle of the table halfway round, which of course carried the cup containing the larvæ from L to B. The ants took no notice of this, but went straight to L.

4. When the ants were between I and B, I rotated the table several times, bringing it finally to the original position. This disturbed them a good deal; but eventually they all continued their course to L.

5. When the ants were between I and D, I half rotated the two centre parts of the table, the result of which, of course, was that the ant was moving away from, instead of towards, the nest. In every case the ants turned round too, so as duly to reach L. So also those which were on their way from the nest to the larvæ turned in the same manner.

6. When the ants were between I and D, I half rotated the whole table. Again the ants turned round too, though of course in this case, when they reached the place where L had been, the cup with the larvæ was behind them at B.

These two experiments, though quite in accordance with those previously made, puzzled me a good deal. Experimeut 3, as well as those recorded in previous papers, seemed to show that ants were little guided iu such cases by the position of surrounding objects. However, I was anxious to test this.

7. Accordingly I took a round box and placed it upside down on the table, having cut two niches, one at each side, where it lay on the paper path, so as to afford a passage for the ants, as in the experiments recorded in my previous paper; but on this occasion I left the lid on, cutting, however, a hole through which I could watch the result. In this case, therefore, the surrounding objects, *i. e.*

the walls of the box, turned round with the table. Then, as before, when the ants were between I and D, I turned the table half round. The results were as follow :—

	Ants which turned.	Ants which did not turn.
Exp. 1.	1	2
„ 2.	1	1
„ 3.	1	1
„ 4.	4	2
„ 5.	0	1
„ 6.	0	1
„ 7.	0	3
„ 8.	1	1
„ 9.	0	1
„ 10.	2	2
„ 11.	1	1
„ 12.	0	3
	11	19

In this case, then, only 11 ants turned; and as 4 of them were together, it is possible that 3 simply followed the first. Moreover, the ants which turned did so with much more hesitation and less immediately.

8. For comparison, I then again tried the same experiment, but without the box. The results were as follows :—

	Ants which turned.	Ants which did not turn.
Obs. 1.	3	0
„ 2.	3	0
„ 3.	3	1?
„ 4.	3	0
„ 5.	4	0
„ 6.	4	0
	20	1?

Under these circumstances, therefore, all the ants but one certainly turned, and her movements were undecided.

From these last two experiments it is obvious that the presence of the box greatly affected the result, and yet the previous results made it difficult to suppose that the ants noticed any objects so distant as the walls of the rooms, or even as I was myself. The

result surprised me considerably; but I think the explanation is given by the following experiments.

I again put some larvæ in a cup, which I placed in the centre of the table; and I let out an ant which I had imprisoned after the previous experiments, placing her in the cup; she carried off a larva to the nest and soon returned. When she was again in the cup, I rotated the table; when she came out she seemed a little surprised; but after walking once round the cup, started off along the paper bridge straight home. When she returned to the cup, I again half rotated the table. This time she went back quite straight. When she had come again, I once more half rotated the table; she returned quite straight. Again the same happened. A second ant then came: I half rotated the table as before. She went wrong for about an inch and a half, but then turned round and went straight home.

I was working by the light of two candles which were on the nest-side of the table. The next time the two ants came, I half rotated the table as before and moved the candles to the far side. This time the ants were deceived, and followed the paper bridge to the end of the table furthest from the nest. This I repeated a second time, with the same result. I then turned the table as before without altering the lights, and the ants (four of them) went back all right. I then again turned the table, altering the lights, and the ant went wrong.

I then altered the lights without rotating the table: the first ant went wrong; the second right; the third wrong; the fourth wrong; the fifth hesitated some seconds, and then went wrong; the sixth right; the seventh went all but to the edge the wrong way, but, after various wanderings, at last went right. When, therefore, the direction of the light was changed, but every thing else left as before, out of seven ants, five were deceived and went in the wrong direction.

After an interval of a week, on March 25, I arranged the nest and the rotating table as before, and let out three ants which I I had imprisoned on the 19th, and which knew their way. I put them on the larvæ at M as before. The paper pathway had been left untouched. The ants examined the larvæ and then went straight home along the paper path; but, to my surprise, only one of them carried off a larva. Nevertheless they had evidently taken the news to the nest; for the ants at once began coming to the cup in considerable numbers and carrying off the

larvæ. I do not altogether understand this proceeding, and unluckily had not marked the first three ants; so that I cannot tell whether they brought or sent their friends. It seems possible that they felt unequal to the exertion of carrying a burthen to the nest until they had had some food.

When the ants were fairly at work, I turned the table 90 degrees. In this case eight ants continued their march along the paper, while two turned back; but none left the paper, and went across the table straight for the larvæ.

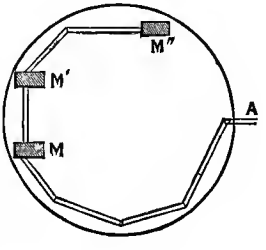
I then stopped the experiment for a while, so that the excitement might subside; as when the ants become too numerous it is not so easy to watch them.

When all was quiet, I put the cup with the larvæ on the middle of the table, and covered the greater part of the table with the box as before (p. 379). When the ants were leaving the cup on their way home, I then, as before, turned the table half round.

