sufficient to produce any sensible effect could not possibly have been caused by the amount of light which was able to diminish the resistance in a marked degree. Lastly, in the case of a cell composed of silver and silver sulphide in equal parts, light and heat produced opposite effects. A paraffin lamp at a distance of 18 inches diminished the resistance. But when the lamp was placed at a distance of only 10 inches, the needle of the galvanometer first moved in a direction indicating a further fall of resistance; and then, a few seconds after, when the temperature had begun to rise, it turned in the opposite direction. On again moving the lamp to a distance of only 4 inches from the cell, there was at once a large deflection, indicating increased resistance; the effect of the temperature completely predominating over that of light radiation. To show how little effect temperature had upon the action of the cells in comparison with the influence of light, Mr. Bidwell stated that when placed at a distance of 16 feet from a small window (the day being overcast, and all other windows closed), the light from this window had an effect upon a sulphide of silver cell represented by the movement of the spot of light of a mirror galvanometer through 90 scale divisions. By the substitution of a delicate thermopile for the sensitive cell, it was found that the heat radiation in this case was equal to that from a human body at a distance of 10 ft. 6 in. A nearly red-hot glass ball, held within an inch of the cell, produced a fall of resistance only equal to 23 scale divisions in 15 seconds, while, of course, the effect produced by the human body at the distance named was absolutely *nil*. It appears, therefore, that Mr. Bidwell has a means of observing light radiations not liable to be vitiated by ordinary temperature conditions.

INTELLIGENCE OF THE DOG.

BEFORE a crowded sitting of the Biological Section of the British Association, Sir John Lubbock read a paper in which he gave some interesting notes on the intelligence of the dog. The man and the dog, he said, have lived together in more or less intimate association for many many thousands of years, and yet it must be confessed that they know comparatively little of one another. That the dog is a loyal, true, and affectionate friend must be gratefully admitted; but when we come to consider the psychical nature of the animal, the limits of our knowledge are almost immediately reached. I have elsewhere suggested that this arises very much from the fact that hitherto we have tried to teach animals rather than to learn from them—to convey our ideas to them rather than to devise any language or code of signals by means of which they might communicate theirs to us. The former may be more important from a utilitarian point of view, though even this is questionable, but psychologically it is far less interesting. Under these circumstances, it occurred to me whether some such system as that followed with deaf mutes, and especially by Dr. Howe with Laura Bridgman, might not prove very instructive if adapted to the case of dogs.

I have tried this in a small way with a black poodle named Van. I took two pieces of cardboard about 10 in. by 3 in., and on one of them printed in large letters the word "food," leaving the other blank. I then placed two cards over two saucers, and in the one under the "food" card put a little bread and milk, which Van, after having his attention called to the card, was allowed to eat. This was repeated over and over again till he had had enough. In about ten days he began to distinguish between the two cards. I then put them on the floor and made him bring them to me, which he did readily enough. When he brought the plain card I simply threw it back, while, when he brought the "food" card, I gave him a piece of bread, and in about a month he had pretty well learned to realize the difference. I then had some other cards printed with the words "out," "tea," "bone," water," spelt phonetically so as not to trouble him by our intricate spelling, and a certain number also with words to which I did not intend him to attach any significance, such as "nought," "plain," "ball," etc.

Van soon learnt that bringing a card was a request, and soon learned to distinguish between the plain and the printed cards; it took him longer to realize the difference between words, but he gradually got to recognize several, such as food, out, bone, tea, etc. If he was asked whether he would like to go out for a walk, he would joyfully fish up the "out" card, choosing it from several others, and bring it to me, or run with it in evident triumph to the door.

I need hardly say that the cards were not always put in the same places. They were varied quite indiscriminately and in a great variety of positions. Nor could the dog recognize them by scent. They were all alike, and all continually handled by us. Still, I did not trust to that alone, but had a number printed for each word. When, for instance, he brought a card with "food" on it, we did not put down the same identical card, but another bearing the same word; when he had brought that, a third, then a fourth, and so on. For a single meal, therefore, eighteen or twenty cards would be used, so that he evidently is not guided by scent. No one who has seen him look down a row of cards and pick up the one he wanted could, I think, doubt that in bringing a card he feels he is making a request, and that he can not only distinguish one card from another, but also associate the word and the object. This is, of course, only a beginning, but it is, I venture to think, suggestive, and might be carried further, though the limited wants and aspirations of the animals constitute a great difficulty. My wife has a very beautiful and charming collie, Patience, to which we are much attached. This dog was often in the room when Van brought the "food" card and was rewarded with a piece of bread. She must have seen this thousands of times, and she begged in the usual manner, but never once did it occur to her to bring a card. She did not touch or indeed even take the slightest notice of them. I then tried the following experiment: I prepared six cards about 10 in. by 3 in., and colored in pairs—two yellow, two blue, and two orange. I put three of them on the floor, and then, holding up one of the others, endeavored to teach Van to bring me the duplicate. That is to say, that if the blue was held up, he should fetch the corresponding color from the floor; if yellow, he should fetch the yellow, and so on. When he brought the wrong card, he was made to drop it, and return for another till he brought the right one, when he was rewarded with a little food. The lessons were generally given by my assistant, Miss Wendland, and lasted half an hour, during which time he brought the right card on an average about twenty-five times. I certainly thought that he would soon have grasped what was expected of him. But no. We continued the lessons for nearly three months, but, as a few days were missed, we may say ten weeks, and yet at the end of the time I cannot say that Van appeared to have the least idea what was expected of him. It seemed a matter of pure accident which card he brought. There is, I believe, no reason to doubt that dogs can distinguish colors; but as it was just possible that Van might be color blind, we then repeated the same experiment, only substituting for the colored cards others marked respectively I., II., and III. This we continued for another three months, or, say, allowing for intermission, ten weeks, but to my surprise entirely without success.

