THE HONEY BEE

THE

Dzierzon Theory;

BEING A FULL ELUCIDATION OF

SCIENTIFIC BEE-KEEPING,

BY THE

BARON OF BERLEPSCH.

CHICAGO, ILLINOIS:
OFFICE OF
The American Bee Journal.
THOMAS G. NEWMAN & SON.
1877.

HONEY JARS.

One pound (square) Honey Jars, ℔ gross	$6.00
Two " " " " "	8.00
One " " " "flint glass"	8.50
Two " " " " " "	10.50
Corks for 1 and 2 ℔ jars, per gross	.75
Tin Foil Caps " " " "	1.00
Labels for " " " "	.75
1,000 Labels, address printed to order, ...	5.00
One-qt. Fruit Jars (Mason's Pat.) ℔ gross	16.00
Labels for same "	.65
A thousand Labels for same, address printed to order.....	4.00
Uncapping Knives, as good as any, each..	.50
" " " per doz.	4.50
Alsike Clover Seed, bushel.	16.00
" " " " peck	4.00
" " " " pound	.50

LANGSTROTH'S BEE-HIVES,

Straw Mats. Bee-Veils, Alsike Clover Seed etc., at reasonable rates.

For further particulars address,

CHAS. F. MUTH, Cincinnati, Ohio

C. F. Muth.

The Dzierzon Theory.

We propose to present to the reader, in the form of distinct propositions, the fundamental principles of Dzierzon's system of bee culture, as set forth by the Baron of Berlepsch, in his celebrated Apistical Letters; designing to furnish a condensed statement of the facts and arguments by which these propositions are demonstrated. Without an accurate and familiar acquaintance with this theory, the practice of bee culture cannot be conducted with the judgment and skill requisite to justify an expectation of successful results. The practical operations must be based on and adapted to the theory, which, hence, becomes a proper subject of study.

The following propositions, which embrace, substantially, the entire Dzierzon theory, are, in so far as they contain or propound anything novel, deduced from the personal observations and experiments of that celebrated apiarist. Several of them were warmly impugned by some of the ablest correspondents of the German *Bienenzeitung*. But Dzierzon alone, for a season, and the Baron of Berlepsch, and others, subsequently defended them with equal astuteness and vigor—adducing unquestionable facts in their support. The controversy was a very animated one; nor was opposition silenced till, by the introduction of the Italian bee, the means of conclusively determining the chief points at issue were furnished. The evidence thus supplied was so clear and decisive, that all serious opposition ceased, and the truth of the positions were conceded by all intelligent apiarists. Naturalists and physiologists, however, continued to discredit and reject some parts of the theory, because they contravened so directly their own long-cherished views and opinions. But even they were ultimately constrained to yield to the evidence, when the facts as ascertained by Professors Leuckart and Von Siebold no longer left room for cavil or doubt.

THE DZIERZON THEORY.

BY THE BARON OF BERLEPSCH.

The first proposition which we are now to consider is in the following words:

I.—A colony of bees, in its normal condition, consists of three characteristically different kinds of individuals—the queens, the workers, and (at certain periods) the drones.

All this will be readily conceded. But there are some apiarists who contend that there is, in every colony, a fourth kind—the *black bees*, quite as distinct as any of the others, and to which they ascribe the function of laying the drone-eggs. This seems plausible, too, for it is an undeniable fact that bees do occur which are distinguished from the rest by their darker color; and the question can only be whether the blackness of these bees is an accidental trait or constitutes a characteristic difference. Dr. Magerstedt contends that the color is constitutional, and enumerates besides not less than twelve other points of difference between these black bees and common workers. Thus, among other things, he alleges that these bees are black when they emerge from the brood-cells; that their proboscis is much shorter; that they have no corbicula on their thighs; that they have a smaller sting, and possess ovaries. It is hard to contend against facts, if these be facts—which I cannot concede. Among the many thousands of young bees which I have seen emerge from the brood-cells, I never saw one come forth of any other color than a lightish grey. All the black bees I have ever observed in my apiary have probosci-des as long as others, corbicula as deep, and stings as large.

I would further suggest that before comparison is instituted, the specimens of common workers selected be deprived of their hirsuties, or at least thoroughly wetted. Possibly if this were done with an entire colony, the operator might discover that he has before him black bees exclusively—precisely similar in all respects to those which have been termed *drone-mothers!* I am not disposed to be jocular, but there

cannot be a doubt that the allegations of Dr. Magerstedt to which I have referred, rest on sheer delusion. Excepting the variation in color, and the greater or less absence of hair, there is no physical difference whatever between the common and the black bees. The unprejudiced use of one's eyes, is sufficient to settle this point conclusively.

But in what way do these bees lose their hair? Or, rather, how do they become black? Is it the result of age, or of toil? Not precisely. Toil-worn veterans are easily known by their dark-brown color, and lacerated wings, whilst these are of a glossy-soot color, sometimes even coal-black, and have generally uninjured wings. The *former* are the kind which may be found in large numbers, almost every summer, when there are abundant and long-continued supplies of pasturage. They are the oldest among the workers; those which have labored longest and most industriously, on the blossoms of such plants as the centaurea, euphorbia, etc., and whose coats, as Mr. Frank graphically expressed it, have become *seedy* or "shabby-genteel" in the service. The *latter* kind, the glossy-soot black, appear early in the spring, not indeed in every colony, nor in large numbers, but singly, here and there, and have all disappeared before autumn. Yet catch one of these, scrutinize it with the utmost care, and you will find you have—nothing but a common worker.

I have often compared with each other these two differently shaded black bees, and have always found that the prime color of the latter was a sooty or glossy-black, while that of the former was a brownish-black. This was the case still when the latter was more completely deprived of its hirsuties than the former. This led me to conjecture that the glossy-sooty color did not result from the loss of hair; but that some other influences had operated on the corneous tunics of those bees.