Under these circumstances, however, instead of turning as in the previous experiment, ten ants, one after another, continued their course, thus coming out of the box at the end furthest from the nest. When ten ants successively had, under these circumstances, gone wrong, I then, to make the experiment complete, tried it again, every thing being the same, except that there was no box. Under these circumstances five ants, one after the other, turned directly the table was rotated. It seems clear, therefore, that in determining their course the ants are greatly influenced by the direction of the light.

March 27. I let out two ants imprisoned on the 25th, and placed them on the larvæ, which I put on a column 7 inches high, covered with blue paper, and communicating with the nest by the paper path (A, fig. 3) arranged as usual, but supported on pins. At first I arranged it as shown below, placing the larvæ at M, so that the ants, on arriving at the larvæ, made nearly a semicircle round the edge of the table. I then gradually moved the larvæ to M′ and afterwards to M″. The ants, however, obviously knew that they were going unnecessarily round. They ran along the paper bridge in a very undecided manner, continually turning round and often coming down the pins; while in

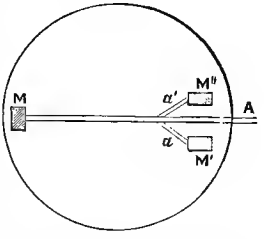
Fig. 3.



returning to the nest they persistently came down the side of the pillar nearest to the nest, though we repeatedly attempted to guide them the other way. Even when placed on the paper bridge between M and M′, they were very dissatisfied. In fact it was obvious that they knew they were being sent a long way round, and were attempting to make a shorter cut.

I then again placed the larvæ at M, and when the ants were once more going to and fro regularly along the paper path, I altered the position to M′, placing the edge of the pillar, which the ants had been accustomed to ascend, towards the paper bridge, connecting it with the original bridge by a side-bridge *a*, M being an inch from the original bridge. Under these circumstances three ants ran on to M; then two found their way over the bridge *a* to M′. Of the next ten ants, five went to M and five over *a* to M′. The next ten all went over the paper bridge *a* to M′.

Fig. 4.



I then put the pillar and the larvæ on the other side of the original paper path at M″ connected with the main path by a short bridge *a*′, and took for *a*′ a new piece of paper, so that scent would be no guide. I left the little bridge *a* in its place. The ants went as follows:—

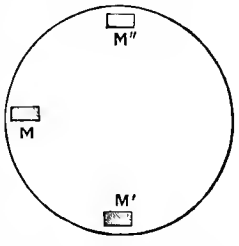
To	M″.	M′.	M.
	1	0	0
	1	0	1
	1	0	1
	1	0	1
	1	1	1
	0	0	1
	1	0	0
	1	0	0
	1	0	0
	1	0	0
	1	1	0
	1	1	0
	1	0	0
	12	3	5

It seems clear, therefore, that though the ants did not trust so

much to their eyes as a man would have done under similar circumstances, yet that they were to some extent guided by sight.

I then removed all the paper pathways and put the pillar to M. Of the two first ants which came to the table, the first found the pillar in 5 minutes, the second, after wandering about for a quarter of an hour, gave the search up in despair, and went home. I then moved the pillar to M′ and watched the next ant that came on to the table; she found it in a minute or two. I then moved it to M″. Two ants came together. One found the pillar in 7 minutes; the other took no less than 25. Obviously, therefore, though it seems clear that they are helped by sight, still these last observations support those previously recorded, and show that in finding their way they do not derive by any means so much assistance from their eyes as we should under corresponding circumstances.

Fig. 5.



Production of Queens.

I have mentioned in one of my previous papers that queens have never (so far, at least, as I had been able to observe) been produced in my nests. I was therefore much interested last year (1880) to find five queens developed in one of my nests of *Formica fusca*. The nest had been under observation since April 1879, and the eggs therefore must have been laid in captivity. The nest had been richly supplied with animal food, and this may possibly account for the fact.

It is known that bees, by difference of food &c., possess the power of obtaining at will from the same eggs either queens or ordinary workers. Mr. Dewitz*, however, is of opinion that among ants, on the contrary, the queens and workers are produced from different kinds of eggs. He remarks that it is very difficult to understand how the instinct, if it is to be called instinct, which would enable the working ants to make this difference can have arisen. This is no doubt true; but it seems to me quite as difficult to understand how the queens, which must have originally laid only queen eggs and male eggs, can have come to produce a third class. Moreover, however great the difficulty may be to

* Zeit. für wiss. Zool. 1878, p. 101.

understand how the ants can have learnt to produce queens and workers from one kind of egg, the same difficulty exists almost to the same extent in bees, which, as Mr. Dewitz admits, do possess the power. Moreover, it seems to me very unlikely that the result is produced in one way in the case of bees, and in another in that of ants. It is also a strong argument that in all my nests, though thousands of workers and males have been produced, I have never observed a queen to be so until this year. On the whole, then, though I differ from so excellent a naturalist with much hesitation, I cannot but think that ants, like bees, possess the power of developing a given egg into either a queen or a worker.

Affection and Kindness.