I was rather disappointed at this, as, if it had succeeded, the plan would have opened out many interesting lines of inquiry. Still, in such a case, one ought not to wish for one result more than another, as of course the object of all such experiments is merely to elicit the truth, and our result in the present case, though negative, is very interesting. I do not, however, regard it as by any means conclusive, and should be glad to see it repeated. If the result proved to be the same, it would certainly imply very little power of combining even extremely simple ideas. I then endeavored to get some insight into the arithmetical condition of the dog's mind.

On this subject I have been able to find but little in any of the standard works on the intelligence of animals. Considering, however, the very limited powers of savage men in this respect—that no Australian language, for instance, contains numerals even up to four, no Australian being able to count his own fingers even on one hand—we cannot be surprised if other animals have made but little progress. Still, it is surprising that so little attention should have been directed to this subject. Leroy, who, though he expresses the opinion that "the nature of the soul of animals is unimportant," was an excellent observer, mentions a case in which a man was anxious to shoot a crow. "To deceive this suspicious bird, the plan was hit upon of sending two men to the watch-house, one of whom passed on, while the other remained; but the crow counted, and kept her distance. The next day three went, and again she perceived that only two retired. In fine, it was found necessary to send five or six men to the watch-house to put her out in her calculation. The crow, thinking that this number of men had passed by, lost no time in returning." From this he inferred that crows could count up to four. Lichtenberg mentions a nightingale which was said to count up to three. Every day he gave it three mealworms, one at a time; when it had finished one, it returned for another, but after the third it knew that the feast was over. I do not find that any of the recent works on the intelligence of animals, either Buchner, or Peitz, or Romanes, in either of his books, give any additional evidence on this part of the subject. There are, however, various scattered notices.

There is an amusing and suggestive remark in Mr. Galton's interesting "Narrative of an Explorer in Tropical South Africa." After describing the Damara's weakness in calculations, he says: "Once while I watched a Damara floundering hopelessly in a calculation on one side of me, I observed Dinah, my spaniel, equally embarrassed on the other; she was overlooking half a dozen of her new-born puppies, which had been removed two or three times from her, and her anxiety was excessive, as she tried to find out if they were all present or if any were still missing. She kept puzzling and running her eyes over them backward and forward, but could not satisfy herself. She evidently had a vague notion of counting, but the figure was too large for her brain. Taking the two as they stood, dog and Damara, the comparison reflected no great honor on the man." But even if Dinah had been clear on this subject, it might be said that she knew each puppy personally, as collies are said to know sheep.

The same remark applies generally to animals and their young. Swans, for instance, are said to know directly if one of their cygnets is missing, but it is probable that they know each young bird individually. This explanation applies with less force to the case of eggs. According to my bird-nesting recollections, which I have refreshed by more recent experience, if a nest contains four eggs, one may safely be taken; but if two are removed, the bird generally deserts. Here then it would seem as if we had some reason for supposing that there is sufficient intelligence to distinguish three from four.

An interesting consideration rises with reference to the number of the victims allotted to each cell by the solitary wasps. Ammophile considers one large caterpillar of Noctura segetum enough; one species of Eumenes supplies its young with 5 victims; another 10, 15, and even up to 24. The numbers appear to be constant in each species.

How does the insect know when her task is fulfilled? Not by the cell being filled, for if some be removed she does not replace them. When she has brought her complement, she considers her task accomplished, whether the victims are still there or not. How, then, does she know when she has made up the number 24? Perhaps it will be said that each species feels some mysterious or innate tendency to provide a certain number of victims. This would under no circumstances be any explanation, but it is not in accordance with the facts. In the genus Eumenes the males are much smaller than the females. Now, in the hive bees, humble bees, wasps, and other insects where such a difference occurs, but where the young are directly fed, it is of course obvious that the quantity can be proportioned to the appetite of the grub. But in insects with the habits of Eumenes and Ammophila the case is different, because the food is stored up once for all. Now, it is evident that if a female grub was supplied with only food enough for a male, she would starve to death; while if a male grub were given enough for a female, it would have too much. No such waste, however, occurs. In some mysterious manner the mother knows whether the egg will produce a male or female grub, and apportions the quantity of food accordingly. She does not change the species or size of her prey; but if the egg is male, she supplies 5, if female 10, victims. Does she count? Certainly this seems very like a commencement of arithmetic. At the same time it would be very desirable to have additional evidence how far the number is really constant. Considering how much has been written on instinct, it seems surprising that so little attention has been directed to this part of the subject. One would fancy that there ought to be no great difficulty in determining how far an animal could count; and whether, for instance, it could realize some very simple sum, such as that two and two make four. But when we come to consider how this is to be done, the problem ceases to appear so simple. We tried our dogs by putting a piece of bread before them, and prevented them from touching it until we had counted seven.