The common opinion is that the darker color of these bees results from their having been besmeared with honey; and that they are those precisely, which have been most frequently and longest engaged in robbing other colonies of their stores. To test this, I made the following experiment:—I set before a populous colony, in which I could not discover a single black bee, a plate of honey, and when it was densely covered with bees, I poured honey on them, and left

them till they had been licked clean by their companions. I repeated this on four successive evenings, and finding that not one sooty-black bee was to be found amongst them after the operation, I concluded that this peculiar color was not produced in the manner supposed. Nor could I find that it resulted from creeping through crannies and crevices, nor from occasional combats with bees into whose hives they attempted to intrude. Careful investigation satisfied me that nothing of this kind could produce the effect.

Finally, an accident, or rather a mishap, led to the discovery of the cause, and convinced me that Mr. Sudz had, as early as December, 1848, substantially surmised the truth.

In April, 1852, I exhibited my glass hives to a friend then visiting me, who was so delighted therewith, that he expressed a strong desire to obtain one. I promised to gratify him, but as the combs in mine were already much discolored, I proposed to stock a new hive for him, with a swarm, at the proper season. A hive was immediately procured, and on the 19th. of May, I introduced into it an unusually large swarm. The bees had nearly all entered, when a servant came to announce the arrival of company. To receive them, I left the work unfinished, and could not return for nearly two hours. When I got back, I found the exterior of the hive densely covered with bees, and at once conjectured that another swarm had lit on it. The crowd had literally choked up the entrance, and from the great heat in the hive, and the want of air, fully two-thirds of the bees were suffocated, and all were dripping with perspiration. As a sufficient number to form a colony still survived, and I did not find the queen among the dead, I set matters in order and placed the hive in my apiary. Next morning I was greatly astonished to see hundreds of glossy soot-black bees, issuing from and entering this hive. For more convenient observation I transferred the colony next day to a more suitable hive, and soon satisfied myself that these black bees were Matuschka's and Magerstedt's famous *drone-mothers*—precisely similar in all respects to samples which I had seen on various occasions.

1. Their ocellæ seemed more prominent, their bodies thinner and slenderer, their corbicula smaller and shallower—all because they had no hair.

2. The head seemed further and more distinctly separated from the thorax, and this again from the abdomen—because from the absence of hirsuites, the intersections were simply more obvious.

3. They appeared generally to be more supple and active than common workers, because from their nudity the movements of the limbs were more perceptible.

4. Many of them appeared to *drag* their abdomen along, as though their muscular powers were somewhat impaired.

5. They carried in no pollen, or at most only mere rudimentary pellets; because these could not be securely retained in the corbicula denuded of their bristly fringe.

6. They showed no disposition, even in the most pleasant part of the day, when pasturage abounded, to rifle the flowers of their sweets. Hardly one in twenty which I examined had its honey-bag filled, and then not with the nectar of flowers, but with pure honey. It is evident from this that robbing is their perilous pursuit, that they no longer possess a predilection for gathering the nectar of flowers.

7. On every hand they endeavored to force their way into other colonies, because in accordance with their natural instinct they desire to appropriate honey, and the change which their physical structure had undergone, disabled them to gather from flowers.—When other bees attempted to sieze them, they shrunk away timidly, or fawningly presented their proboscis, as all robbing-bees are prone to do under such circumstances,

8. When hovering about a hive, doubtful of their reception, they hold their hinder legs suspended downward and rearward, as other robbing-bees do in similar circumstances—thus showing that they are consciously strangers.

9. Occasionally they succeeded in effecting an entrance; but generally they were seized and killed, or crippled, as such intruders usually are.

10. Observing on one occasion a considerable number of them entering a hive together, I watched for their return, caught several of them, and satisfied myself by an inspection of their well-gorged honey-bags, that their object was to steal the garnered stores.

11. Their numbers decreased gradually, and by the middle of August they had altogether disappeared.

Now is not all this just the same as with the glossy-black bees noticed in so many different districts, and about which some bee books make so much ado? And is it not thus demonstrated that their loss of hair and their glossy-black color are ordinarily the effects of fright, perspiration and exposure to a hot and humid atmosphere? I say *ordinarily*, because I would not assert that other causes may not, at times, produce similar effects; though I much doubt whether merely wetting the bees thoroughly would work such a change of color and appearance. Often as, in the course of my experiments with bees, I had occasion to immerse entire colonies, not unfrequently keeping them wet for hours, when searching for the queen or picking out the drones, I never saw that the bees changed color in consequence. I have often, also, noticed bees returning to their hives after a shower of rain, in which they had been thoroughly drenched, but never perceived that the number of black bees was thereby increased.

The foregoing remarks will serve to elucidate several other points:

1. The black bees usually make their appearance in early spring, because during the winter the animal exhalations in the hive are strongest, most confined, and most penetrating. Some of the bees remaining quiet and almost motionless for a considerable period, are consequently the more thoroughly exposed to the effects of these exhalations.

2. They disappear in the fall, because during the summer the causes producing them, such as the one mentioned above, are comparatively inoperative, the bees being less confined to their hive. Those which were observed in the spring have either died from old age, or were destroyed, like the drones, as useless members of the community.

3. These black bees are not apt to sting, because they are usually encountered at other hives than those to which they belong; and being consciously trespassers and robbers, they are timid and will not attempt to sting, but eagerly seek to escape—"Conscience making cowards of them all."