While I was watching one of my nests of *Formica fusca* on the 23rd of January last (1881), I perceived a poor ant lying on her back and quite unable to move. The legs were in cramped attitudes, and the two antennæ rolled up in spirals. She was, of course, altogether unable to feed herself. After this I kept my eye on her. Several times I tried uncovering the part of the nest where she was. The other ants soon carried her into the shaded part. On the 4th March the ants were all out of the nest, probably for fresh air, and had collected together in a corner of the box; they had not, however, forgotten her, but had carried her with them. I took off the glass lid of the box, and after a while they returned as usual to the nest, taking her in again. On the 5th March she was still alive; but on 15th, notwithstanding all their care, she was dead.

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 am disposed to think 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, of course, I cannot prove. The queens, however, are certainly seven, and probably eight,

* They are still alive and well, Sept. 25, 1881.

years old. In the following nests, viz. another nest of *Formica 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 six and seven years old. I may add that in these nests there have been for the last year very few deaths†.

In conclusion, I may place on record a new species of mite which I have found in nests of *Lasius flavus*, and of which Mr. Michael has been good enough to draw up the following description.

Uropoda formicariæ, sp. nov.

This species, although it falls strictly within the genus *Uropoda*, and not within Kramer's genus *Trachynotus* as defined by that writer, still in most respects, except the very distinctions upon which the genus is founded, resembles *Trachynotus pyriformis* (Kramer) more closely than it does any other recorded species. It is, however, decidedly different; and is characterized by the squareness of its abdomen, the thickness and roughness of its chitinous dermal skeleton, and *especially* by the powerful chitinous rdiges or wing-like expansions on the lateral surface between the second and third pair of legs.

Length, ♂ and ♀, about ·95 millim.

Breadth ,, ,, ·55 ,,

The abdomen is almost square, but somewhat longer than broad, and slightly narrowed at its junction with the cephalothorax, from which it is not plainly distinguished. The extreme edge is a strong chitinous ridge bordered with a thick fringe of short, stout, curved hairs, as in *T. pyriformis*. The dorsal surface of the cephalothorax is also narrowed towards the front, and has a curved anterior margin bent down so as to protect the mouth, as in that species; it bears a few of the same kind of hairs as the abdomen, and has a chitinous thickening at each side. The abdomen rises almost perpendicularly from the marginal ridge. There is a central depression occupying the posterior half, or rather more than half of the abdomen; and at the bottom of this depression are transverse ridges, the hinder ones nearly straight, and the anterior ones bent

* The last of these died on June 15, 1881.

† These ants died off somewhat rapidly, the last on July 23, 1881.

in the middle, the central point being forward; at the sides of, but not in, this depression, are two chitinous blocks which seem to form a starting-point for the ridges. Anterior to this depression the central portion of the creature, *i. e.* its longitudinal dorsal axis, is higher in level than in parts nearer the margin, and forms an irregular triangle of rough chitine. A broad chitinous plate or ridge projects on each side above the second leg, and between that and the third, evidently for their protection; it is probably flexible at the will of the creature, as in the genus *Oribates*.

The sternal surface has strongly marked depressions for the reception of the legs. The coxæ of the first pair of legs are largely developed, flattened, almost touch in the median line, and nearly conceal the mouth, as in the typical *Uropodas*. The genital opening of the male is rather large, round, and placed centrally between the coxæ of the second pair of legs. The female appears only to be distinguished from the male by being more strongly chitinized, and by the conspicuous valval plate which occupies the whole space between the coxæ of the second and third pairs of legs and extends beyond both.

The nymph is less square in the abdomen than the adult, and the border of hairs is absent; the margin is somewhat undulated, the concave undulations being so placed as to give free action to the legs when raised; the central depression of the abdomen is far less marked than in the adult; a slight ridge runs all round the dorsal surface a little within the margin; four ridges, two anterior and two posterior, run from the circumscribing ridge to a raised ellipse in the centre; there are not any plates for the protection of the legs, and the coxæ of the first pair are not flattened as in the adult.

This mite lives in the nests of *Formica flava*.

[*Extracted from the* LINNEAN SOCIETY'S JOURNAL—ZOOLOGY, vol. xvii.]

OBSERVATIONS

ON

ANTS, BEES, AND WASPS.

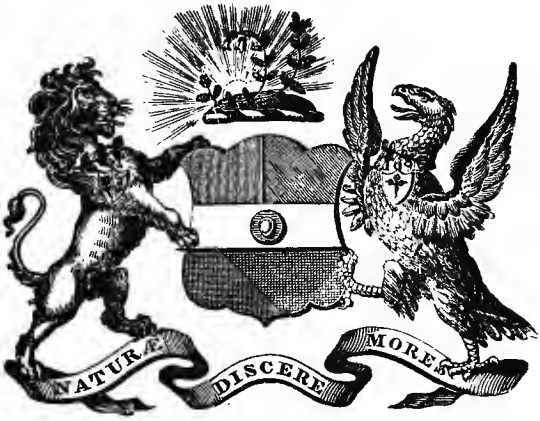
PART X.

WITH A DESCRIPTION OF A NEW GENUS OF HONEY-ANT.

BY

SIR JOHN LUBBOCK, BART., M.P., D.C.L., LL.D., F.R.S.

(PLATE II.)



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Observations on Ants, Bees, and Wasps.—Part X. With a Description of a new Genus of Honey-ant.