To prevent ourselves from unintentionally giving any indication, we used a metronome (the instrument used for giving time when practicing the pianoforte), and to make the beats more evident we attached a slender rod to the pendulum. It certainly seemed as if our dogs knew when the moment of permission had arrived; but their movement of taking the bread was scarcely so definite as to place the matter beyond a doubt. Moreover, dogs are so very quick in seizing any indication given them, even unintentionally, that, on the whole, the attempt was not satisfactory to my mind. I was the more discouraged from continuing the experiment in this manner by an account Mr. Huggins gave me of a very intelligent dog belonging to him. A number of cards were placed on the ground, numbered respectively 1, 2, 3, and so on up to 10. A question is then asked; the square root of 9 or 16, or such a sum as $6 \times 52 - 3$. Mr. Huggins pointed consecutively to the cards, and the dog barked when he came to the right one. Now, Mr. Huggins did not consciously give the dog any sign, yet so quick was the dog in seizing the slightest indication that he was able to give the correct answer. This observation seems to me of great interest in connection with the so-called "thought reading." No one, I suppose, will imagine that there was in this case any "thought reading" in the sense in which this word is used by Mr. Bishop and others. Evidently Kepler seized upon the slight indications unintentionally given by Mr. Huggins. The observation, however, shows the great difficulty of the subject. I have ventured to bring this question before the section, partly because I shall be so much obliged if any lady or gentleman present will favor me with any suggestions, and partly in hope of inducing others with more leisure and opportunity to carry on similar observations, which I cannot but think must lead to interesting results.

THE BED OF THE OCEAN.

THE Tuesday evening discourse during the meeting of the British Association was delivered by Mr. J. Murray, F.R.S., of the Challenger expedition, who took for its subject the "Bed of the Ocean, and Some Results of the Expedition."

In commencing his lecture, Mr. Murray traced the development of geographical knowledge from the crude conception of the ancients down to the extended knowledge of the nineteenth century. It was not easy, he said, to estimate the relative importance of the events of one's own time, yet, in all probability, the historians of the reign of Victoria would point to the recent discoveries in the great oceans as the most important events of the century with respect to the acquisition of natural knowledge—as among the most brilliant conquests of man in his struggle with nature; and doubtless they would be able to trace the effects of these discoveries on the literature and on the philosophic conceptions of our age. The last of the great outlines showing the surface features of our globe had been boldly sketched; the foundations of a more complete and scientific physiography of the earth's surface had been firmly laid down. The lecturer then briefly described the chief surface features of the globe, the action of wind and water and ocean currents; referred to the temperature of the surface of the sea, and explained that the most important as well as the most direct effect of the unequal distribution of temperature over the surfaces of the oceans and continents was an unequal distribution of atmospheric pressure, varying more or less with season. He then proceeded: The advances during recent years in the knowledge of one form of life inhabiting the floor of the ocean surpassed those in any other department of oceanic investigation. Thousands of new organisms have been discovered in all seas and at all depths in the ocean, and either had been or were now being described by specialists in all quarters of the world. There did not seem to be any part of the ocean bed so deep, so dark, so still, or where the pressure was so great as to have effectually raised a barrier to the invasion of life in some of its many forms. Even in the greater depths all the great divisions of the animal kingdom were represented. As they descended into the deeper waters, and proceeded further seaward from the borders of the continents, species and the number of individuals became fewer and fewer, though they often presented archaic or embryonic characters, till a minimum was reached in the greatest depths farthest from continental land. Distance from continental land was indeed a much more important factor in the distribution of deep-sea animals than actual depth. If they neglected the Protozoa, and compared the results of twelve of the Challenger's trawlings and dredgings in the central line of the Pacific, in depths greater than 2,000 fathoms, on globigerina ooze, radiolarian ooze, and red clay, with twelve trawlings and dredgings taken under similar conditions and depths, but on the blue and green muds within 200 miles of the continents, they found that the Central Pacific stations yielded 92 specimens of animals belonging to 52 species, all—with two doubtful exceptions—new to science, and among them 13 new genera. On the other hand, the stations near the continents gave over 1,000 specimens, belonging to 211 species, of which 145 were new species and 66 belonged to species previously known from shallower water.

Although no new types of structure had been discovered in organisms from the deep sea, the peculiar modifications which animals had undergone to accommodate themselves to abysmal conditions were sufficiently interesting and remarkable. The eyes of some fish and crustaceans had become atrophied or had disappeared altogether, while in others they had become of exceedingly large size, or been so modified as to be scarcely recognizable as eyes. Fins and antennæ had become extraordinarily elongated, and at times appeared to simulate the alcyonarians of the deep sea.