4. Their sting is not particularly painful, because they do not, like other bees, edulcorate the nectar of

flowers in their stomachs, and hence probably secrete a less virulent poison. Is there not, moreover, in this respect, a remarkable difference in the effect of stings in general? Is a sting as painful in early spring as it is when the lindens are in blossom? These are questions not easily answered. Some persons also are so organized constitutionally, or have in time become so habituated to the poison, that in them the sting of a bee does not at any time, or under any circumstances, cause pain or swelling. Others are so accustomed to suppress their feelings, that, in all ordinary cases, they "grin and bear" such inflictions without wincing. Others again are so exceedingly sensitive, that a sting produces frightful swelling and dangerous illness. Who then can say that the sting of one bee is more, or less, painful than that of another?

5. At the time of the general drone slaughter, as well as at other times in summer, dead black bees are occasionally found in front of the hives. These *may*, indeed, be common black bees, which have died a natural death, or have been massacred and cast out. But they may also be common workers which turned black in the agonies of death, if they were killed by suffocation—which is the usual process. Let any one examine a suffocated queen, and see if in every instance she be not found of a glossy-black color, with but a faint trace of yellow; and that workers also are at times subjected to the same species of capital punishment, is too well known to require proof.

6. They usually disappear at the general expulsion of the drones, because at that period all useless members of the community are banished. It is, however, not correct to say that they are expelled *en masse*, simultaneously with the drones; though till recently I was myself of that opinion. At this period many are cast out, and an unusual number of dead black bees are observed, and it thus came to be inferred that they are expelled or killed in the same manner, and as regularly as the drones. On the whole, more die from superannuation than from any other cause.

Having fully considered the only objection urged against the first proposition, and, as I conceive, finally disposed of the *black bees*, I shall now proceed to discuss the second—arranging it, for greater convenience, under several distinct heads. The second proposition reads thus;

II. In the normal condition of a colony, the queen is the only perfect female present in the hive, and lays all the eggs found therein. These eggs are male and female. From the former proceed the drones; from the latter, if laid in narrow cells, proceed the workers, or undeveloped females; and from them also, if laid in wider acorn-shaped and vertically suspended, so-called royal cells, lavishly supplied with a peculiar pabulum or jelly, proceed the queens.

1. In the normal condition of a colony the queen is the only perfect female present.

This will commonly be conceded, even by those apiarists who believe that, in the normal condition of a colony, the drone-eggs are laid by a distinct class of bees, which they call *drone-mothers*. They cannot, however, do so consistently. For if the queen can lay worker-eggs only, and the conjectural drone-mothers lay drone-eggs only, it is not very clear how the queen can be regarded as, in any respect, a more perfectly developed female than those hypothetical drone-mothers. But we need not now discuss this point, because it will incidentally be disposed of when we prove, as we presently shall, that the queen lays the drone-eggs also.

2. In the normal condition of a colony the queen lays all the eggs found therein.

Since no one now-a-days denies that the queen regularly and exclusively lays all the eggs from which workers and queens are produced, the only question remaining is whether, in the normal condition of a colony, the queen lays the drone-eggs also. Though Dzierzon alleges that he has seen her do so more than fifty times, still the fact is not conceded by all; and many apiarists continue to controvert the doctrine.

Last summer we saw the queen of an undoubtedly normal colony, lay eggs in drone-cells, as we shall relate more in detail when we come to consider the third proposition.

We must regard all further discussion of this point as superfluous till the opponents of the doctrine produce new evidence in support of their views, or new arguments to sustain them.

Though many deny that in the normal condition of a colony, the queen lays drone-eggs, yet some admit that exceptional cases occur when as in drone-breeding colonies, the queen lays the eggs from which the

drones proceed. Others, however, go so far as to assert that, at no time, and under no circumstances, can a queen lay drone-eggs. It must be acknowledged that the latter reasons are more consistent than the former, though they are, in fact, involved in an equally gross error. Instead of relying on sheer *a priori* reasoning, they should take some drone-breeding colony in which eggs are laid with regularity, cell after cell, and in worker cells especially, drum out the bees, and transfer them to an observatory hive, furnished with empty combs. They will then speedily see, what we and others have often seen, a queen laying drone-eggs, or eggs from which drones will in due time be hatched. Experiments, however, demand patient observation, and a degree of tact and skill which all do not possess, and which, it would seem, are more difficult to acquire than the art of composing fanciful treatises on bee culture.

3. The eggs in a normal colony are male and female. From the former proceed the drones. From the latter, if laid in narrow cells, proceed the workers or undeveloped females.

Against this portion of the proposition objections are urged, which will be discussed and exploded as we proceed.

We will now consider the third proposition:

III. The queen possesses the ability to lay male or female eggs at pleasure, as the particular cells she is at any time supplying, may require.

After I had satisfied myself, by experiments instituted in 1851, that normally the queen is the mother not only of the workers, but of the drones also, I became exceedingly anxious to see her supply drone-cells with eggs. I wished to obtain ocular demonstration of the fact. To this end, in the fall of 1851, having meantime examined properly-constructed Dzierzon hives at Carlsmarket, I caused one to be made having a glass door in the rear, which could be covered. It was made of such width as to suit the combs of some of my old hives, and about the middle of October I selected sixteen combs, containing a sufficient winter supply of honey, but consisting of worker-cells exclusively. There was not a single drone-cell in any of these combs. I inserted and arranged them in two tiers, one above the other, and introduced into the hive a strong colony with a young queen. In the spring