By Sir John Lubbock, Bart., M.P., D.C.L., LL.D., F.R.S.

[Read November 2, 1882.]

(Plate II.)

On Bees.

Dr. Müller's Criticisms.—A recent number of 'Kosmos' contains a very courteous and complimentary notice, by Dr. H. Müller, of my recent book on Ants, Bees, and Wasps, which of course, coming from so high an authority, is especially gratifying. Dr. Müller, however, criticises some of the experiments by which I think I have shown experimentally that bees are attracted by different colors, and that they prefer blue to red, yellow, white, or green.

He remarks that in order to make the experiment absolutely correct, the seven glasses should have been arranged in every possible order, and that this would give no less than 5040 combinations. I did not, however, suppose that I had attained to mathematical accuracy, or shown the exact degree of preference; all I claimed to show was the order of preference; and I think that as in my experiments the position of the colors was continually being changed, the result in this respect would have been substantially the same.

Dr. Müller also observes that when a bee has been accustomed to come to one place for honey, she returns to it, and will tend to alight there whatever the color may be; and he shows, by the record of his own experiences, that this has a considerable influence. This is so. Of course, however, it applies mainly to bees which had been used for some time, and were accustomed to a particular spot. I was fully alive to this tendency of the bees, and neutralized it to a considerable extent, partly by frequently changing the bee, and partly by moving the glasses. While, however, I admit that it is a factor which has to be taken into consideration, I do not see that it is any argument against my conclusions. The tendency would be to weaken the effect of preference for any particular color, and to equalize the visits to all the glasses. This tendency on the part of the bees was, as my experiments show, overborne by the effect produced upon them by the color. So far from weakening my conclusions, the

fact, so far as it goes, tends to strengthen them, because it shows that notwithstanding this tendency the blue was preferred and the honey on colorless glass neglected. The legitimate conclusion to be drawn seems, I confess, to me, not that my mode of observation was faulty, but only that the preference of the bees for particular colors is really somewhat greater than the numbers would indicate.

Next, Dr. Müller objects that when disturbed from one drop of honey, the bees naturally would, and that in his experiments they actually did, fly to the next. He gives the two following cases in illustration:—

Blue.	Yellow.	Greenish yellow.	Scarlet.	White.	Red.	Green.	Violet.
	7	6	5	4	3	2	1

Violet.	Green.	Red.	White.	Scarlet.	Greenish yellow.	Yellow.	Blue.
8	7	6	5	4	3	2	1

in which, as will be seen, the bee came in both cases to the right-hand drop, and then went regularly along the line, whatever the color might be. Out of 240 cases he found that the bee, when disturbed, flew to the nearest drop in 207. As a matter of fact, however, this did not happen in my experiments, because, to avoid this source of error, when I removed the colour I gave the bee a good shake and so made her take a flight before settling down again.

According to my experience, bees differ considerably in character, or, I should rather perhaps say, in humor. Some are much shier and more restless than others. When disturbed from the first drop of honey, some are much longer before they settle on the next than others. Much also of course depends on how long the bee has been experimented on. Bees, like men, settle down to their work. Moreover it is no doubt true that, *cæteris paribus*, a bee in search of honey will go to the nearest source.

But, as a matter of fact, in my hundred experiments I only had a few cases like those quoted above from Dr. Müller. This arose partly from the fact that my bees were frequently changed, and partly because I took care, in removing the color, to startle the bee enough to make her take a little flight before alighting again. Dr. Müller says that in his experiments, when the bee did not go to the next honey, it was when he shook her off *too* vigorously. Under the circumstances, I should rather say that in the two observations quoted above he did not shake the bee off vigorously enough. The whole objection, however, is open to

the same remark as the last. The bee would have a tendency of course, like any one else, to go to the nearest honey. Hence I never supposed that the figures exactly indicate the degree of preference. The very fact, however, that there would naturally be a tendency to save themselves labor by going to the nearest honey, makes the contrast shown by my observations all the more striking. Dr. Müller's criticism does not in any way invalidate my conclusions, as he supposes, but, as it seems to me, strengthens my argument.

I have never alleged that it was possible in the case of bees (or for that matter of men either) to get any absolute and exact measure of preference for one color over another. It would be easy to suggest many considerations which would prevent this. For instance, something would probably depend on the kind of flower the bee had been in the habit of visiting. A bee which had been sucking daises might probably behave very differently from one which had been frequenting a blue flower.

So far, however, as the conclusions which I ventured to draw are concerned, I cannot see that they are in any way invalidated by the objections which Dr. Müller has urged.

I exhibit to the Society my original notes; and it will be seen that there is no such sequence as Dr. Müller supposes.

I am glad to see that Dr. Müller is himself about to make a series of experiments on bees with reference to color; and I doubt not they will be most interesting.

Hearing in Bees.—Aug. 27. I brought two sets of bees from different hives to two deposits of honey a few yards apart, and after arranging a telephone with a microphone in the circuit, disturbed one set of bees, holding a telephone close over the other, to see if they would make any noise which would affect the others. I tried it several times, but with no result.

I then placed one telephone just at the entrance of the hive; but whether the apparatus was connected or not, and whether the bees were disturbed or not, seemed to make no difference.

