

XVI. *On some circumstances relating to the economy of bees.* By THOMAS ANDREW KNIGHT, *Esq. F.R.S. President of the Horticultural Society.*

Read May 22, 1828.

IN a paper which I had the honour to address to the Royal Society about twenty years ago (in the year 1807) upon the Economy of Bees, I stated, that having adapted cavities in hollow trees for the reception of swarms of those insects, I had observed that several days previous to the arrival of a swarm, a considerable number of bees were constantly employed in examining the state of the tree, and particularly of every dead knot above the cavity which appeared likely to admit water into it. At that period it appeared to me rather extraordinary, that animals so industrious as bees, and so much disposed to make the best use of their time, should, at that important season of the year, waste so much of it in apparently useless repetitions of the same act: for I, at that time, supposed that on different days, and at different periods of the same day, I saw only the same individuals. But in a case which at a subsequent period came under my observation, where the cavity into which the bees apparently proposed to enter, was not more than a quarter of a mile distant from the hive whence a swarm were prepared to emigrate, I witnessed a very rapid change of the individuals who visited their future contemplated habitation; and the number which in the course of three days entered it, appeared to me to be fully equal to constitute a very large swarm: and upon the evidence of these and other facts, which I shall proceed to state, I am much disposed to infer, that not a single labouring bee ever emigrates in a swarm without having seen the future proposed habitation of that swarm. That the queen bee has also always seen her future habitation, I am also much inclined to believe, as she is well known to absent herself from the hive some time previously to the emigration of a swarm: though her object may be to meet a male of another hive; for I much doubt whether she ever receives the embraces of a brother. The results of some of HUBER's experiments are very favourable

to this conclusion, as is the otherwise excessive number of male bees, and in both the animal and vegetable world nature has taken very ample means of facilitating what the breeders of improved varieties of domesticated animals call cross breeding.

I have also been led by the following facts to believe, that not only the future permanent habitation of each swarm, but the place where they temporarily settle, apparently to collect their numbers, soon after they quit their hive, is known also to each individual. Different families of domesticated animals of every species present some peculiarities of disposition and habit; and the swarms of the family of bees, which were the subject of my experiments, showed, I think, more than an ordinary disposition to unite, by two apparently joining the same queen. My attention was consequently attracted to the circumstances which preceded such unions.

The simultaneous movements and agitation of two hives, had during several days led me to expect that a junction of their swarms was contemplated; and the two ultimately issued out almost at the same moment, and instantly united, as I had concluded they would. The weather was excessively hot; and I put them into a hive which was scarcely large enough to hold them, affording them no further shelter from the sun than I thought just sufficient to prevent the melting of their combs. This occurred upon the first day of June, and in the morning of the twenty-third a very large swarm emigrated. There was in this, I believe, nothing very extraordinary or peculiar, except the excessive expedition apparently employed in raising a second queen.

In the following year two other hives presented similar indications that their swarms would unite; and being anxious to ascertain whether such unions were accidental, or the consequence of previous arrangements, I paid very close attention to their proceedings, and the following singular circumstances came under my observation. After both hives had given frequent indications that a swarm was ready to issue from each of them, one swarm only rose, and that, after hovering in the air during a much longer time than ordinary, settled upon, and around, a bush about twenty-five yards distant from the hive, whence they had issued: but instead of collecting together into a compact mass, as they usually do, they remained thinly dispersed, scarcely two being any where in contact with each other. In this state they continued nearly half an hour

motionless and apparently discontented and sulky ; and they then gradually began to rise and return home, not apparently in obedience to any command or signal ; for they did not rise more abundantly at any one point of time than at another, but each individual seemed to go when tired of waiting.

The next morning a swarm issued from the other hive, and proceeded to the bush upon and around which the other swarm had settled on the preceding day, collecting themselves into a mass as they usually do when their queen is present. This was precisely what I had anticipated, but I was much disappointed that no movement or agitation took place in the other hive. Within a very few minutes, however, and very soon after the swarm above mentioned had fully settled, a very large number of bees suddenly rushed from the hive to which the swarm had returned on the preceding day, and proceeded so directly to the swarm which had just settled, that their course was marked through its whole extent by a perfectly visible dark and narrow line, and they united themselves, without hovering a single instant, to the other swarm. These circumstances conjointly with others which I have stated in my former communication upon this subject, satisfied me that these unions are generally, if not always, the result of previous and perfectly well understood arrangements, though it is not easy to conjecture how such arrangements can be made.

I shall proceed to state a few circumstances which appear to throw light upon some of the phænomena observable in the mode of breeding of bees. It has long been known that these animals possess the power of raising a queen bee from any recently deposited egg, which under ordinary circumstances would have produced a labouring bee ; but whether this power extends to those eggs, which, when deposited in larger cells, afford male or drone bees, has not, I believe, been accurately ascertained. The following circumstances lead me to believe that sex is not given to the eggs of birds, or to the spawn of fishes or insects, at any very early period of their growth.

I selected early in winter four female birds of the common duck, which I kept apart from any male bird of that or any kindred species, till the period of their laying eggs approached. One was then killed, and the largest of its eggs was found to be three lines in diameter. A musk drake (*Anas moschata*) was then put into company with the three remaining ducks ; and from these I obtained a numerous offspring, six out of seven of which proved to be males,

as the result of similar previous experiments (but in which the male of another species had been introduced at a period when the growth of the eggs was less advanced,) had led me to expect. I repeated the experiment often, and always with nearly the same result, a large majority of male birds being uniformly produced ; and hence I conclude that the eggs of birds in early periods of their growth are without sex.

I have never possessed means of obtaining mule fishes ; but one kind of fish, which I think is obviously a mule, is found in many rivers where the common river-trout abounds, and where a solitary salmon is sometimes seen. These formerly existed, in some seasons, in considerable numbers, in the river which passes near my residence ; but since salmon have become scarce, they have wholly disappeared. I had formerly opportunities of examining a large number of them, without having ever found a single female. I have subsequently found them in large numbers in small mountain rivulets in Wales, below, but never above, the lowest Cataract. They are readily distinguished from the young salmon, by their form being intermediate between that of a trout and of a salmon ; by their being all, or nearly all, males ; and by their remaining through the summer and autumn in the rivers, long after the young salmon have descended to the sea : they leave the fresh water with the first winter floods, and I believe are not known ever to return. In the North of England they are distinguished by the name of wrackriders, and by that of samlets in some other parts. If these be mules, as I do not entertain any doubt that they are, the spawn of fishes must be without sex when it is deposited by the female ; and I am much disposed to entertain the same opinion respecting the spawn (for it is more properly spawn than eggs) of bees.

I have frequently witnessed some somewhat analogous circumstances in the vegetable world, respecting the sexes of the blossoms of plants ; and I can at any time succeed in causing several kinds of monoecious plants to produce solely male or solely female blossoms. If heat be, comparatively with the quantity of light which the plant receives, excessive, male flowers only appear ; but if light be in excess, female flowers alone will be produced :—the experiments necessary must of course be made with skill and accuracy.

In a former communication to the Royal Society “ Upon the comparative influence of the male and female parent upon the character of the offspring,”

I have inferred, from facts there stated, that the sex of the offspring of some species of animals is given by the female parent. Subsequent experience and observation have strengthened my belief in the truth of this inference: but I believe the power of the female parent to be rather strongly influential than positive, and that external causes operate, which (I have some reason to suspect) are not in all cases wholly beyond the reach of human controul.

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