of 1852, I fed them lavishly with slightly diluted honey two weeks before the rape came into blossom; and on the evening of the 12th of May, the bees began to *hang out* in clusters. On the 16th, I observed that on all the combs the cells not stored with honey were filled with brood. I now took out the first comb of the lower tier, facing the glass door, and inserted one containing chiefly drone cells—there being only about 250 worker cells in a portion of it. From the reverse side of this comb, which could not be seen when inserted in the hive, I had scraped off carefully all the cells from the foundation or middle partition, and cemented thereon instead, a comb of worker-cells, from one side of which the cells had in like manner been removed. When this was placed in the hive, I could see all the drone-cells through the glass door. It was inserted at 3 o'clock P. M., and was very soon covered with bees, which commenced cleansing the cells, and repairing such as had been broken or sustained injury. In the evening, when the bees had ceased to fly, those which clustered at the entrance were carefully brushed off, and the hive was removed to a dark room in my apiary. After we had again taken out all the combs, to assure ourselves by close inspection that all the cells were still empty, we commenced observations by the light of a small lamp with a movable shade. But in about forty-five minutes we were compelled to desist, because, as the evening was very warm and sultry, the bees soon began to hang out in large clusters, and many of them, attracted by the light of the lamp, perished in the flame. We carried the hive back to its stand, took out the prepared comb, and substituted another for it. On the evening of the 17th, we reinserted the comb, and carried the hive to the dark chamber, having previously made such arrangements as to preclude the outlying bees from seeing the light of the lamp. Our observations could now be pursued more satisfactorily and without interruption—though after a busy day's work, the bees were fanning and humming loudly at the entrance. Some of the cells contained honey carried in on the previous day, but it was not capped. The bees soon removed this, and we anxiously awaited the appearance of the queen. But greatly to our disappointment, she failed to present herself, though we continued watching all night. At five next morning, when the bees in the apiary had

already begun to fly, we once more replaced the hive on its stand, and removed the prepared comb.

On the evening of the 18th we recommenced observing, I engaging to watch from nine o'clock to eleven. Before ten o'clock the queen made her appearance, and remained inactive about five minutes, whilst the workers were bestowing on her their usual caresses and attentions. She then proceeded to inspect a cell by inserting her head in it, and immediately commenced laying. I had aroused my attendant, Gunther, from his nap, as soon as her majesty presented herself on the comb, and we were delighted to behold *the queen of an unquestionably normal hive lay eggs in drone-cells.* To enable ourselves to witness this act, was the sole design of the experiment, and therein we were completely successful. But we were perfectly astounded to see, when the queen came to the worker-cells, that instead of passing them by, as we anticipated she would, she proceeded without hesitation to supply them likewise regularly, cell after cell. "Oh, that Dzierzon were here!" exclaimed Gunther. "Nay," said I, "not Dzierzon, but Busch, the champion of the drone-mother theory—*he* should be here!" *Five* times did the queen change her position, passing from drone to worker-cells, and from worker to drone-cells, continuing to lay till after one o'clock, with occasional intermissions—once for nearly twelve minutes.

When it appeared that she was about to leave the obverse side of the comb, I opened the glass door, caught her, confined her in a cage, brushed the bees from the comb with a feather, and counted the eggs. We found 204 in drone-cells, and 28 in worker-cells. I now removed the prepared comb and substituted an empty drone-comb, to ascertain whether any eggs would be laid *in the absence of the queen.* None were laid, and on the evening of the 20th I found the cells were filled with honey. On the 27th, the cells having been capped, I removed the comb and inserted another empty drone-comb instead. On the 16th of June, when I took out and examined all the combs, I found the drone-comb still empty—containing neither eggs, brood, nor honey. On the 26th of May, a tremendous and devastating hail-storm had destroyed the crops in all the region round, and entirely deprived the bees of pasturage.

In the course of these observations, I discovered what I will here mention incidentally, that bees, when in want of drone-cells, know how to provide a supply for themselves. On portions of several combs they had evidently demolished the worker-cells and built drone-cells instead, numbering altogether 68, in which, manifestly, from their appearance, drones had been bred; and I also saw several drones when brushing the bees from the combs. How suggestive is this fact. Does it not point out clearly the expediency and advantage of restricting and repressing the production of drone-comb in the brooding apartment of a hive, and of removing therefrom all it contains of such comb?

On the 18th of May, I inserted the prepared comb which contained the worker and drone-eggs laid by this queen, in a hive from which a swarm had issued on the preceding day. On the 8th of June I took it out and saw in the cells perfectly developed workers and drones nearly mature.

By this experiment, (which I would suggest that other inquirers also should try, and which I have so minutely detailed to enable others to devise improvements,) it is incontestibly proven that in the normal condition of a colony, the queen not only lays all the drone-eggs, but—what is of still greater importance—that she is able to lay male or female-eggs *interchangeably, at pleasure.*

The fourth proposition of the Dzierzon theory is:

IV.—In order to become qualified to lay both male and female-eggs, the queen must be fecundated by a drone or male bee.

The correctness of this proposition is now admitted by so large a majority of intelligent apiarists, that I might almost say it is universally conceded. A small number only, still regard it as doubtful or continue to controvert it, because hitherto they have not been able to satisfy themselves that the drones are males. Formerly I also refused to receive the doctrine, as it appeared to me to be contrary to all analogy that such clumsy, awkward, sluggish creatures as the drones, so palpably subordinate to the workers, should belong to the male sex. To determine the facts and ascertain the truth, I instituted numerous and most diversified

experiments, which involved the total destruction of twenty-three colonies, and the partial ruin of at least as many more. But the result was, that I obtained clear and conclusive evidence of the virility of the drones, and of the purpose of their existence. Permit me to describe in detail some of the more important of these experiments.

I began in the fall of 1837, after all the drones had disappeared, by removing the queens from two of my colonies. As these colonies were at my late residence, Seebach, and I then held the office of Referendary at Muhlpausen, I found that I could not prosecute my observations with the requisite attention, and was constrained to defer them till the ensuing spring. In March, 1838, I found that neither of these colonies contained brood, though each had reared and still retained a queen. So likewise the young queen reared in a colony from which the old one was removed in that month, laid no eggs.