I then made the following experiment:—On the 30th September I put out a small quantity of honey on my lawn and brought some bees to it. I then set a musical box going, and continually replenished the honey and wound up the box. The weather was lovely, and all day a certain number of bees visited the honey.

Then on the 8th October I removed the honey to an open window

on the first floor, and set the musical box playing as usual by its side. I waited half an hour, but not a bee came. I need hardly say that the music was quite audible on the lawn. I then again put the musical box and the honey on the lawn, and the bees very soon again began work. After the lapse of an hour I brought the honey and musical box into the house, and placed them at an open drawing-room window less than 15 yards from where they had stood on the lawn. The music was kept going for an hour, but not a bee came.

The following day was again extremely fine. The bees came as usual to the honey. I let them feed till 10 A.M., when I removed the honey as before to the drawing-room. After the lapse of half an hour I set the box playing and waited half an hour, but not a bee came.

I then put the honey and musical box again out on a chair on the lawn 5 yards in front of the drawing-room window. The first bee found the honey in $5\frac{1}{4}$ minutes. I left it so for three quarters of an hour, and then brought the honey and the musical box into the house and put them just inside the window but out of sight. The box was kept playing for three quarters of an hour, during the whole of which a few bees kept hovering round the chair; but not a single bee found the honey, or even was attracted by the music into the room. I then took the honey and put it again on the chair outside. In less than 5 minutes nine bees had settled on it. I then brought it back into the room and put it, with the bees on it, where it had stood previously. The bees fed, returned to the hive, and came back again to the honey as usual, showing that they had not the slightest objection to enter the house.

I then took the honey and the musical box down to the hives. Immediately (*i. e.* about a yard) in front of my hives is a low wall; and I put the box and the honey on the far side of the wall, so that they were something less than 4 yards distant from the hive, but of course not directly visible. I then kept the music going for two hours, from 1.30 to 3.30 P.M., but not a bee came to the honey.

From these experiments we are, I think, justified in concluding either that the bees did not hear the music, or that, though they had been feeding close to the music, eight days was not a long enough period to suggest to them that there could possibly be any connexion between the honey and the musical box.

To decide between these two alternatives, I moved the musical box (without setting it to play) and honey to another part of the lawn about 15 yards from the first, and put an equal quantity of honey on a similar piece of glass at about the same distance both from the musical box and from the spot where the box had previously been. In half an hour there were several bees at the honey on the musical box, and none at the other. After this we had a week of rain. The next fine morning I again put out the musical box with some honey, and at a distance of about 15 yards a similar quantity of honey on a bit of glass on the grass. In half an hour there were several bees at the honey on the musical box, and none on the other.

I had intended to repeat this several times for greater security, but was unfortunately prevented by bad weather. The observations, however, indicate, as far as they go, that the bees did connect the presence of the musical box with that of the honey, and were guided by it, even if it were not playing, so long as they could see it, but that if they could not see it, even though it were playing, it did not assist them.

At first sight it might seem that these experiments are in direct opposition to the general idea, that a clanging noise is useful in causing bees when swarming to settle soon. This notion is as old as Aristotle, who says, "Bees also appear to have pleasure in noises, so that they say that they collect them into their hives by striking earthen vessels and making noises." He adds, however, "But it is very doubtful whether they hear or not."

The general opinion among writers on bees now seems to be that "tanging" is quite unecessary. Bevan says:—"A tinkling noise is generally, though erroneously, considered to be useful in inducing bees to settle; it is usually made by drumming smartly upon a frying-pan with a large key; and the cottagers call it 'tanging' or 'ringing.' It was probably practised, 'at first, as Butler says, 'to proclaim to the neighbours that a swarm was up, serving as a public notification of the hive from whence it proceeded. This view of the matter is confirmed by the opinion prevalent in some districts, that unless the apiarian can prove the tanging, he cannot justly lay claim to the swarm if it happen to cluster on the premises of a neighbour. The original object of this proceeding, however, seems to be forgotten; and the practice is regarded by most of the cottagers as quite necessary to effect a speedy and satisfactory settling of the bees. Most

scientific apiarians discountenance it; and I am convinced that it is wholly useless.' Mr. Dunbar, during a period of nearly forty years, has only lost one swarm; and that, contrary to his own judgment, was most sonorously tanged"*

I cannot from my own experience decide the point. Admitting, however, that a custom so ancient and so widely spread, is unlikely to be entirely without foundation, I would suggest as possible that what the bees hear under these circumstances are not the sounds which affect us, but the high upper tones near and beyond our range of hearing. Mr. G. Darwin and Lord Rayleigh, whom I have consulted on the subject, inform me that the presence of these inaudible overtones is unquestionable. Mr. Darwin says:—"The high overtones (generally non-harmonic overtones) are very strong within the limits of audibility; and it is almost certain, though not experimentally verified, that the overtones beyond the limits of audibility are strong also."

On Wasps.

Industry of Wasps.—The statement that wasps are as industrious as bees has been received with some incredulity, and has been by many regarded rather as a perhaps pardonable exaggeration arising from individual partiality, than as the strict and sober truth.