On the 6th and 7th of March, 1843, having returned to reside at Seebach, and established an apiary there of about one hundred colonies, I removed the queens from six of them. Four of these reared young queens, and two remained queenless. No eggs were laid in any.

In June, 1844, I placed three second-swarm colonies at my mill, situated about two miles from Seebach, having previously removed from them all their drones. In two of them no eggs were laid, but I found some in the third. This staggered me for the moment, and I took up the notion suggested by others, that queens were susceptible of fecundation only at the most genial season of the year. Yet I could not, on the other hand, account satisfactorily for the diverse results obtained. I came to the conclusion finally, that the location of the mill was not sufficiently remote from the nearest apiary to preclude all intercourse between the queens and drones.

I now endeavored to find a locality within the circuit of a league from which no apiary existed and no bees were kept. My researches were fruitless; I could discover no such spot, unless it were in the recesses and seclusion of a distant forest. Even in that by-place it was possible some wild bees might harbor. Inquiring of the forester who had charge of the district, and of the wood-choppers who were employed

there, whether they knew of any wild bees within the designated bounds, or had ever heard of any being observed there, I was answered in the negative. The forester said it was reported in former times bees had occasionally been found, but that after all the old and decaying timber was removed they had disappeared. I then offered the wood-choppers a reward of five dollars for every colony of wild bees they might discover within certain limits; but no one came forward to claim the reward. Thereupon I concluded to make the forest the scene of my experiments. On the 27th of June, 1845, I immersed three second swarms in a bath, removed all the drones from each, hived the bees again, and transported them by night to the spot which I had selected. This was about three leagues from Seebach, and there I concealed the hives carefully in a dense thicket. Besides myself, my overseer, and a faithful old servant who carried the hives, not a soul knew aught of my proceedings. On the 1st of August, having in the meantime repeatedly visited them alone and in secret, I had the hives carried back to Seebach. No brood was found in either on examination, but each contained a vigorous queen. In 1846 the experiment was repeated, with like results. After the lapse of four weeks, two of the colonies contained no brood. The third had deserted its hive.

These experiments fully convinced me that in the absence of drones, no queen could be fecundated, and that the drones are males, the only males in the colony. I did not subsequently repeat the experiments to establish the virility of the drones; nor shall I do so hereafter. I consider the fact as already empirically demonstrated, and the question definitely settled. It is true, Capt. Baldenstein's experiments led him to a different conclusion. But I cannot hesitate to believe that there was, unknown to him, a colony of domestic bees at some point within a short distance of the Italian colonies with which he experimented.

I must also add that, as I distinctly remember, in my earlier experiments many of my experimental colonies produced drones. But I was then still a firm believer of the doctrine that drone-mothers are regular and essential members of the commonwealth of bees, and the production of drones in colonies of any description, never so excited surprise or suspicion as to induce me to note the circumstances under which it occurred in my bee diary.

Let us now consider the proposition next in order:

V. The fecundation of the queen is always effected outside of the hive, in the open air, and while on the wing. Consequently, in order to become fully fertile, that is, capable of laying both male and female eggs, the queen must leave her hive at least once.

He who doubts or denies the regular hymeneal excursions of the young queens, which Janscha first observed, should place a second swarm in some isolated locality, and watch it closely when the bees are disporting, at or about noon, in fair mild weather. By vigilant and persevering attention, he will then certainly see the queen issue, take wing, and return—unless she be unfortunate while away. I have witnessed this in numerous instances. These excursions alone plainly indicate that fecundation is accomplished outside of the hive. But it is clearly proven by the well-known fact that virgin queens with crippled wings, or whose wings have been designedly clipped, never become capable of laying *worker* eggs. On the other hand, the wings of a fecundated queen may be clipped without detriment to her capacity to lay *both* kinds of eggs. I have clipped the wings of eight or ten second-swarm queens just emerged from the royal cells, and not one of them ever laid *worker* eggs.

I would suggest to those who desire to make such experiments, to place the hives level with the ground; otherwise the queen, unable to fly, would be lost. In her efforts to take wing, she falls to the ground, and cannot regain her hive. But if the hive is level with the ground, she will generally crawl back. This also enables one to witness her singular gymnastics in front of the hive, which are sometimes quite ludicrous—especially when only one wing has been clipped.

The invariable inability to lay *worker* eggs, characterising young queens which are, from any cause, incapable of flying, is proof manifest that fecundation is accomplished only outside of the hive, and never within it. This, I think, must be conceded, or will be by those who carefully make the experiments I have suggested.

As for the twin doctrines, that queens are born fertile and do not need fecundation, but only require

exercise in the open air to set their ovaries in action; or that the observed excursions take place only after copulation within the hive, and are mere pleasure jaunts; they are too futile to require serious notice—being mere fanciful conjectures, without a shadow of proof or even plausibility.

It is "a fixed fact" that fecundation is accomplished outside of the hive; and this in every instance, without exception. Hence it is indispensable that the queen must leave her hive "at least once," though repeated excursions may be required. Scarcely one in a hundred becomes fertile after leaving for the first time. Before taking wing on the first occasion, she will generally crawl about on the alighting-board and on the front of the hive, and then hover about awhile with her head turned towards it, alternately receding and returning, evidently noting its form and appearance, and marking its locality ere she takes her departure, so as to be able to find it readily on her return. Nor does she usually remain absent long on this her trial trip, returning to and approaching the hive as warily, and with as many precautions as when she left it. Even on the second occasion, she still displays hesitation and anxiety; but subsequently she goes and comes with the readiness and freedom of a veteran worker. Generally the young queen leaves for the first time while the bees are most busily disporting in front of the hive, as though instinctively conscious that that was the most opportune time. We seldom see one issue before 12 o'clock or noon, or after 4 o'clock, P. M. While she is preparing to leave, the workers pay her hardly any attention, neither feeding nor fondling her; and the drones seem unconcerned and frigid. She commonly begins to make her excursions on the third day after leaving her cell, unless the hive contains other queens, nearly mature, in royal cells. In such case the excursions are deferred till all the rivals have been dispatched or removed.

It is evident that copulation takes place in the air, and while both parties are on the wing. A healthy drone is never seen to alight anywhere except at the entrance of his hive; and queens, when making their hymeneal excursions,always direct their flight upward, and vanish from view at a high elevation. *A priori* evidence of the necessity of this is moreover furnished by the physical organization of the drone.

The next proposition to be examined is—

VI. In the act of copulation the genitalia of the drone enter the vulva of the queen, are there retained, and the drone simultaneously perishes.

This was, in substance, the view held by Huber, and rejected as ridiculous or absurd by Spitzner and others, then and since. It is unnecessary now to recapitulate Huber's experiments and reasoning, because we have in addition recent and positive evidence that his surmise was well grounded. Queens have, in numerous instances, been seen returning from their hymeneal excursions with the evidence of copulation protruding from the vulva as a white threadlike substance. This has commonly been regardrd as the penis of the drone, but is, in reality, only a fragmentary portion of the seminal duct adhering to the severed genitalia. Conjecturing this, I resolved to have the facts ascertained by procuring the dissection of a queen caught on her return. On the 23d of July, 1853, I had an opportunity to secure one whose vulva was distended by a white mass seemingly firmly wedged in, showing that a severance of the entire sexual apparatus of the drone had taken place. I immediately immersed her in diluted alcohol, and sent her to Prof. Von Siebold, who has published the following account of the result of his investigation:

"I was struck, at first glance, by the fact that the orifice of the vulva of this queen was widely distended, having a clearly defined substance protruding—two yellowish horns, curved upward, being particularly prominent. I soon satisfied myself that this substance was not an extruded portion of the intestines, for, by careful manipulation with a pincette, I succeeded in extracting it from the vulva. On examining it with a microscope, I found I had before me the severed sexual apparatus of a male bee. The two yellowish horns were simply those two curved and pointed *cul-de-sacs* which a slight pressure on the abdomen of a drone causes to spring forth, resulting in his death. Between these horns was a dark brown body projecting into the vulva, which I recognized as the penis of the drone. It was hence evident that I had before me a queen bee in whose vulva the detached sexual organs of the male bee remained inherent after copulation."

It would be superfluous to add anything to this decisive evidence in support of the first portion of the proposition; nor need I take the formal demonstration of the second. That the act of copulation involves the death of the drone is well known to all experienced beekeepers. The extension of the genitalia of a drone, by however slight and gentle pressure effected, invariably results in his immediate death. This may be surprising to all, and incredible to many; but the ascertained fact is all that now concerns us. It is not incumbent on the observer to assign any reason for it.

While on this topic, I may state that I conceive that, for effecting copulation, the queen must possess the free and full use of her hinder legs—though the special necessity of this is not apparent, except from circumstances. Dzierzon states that a virgin queen, crippled from her birth in one of her hinder legs, remained sterile after making numerous excursions while drones abounded. I had the curiosity to verify the fact by mutilating the hinder legs of a young queen before fecundation, and though I saw her fly out frequently afterwards, she never became fertile.

It has also been supposed that the queen employs her hinder legs in ridding herself of the dead drone after copulation. But if this be so, it is still manifest from Dzierzon's observation and my experiment, that this is not the sole nor the primary use she makes of them on the occasion.

I will now take up the next proposition:

VII. The fecundation of the queen, once accomplished, is efficacious during her life, or so long as she remains healthy and vigorous; and when once become fertile, she never afterwards leaves her hive, except when accompanying a swarm.

Since it has been shown that the fecundation of the queen is always effected outside of the hive, and fertile queens with defective wings are known to live several years, (I had one that survived five years after losing her wings, (though the colony continues in a prosperous condition all the time, it is evident that, ordinarily at least, fecundated queens do not again leave their hives except to accompany a swarm. But may not occasional exceptions occur? Were all those mistaken or deceived, who declare that they saw *fertile* queens

issue in the spring and at other seasons, though no swarm left? They certainly were mistaken; and I beg those who may hereafter see a supposed fertile queen issue under such circumstances, to make an immediate examination of the colony. I can assure them they will in no instance find either eggs or brood in the cells; or if brood be found, it will be such only as is sealed and nearly mature.

But the question assumes a different aspect when we inquire whether fecundated queens, *which have not yet begun to lay eggs*, do not occasionally repeat their excursions? To such an inquiry, I must explicitly reply in the affirmative. Two cases came under my own observation, (one in 1843, and the other in 1849,) where young queens which I had seen returning with the marks of copulation, did subsequently issue again—the one on the same day, the other on the day after. I also saw each of these return from her second excursion; but neither of them displayed evidence of renewed fecundation. Dzierzon also observed a queen issuing a second time, under similar circumstances. We may therefore dismiss this, and proceed to the next proposition.

VIII. The ovaries of the queen are not impregnated in copulation; but a small vesicle or sac which is situated near the termination of the oviduct, and communicating therewith, becomes charged with the semen of the drone.

"What a preposterous and ridiculous hypothesis!" So exclaim, not I, but others; though I candidly confess that, when I first heard this doctrine announced, I was exceedingly incredulous. It was only after long hesitation and study, yielding gradually and very slowly to the evidence of undeniable facts, that I became a convert. Even then I assented to it as an hypothesis *to be accepted only for the time;* first, because neither others nor I could offer anything better; secondly, because by it all the mysteries otherwise inexplicable, were solved; and thirdly, because many isolated facts corroborated it.