I thought therefore that it would be interesting to compare a wasp and a bee under similar circumstances for a whole day. Accordingly, on the 6th August I accustomed a wasp and three bees to come to some honey put out for them on two tables, one allotted to the wasp, the other to the bees. The last bee came at 7.15 P.M. The wasp continued working regularly till 7.47, coming at intervals of between 6 and 7 minutes. Next morning, when I went into my study a few minutes after 4 A.M., I found her already at the honey. The first bee came at 5.45; the second at 6.

The wasp occupied about a minute, or even less, in supplying herself with a load of honey, and made during the day, as shown below, no less than 116 visits to the honey, or 232 journeys between my room and her nest, during which she carried off rather more than 64 grains of honey. The bee sucked from 6 to 16

* 'The Honey-bee,' by Ed. Bevan p. 91.

minutes, and made 29 visits to the honey. Next morning she made her first visit at 8.

As regards the wasp, I believe the record gives a fair idea of what an average wasp would do under similar circumstances. The bee, however, was a shy one; and, as shown in previous experiments, most bees would have come much more frequently. As regards the time she commenced and ceased work, however, she kept about the usual hours, and, as will be seen, began later and left off earlier than the wasp. It would, however, perhaps be unfair to the bees to regard this as indicating that they are less industrious than wasps. The difference may be due to their being more susceptible to cold.

Wasp.	Bee, L. W.	Wasp.	Bee, L. W.	Wasp.	Bee, L. W.
4.13		8.40		12.52	
4.32		8.45		12.56	
4.50		8.56		1. 4	1. 5
5. 5		9. 7		1.11	
5.15		9.14		1.20	
5.22		9.20		1.25	1.25
5.29		9.26		1.30	
5.36		9.37		1.35*	
5.43	5.45	9.43		1.43	1.40
5.50		9.50		1.48	
5.57	5.58	9.57		1.53	1.53
6. 5		10. 4		2	
6.14		10.10		2. 7	
6.23		10.15		2.12	
6.30	6.35	10.24		2.23	2.25
6.40		10.29		2.33	
6.48		10.37		2.39	2.41
6.56		10.45		2.45	2.45
7. 5	7. 4	10.50		2.55	
7.12		10.59		3. 2	3
7.18		11. 6		3. 9	3·16
7.25		11.15		3.17	
7.31		11.22		3.25	
7.40	7.41	11.30		3.30	3.32
7.46		11.35		3.37	
7.52		11.47	11.45	3.45	
8		11.55		3.55	
8.10	8.11	12. 6		4. 5	4
8.18		12.14	12.15	4.12	
8.24		12.22	12.25	4.19	4.16
8.29		12.36	12.37	4 28	
8.36	8.35	12.46		4.39	4.38

* She came and a strange one followed; they began to fight, and I separated them.

Wasp.	Bee, L. W.	Wasp.	Bee, L. W.	Wasp.	Bee, L. W.
4.46		5.50	5.50	6.55	
4.56		6. 5		7. 7	
5. 3	5	6.12		7.17	7.15
5.14		6.20		7.30	
5.25	5.20	6.30		7.36	
5.35		6.40		7.46	
5.46		6.46	6.50		

After this I was away from home for six days, returning on the 12th. During the interval I left the honey on the table, covered over, but with a small entrance for the wasp. On my return I found her still industriously at work. The following morning I got up at 3; but that morning she did not make her first visit till 5.10, returning at 5.19. No other wasp came to the honey.

On Ants.

Ants, Recognition by.—With reference to the interesting problem as to how ants recognize their nest companions, I mentioned in my last paper that I had tried the following experiment. took a few specimens of *Formica fusca* from two different nests, which I will call A and B, and placed them together. At first they were rather shy; but after awhile they fraternized. After they had lived amicably together for three months, I put two of these ants from nest A into nest B; but they were soon attacked vigorously and driven out of the nest.

I have now repeated and extended this test.

On the 16th June I put three specimens of *F. fusca* from my nest No. 81, with the same number from nest 71. Then on the 19th September, one of the six having died in the interval, I put the two from nest 81 into nest 71, and the three from nest 71 into 81. They were all attacked, though not very quickly or vigorously, but by the 21st September all five had been expelled.

Again, on the 25th September I took three ants from each of these nests and put the six together. Then on the 19th March following, one having again died, I put the two from 71 into 81 and the three from 81 into 71. They were all attacked; so that they were evidently recognized as strangers; but it seemed to me that the attack was less vigorous, and I could not be sure that they were either killed or driven out. In the course of the week three or four dead ants were brought out of each of the nests;

but I could not feel quite sure that they were those experimented with.

Lastly, on the 9th April I again put twelve ants, six from each of these nests, together, and kept them so till the 22nd October. I then took four of those from 71, put three into 81 and the fourth into 71. I also took four of those from 81, and put three into 71, and the fourth back into 81 among her old friends. The two ants thus restored respectively to their old nests were as usual recognized as friends and quite unmolested. As regards the other six, the results were as follows. The ants were introduced into the nests at 8.15 A.M.