I reasoned thus: Since uniform experience shows that young queens with defective wings never become qualified to lay *worker* eggs, and that healthy young queens, able to fly, become so qualified only when drones exist, it is clear that fertilization qualifying

them to lay such eggs, is effected exclusively outside of the hive. Now, as we further know from experience that queens sometimes emerge from the royal cells with crippled wings, and do yet in exceptional instances lay eggs from which drones are developed, we have only this alternative—either to conclude that such queens lay drone eggs without having been fecundated; or that fecundation, if occurring within the hive, is inefficient. We might be induced to adopt the latter view, were it not also an observed and recorded fact that a young queen with crippled wings reared in an artificial colony, laid drone-eggs, though the hive contained no drones. This being the case, we cannot avoid conceding that, to enable a queen to lay drone-eggs, fecundation is altogether unnecessary. Does not this likewise fairly lead to the further inference that all drone-eggs are unimpregnated?

Again, since it is an admitted fact that the seminal sac or spermatheca of every worker-egg-laying queen contains a milk-white viscid liquid, whilst the spermatheca or every sterile or only drone-egg-laying queen is either entirely wanting, or contains a limpid, watery fluid, is it not reasonable to infer a connection between the production of worker-eggs and the milk-white liquid in the spermatheca, and that the worker-eggs thence derive their impregnating matter?—in other words, that the spermatheca contains the male semen. But if the ovaries be not impregnated in copulation, and all the eggs germinated and developed therein be male, I cannot conceive how the laying of female eggs, which takes place only when the spermatheca contains the milk-white liquid, can be explained otherwise than by assuming that the male eggs are converted into female by being brought into contact with semen from the spermatheca, and that the eggs remain male when not exposed to such contact—an operation which the queen can effect or omit at her option. And that she possesses such optional power is evident from the fact that she frequently passes from worker to drone-cells when ovipositing, and lays, as it were in one breath, eggs from which proceed workers and eggs from which proceed drones.

We will now consider the proposition which contains the most peculiar and characteristic feature of the Dzierzon theory:

IX. All eggs germinated in the ovary of the queen develop as males, unless impregnated by the male sperm while passing the mouth of the seminal sac or spermatheca, when descending the oviduct. If they be thus impregnated in their downward passage, (which impregnation the queen can effect or omit at pleasure,) they develop as females.

From time immemorial naturalists have regarded as universally true, the doctrine that no living creature can be developed from the egg of a female without male impregnation. And when occasionally exceptional cases were adduced, the men of science treated the statements with neglect or contempt, or endeavored to impugn their force or validity by assuming that the observers were either incompetent or careless. But when more recently it was asserted by observers, whose love of truth and skillfulness could not be questioned, that repeated instances had come under their notice where unimpregnated silk-worm eggs had produced larvæ which, passing through the usual metamorphoses, became moths, the necessity of reinvestigating the subject could no longer be denied. At the same time it was announced by other observers and naturalists, that they had witnessed the development of living creatures from unimpregnated eggs of various species of the lower order of animals. Thus, as it were, constrained to the task, the investigation was undertaken by men abundantly qualified by their attainments and skill to conduct it properly.

Though Prof. Luckart had previously reported an instance of such development, the honor of first making an extended and thorough investigation undoubtedly belongs to Prof. Von Siebold, of Munich. He demonstrated clearly that not only do living larvæ occasionally issue from a portion of the unimpregnated eggs of the silk-worm, and develop as moths—some male, others female; but that in various species of butterflies the virgin females regularly lay eggs which, not partially only and occasionally, but uniformly and without exception, produce females.

Prof. Leuckart subsequently noticed a still greater number of exceptions, and says—"There can be no doubt that Parthenogenesis exists far more extensively among insects than is now known or anticipated."

This exception is found also among bees; with this difference, however, that among them, *all* the eggs which remain unimpregnated develop as males, and those which are impregnated invariably develop as females, and that the impregnation of the egg determines its feminine sexuality. Consequently, in the case of bees, not only is every egg susceptible of development though unimpregnated, but masculinity *pre-exists* therein, which—marvellous, indeed!—is transformed into feminity by impregnation with the male sperm.

I am anxious to attract the attention of my readers to this portion of the theory, for it is the most important of all the propositions I have undertaken to discuss. With its aid, almost everything relating to bee-culture becomes intelligible—without it, hardly anything is clear. He who does not fully comprehend this point will grope in the dark in all his practical operations, and be constantly exposed to mistake and disappointment.

1. To demonstrate this proposition, it is necessary to inquire whether there are any queens which have a supermatheca not filled with male semen, and being consequently unfecundated, yet lay eggs from which living creatures, invariably males, are developed. If this can be proven, the fact will be established that the eggs are by *preformation* masculine, and develop without the influence of male sperm. Such queens certainly do occur. I have myself discovered nearly a dozen.

2. On the 30th of June, 1857, Mr. Kehrhalm sent to Prof. Leuckart a queen which had been fecundated in July, 1854, and proved to be unusually prolific till in the autumn of 1856, laying hundreds of thousands of worker-eggs in that period. But in the spring of 1857, and until she was sent away, she laid drone-eggs exclusively. Prof. Leuckart made a microscopic examination of the contents of her spermatheca, and after the most careful search *could not find a single spermatozoa.*

3. I sent to Dr. Barth, three queens which laid drone-eggs exclusively. He examined them carefully, using the celebrated Leuchtenberg microscope at Eichstadt, but found no spermatheca in either. They must consequently have been *unsusceptible of fecundation.*

4. Eggs occasionally laid by fertile workers will hatch, but invariably develop as males. Workers are unsusceptible of fecundation.