	Nest 71.	Nest 81.
8.45.	One was being attacked.	One was being attacked.
9.15.	None were „ „	„ „ „
9.45.	Two were „ „	„ „ „
10.15.	One was „ „	„ „ „
10.45.	None were „ „	„ „ „
12.30.	Two were „ „	„ „ „
1.30.	Two were „ „	None were „ „
2.30.	One was „ „	„ „ „

I do not give these results as by any means proving that ants do not recognize their friends by means of smell. They do seem, however, to show that not even six months of close companionship under precisely similar conditions will so far assimilate the odour as to lead to confusion. If the recoguition *is* due in any degree to this cause, the odour is therefore probably an hereditary characteristic.

The following little fact may be worth mentioning as bearing both on the power of communication possessed by ants, and also on their feeling towards a queen. I was starting a new nest of *Lasius flavus* in which were two queens. We allowed the ants to take one of them into their new glass house; the other we kept with a small retinue in a separate bottle. If this bottle is placed near the nest, some of the retinue leave it, go into the nest, and soon the ants come out in large numbers to see, I had almost said to pay their respects to, their queen.

Longevity of Ants.—In previous papers 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 am disposed to think 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, of course, I cannot prove. The queens, however, are certainly more than eight, and probably now more than nine years old. In the nest of *Lasius niger* which I brought in on the 30th November 1875 there was no queen, and, as already mentioned, no workers have been produced. These two still living are therefore the original ones; and they must be between seven and eight years old *.

I had also some workers of *Lasius niger* which I began to observe on the 6th July 1875; the last of these died on June 15th, 1881.

Lastly, some of *Formica cinerea*, which I began to observe on the 29th November 1875, lived till the ants in this nest died off somewhat rapidly, the last on July 23, 1881. There were no queens in either of these nests; these workers therefore must have been more than six years old. The workers of *Lasius niger* above mentioned, which were born at latest in the spring of 1875, must now be more than seven, and my two queens of *F. fusca* more than eight years old.

Sensitiveness of Ants to the Ultra-violet Rays.—Mr. Wigner (of Messrs. Harland and Wigner) was good enough to prepare me a solution of iodine in bisulphide of carbon, and a second of indigo, carmine, and roseine mixed so as to produce the same tint. To our eyes the two were identical both in color and capacity; but of course the ultra-violet rays were cut off by the bisulphide-of-carbon solution, while they were, at least for the most part, transmitted by the other. I placed equal amounts in flat-sided glass bottles, so as to have the same depth of each liquid. I then laid them as in previous experiments over a nest of *Formica fusca*. In twenty observations the ants went seventeen times all under the iodine and bisulphide, twice under the solution of indigo and carmine, and once there were some under each. These observations therefore show that the solutions, though apparently identical to us, appeared to the ants very different, and that, as

* They are now dead, the last on the 25th of February. The two queens of *F fusca* are (March 1883) still alive.

before, they preferred to rest under the liquid which intercepted the ultra-violet rays. In two or three cases only they went under the other bottle; but I ought to add that my observations were made in winter, when the ants were rather sluggish. I am disposed to think that in summer perhaps these exceptional cases would not have occurred.

Discovery of Ponera *in Britain.*—Lastly, I have to record the discovery by my daughters' governess, Miss Wendland (whose assistance I have already had occasion to acknowledge), of a nest of *Ponera contracta*, the first, I believe, ever found in this country. By an unfortunate accident a community of *Lasius flavus* obtained access to and destroyed them before I was able to make any observations on them. Their nest was under a stone; and the community consisted of about 20 individuals.

Description of a New Honey-Ant.

Mr. Bagot has been good enough to send me from Australia another species of Honey-ant, which appears to form the type of a new genus, which I proposed to myself to dedicate to M. Forel and to its discoverer. I am, however, unfortunately anticipated in the use of *Forelia* as a genus, it recently having been appropriated by Dr. G. Haller for a new form of Swiss Hydrachnidæ (see Mitt. naturforsch. Gesell. Bern, 1882, p. 18). In the preparation of the description, which I subjoin, M. Forel has been good enough to give me his valuable assistance.

Melophorus, gen. nov. (Plate II. figs. 1–10.)

Operaria inflata.—Long. 18 mill. Caput rectangulare, latius quam longius. Mandibulæ angustæ, striatæ, quinquedentatæ, dentibus irregularibus, dente anteriore longissimo. Clypeus brevis, subcarinatus, margine antico angulato, ciliato. Fovea clypei magna. Area frontalis triangularis, lata. Ocelli minuti. Oculi ad partem tertiam posteriorem capitis longitudinis (antice) siti. Sulcus magnus inter pronotum et metanotum. Metathorax late constrictus. Petioli squama verticalis, modice incrassata, antice convexa, postice plana, margine superiore emarginato. Subtiliter coriacea, rugulosa, subnitida; fronte inter laminas longitudinaliter rugulosa; abdomine nitido subtilissime transversim ruguloso. Parsissime pilosa, capite infra setis longis barbato. Pubescentia corporis sparsissima, vix ulla. Femora subadpresse breviter pilosa. Tibiæ setis brevibus crassis spiniformibus. Testaceo-rufescens, abdomen testaceum, mandibulæ rufo-castaneæ, apice castaneo-nigro.

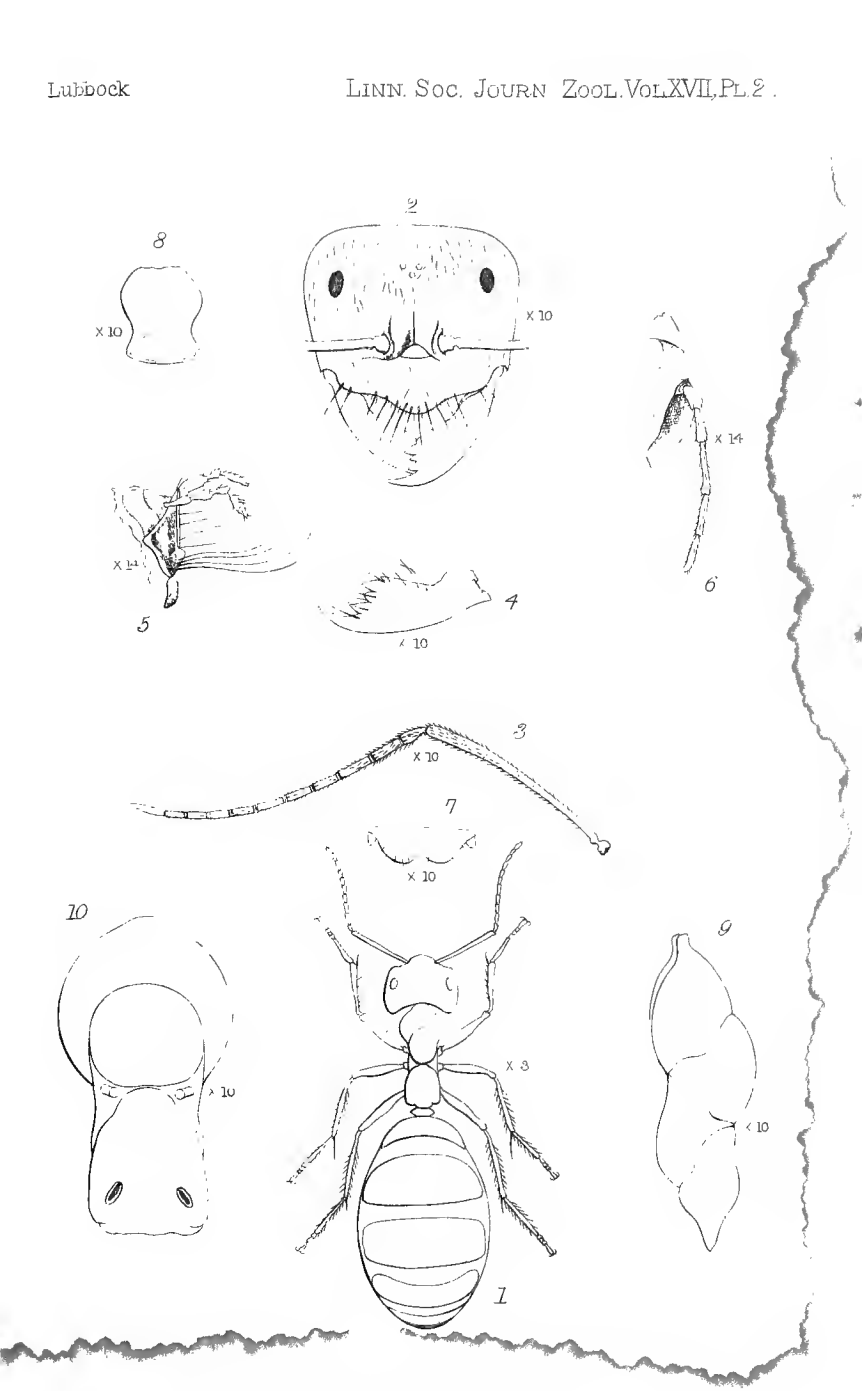
Habitat Australiam; lat. 21° S.

Melophorus bagoti, sp. n.

Operaria.—Antennæ 12-articulatæ, articulis basalibus longioribus, apicalibus (ultimo excepto) brevioribus; palpi maxillares sexarticulati, labiales quadriarticulati; antennæ oriuntur ab angulis clypei posticis; fovea antennalis cum fovea clypei partim confluens; area frontalis lata; laminæ frontales distantes, breves, rectæ; clypeus brevis, antice ciliatus; mandibulæ angustæ, dentatæ; ocelli tres; metathorax constrictus; petioli squama verticalis; abdomen orificio cloacæ rotundo, ciliato, apicali; vesicula venenifica cum pulvinari; gigeriorum lamellæ breves, a basi fortiter divergentes.

DESCRIPTION OF PLATE II.

Fig. 1. Outline figure of a slightly swollen specimen of the new Australian Honey-ant, *Melophorus bagoti*, three times natural size.
2. The head, seen from above, ×10.
3. Antenna, ×10.
4. Mandible, ×10.
5. Labium, ×14.
6. Maxilla, ×14.
7. Labrum, ×10.
8. Knot, seen from behind, ×10.
9. An outline of the thorax, viewed laterally, ×10.
10. An outline of the thorax, seen from above, ×10.



E. Wilson del.
G. Jarman sc.

NEW GENUS OF HONEY-ANT.
(MELOPHORUS).

Hanhart imp