5. If eggs required impregnation in order to develop as males, we should undoubtedly find a virgin queen occasionally, some of whose eggs would fail to hatch. But hitherto not a single case of this kind has been discovered, for whenever a virgin queen lays eggs, these, if kept in brooding temperature, will hatch and develop as males.

6. How does it happen that we never find a queen that lays *worker*-eggs exclusively? Why can every queen that lays worker-eggs, lay drone-eggs also? And why is it that so many queens are found that lay drone-eggs only? Simply because the eggs of the queen bee are male originally, and require impregnation for development as female.

7. It is an undoubted fact that a fecundated queen can lay a male or a female egg at pleasure. But how could this be explained, if the male egg required impregnation in like manner as the female? It has been alleged that the queen is not able to *determine* the sex of the egg, but only to *discriminate* the sexes when supplying drone or worker-cells—permitting the eggs to issue from one ovary or the other, according to the requirements of the case at the moment of ovipositing. This is manifestly false, for, how in such a case could a queen which can only produce drones, lay drone-eggs in worker-cells, knowing, as she must on this supposition, that as male eggs these should be placed in drone-cells. Every colony desires first of all to have workers produced, and the queen intending compliance, designs to impregnate the eggs she lays, but is unable to effect it, either because her spermatheca contains no male semen, or because the spermatozoa therein have become effete, or because she has lost the power of exerting the requisite muscular action.

8. It is a fact that when the fertility of a queen begins to fail, she will lay more or less drone-eggs in worker-cells; and it also not seldom occurs with very prolific queens, that drones will be interspersed among the worker-brood. This, too, would be inexplicable except on the above ground; for the queen obviously designs laying not a drone, but a worker-egg. When the fertality of a queen is failing, she is

no longer able to impregnate every egg, because her spermatheca is no longer adequately supplied with spermatozoa.

9. If the male egg does not need impregnation, then pure Italian queens must invariably produce pure Italian drones, and pure commom queens must as invariably produce pure common drones, though each kind were fecundated by drones of the other race. And such is the ascertained fact. Common queens fecundated by Italian drones, furnish the clearest and most reliable evidence. Among the drones produced by twenty common queens, in my apiary, fecundated by Italian drones, and producing a more or less mixed worker progeny, there was not one bearing the slightest resemblance to an Italian drone —all being thoroughly of the common race.

10. In May, 1854, I caught and removed a fertile queen of the previous year, intending to use her in forming an artificial colony. When closing the slide of the queen-cage in which I confined her, her abdomen happened to be caught and violently compressed; and on releasing her, the hinder portion seemed paralyzed for some time. After the lapse of an hour, she appeared to have recovered, and I replaced her among the bees. She soon recommenced laying, and laid thousands of eggs with regularity in worker-cells, as she had previously done, but *every one of them produced a drone*. Probably the muscles connected with the mouth of the spermatheca had been injured; or, as Prof. Von Siebold surmised, the spermatheca itself had been ruptured or severed from the oviduct, thus disqualifying the queen for impregnating the eggs.

11. In the summer of 1854, it occurred to me that if the unimpregnated eggs of the queen invariably develop as males, then every queen that is able to lay *both kinds* of eggs, must cease to lay worker-eggs and lay drone-eggs only, if I could succeed in destroying the spermatozoa in her spermatheca without killing or otherwise injuring her. While endeavoring to devise some mode of effecting this, I read in Muller's "Human Physiology," vol. 2, page 636, that a very high or a very low temperature would destroy the vitality of spermatozoa. I conceived that as heat was the genial element of bees, exposing the queen to a low temperature, yet not so low as to kill her, would effect what I desired to accomplish.

Accordingly, in July, 1854, I confined three very fertile queens separately in queen-cages, placing them in an ice-house, and let them remain there thirty-six hours. They were thoroughly chilled—stiff with cold, and white with frost. I removed them, placing them in the rays of the rising sun. They all remained motionless a long time, but at about seven o'clock one of them began to move her limbs slightly. I presented some honey to her proboscis, and in ten or twelve minutes she was completely revived. The other two were irrecoverably dead. I restored the revived queen to her hive, and she afterwards laid thousands of eggs in worker-cells, but all of them, without exception, produced drones. After dissecting her for examination, it was found that the contents of the spermatheca were of a yellowish hue and more liquid than usual. Obviously the spermatozoa had been killed by the cold and were decomposed; and the queen could therefore no longer impregnate the eggs.

Dzierzon also records three instances, where queens which had been chilled by exposure to cold, laid drone eggs only after reviving, though each had previously laid both worker and drone-eggs.

From these various observations, experiments and facts, I deduce the following conclusion: As it is certain each egg must be impregnated by spermatozoa from the queen's spermatheca, and it is likewise certain, *in the first place*, that queens and fertile workers occur, which though either having no spermatheca or such as contain no spermatozoa, yet lay eggs invariably producing drones only; *secondly*, since queens by compression, exposure to cold, or from other causes, lose the capacity to lay worker-eggs and lay drone-eggs exclusively; *thirdly*, as no queen has yet been found producing eggs which failed to hatch when brooded in a suitable temperature; *fourthly*, as every queen which can produce workers can likewise produce drones, though not a few produce drones exclusively; *fifthly*, since the capacity of normally fertile queens to produce workers or drones at their own option, can only be explained or accounted for on the assumption that the male eggs remain unimpregnated; *sixthly*, since many queens produce drones when obviously designing to produce workers; and *seventhly*, since common queens fecundated by Italian drones produce mixed or hybrid workers indeed, but pure

common drones exclusively—it necessarily follows that all the eggs in the ovaries of the queen are originally and naturally male, and develop as males when laid without having been impregnated; but are changed to female, if impregnated before being laid.

Such is substantially the position I had reached in 1855. Still the demonstrative scientific proof of its correctness was wanting. It was yet necessary that we should ascertain, by means of the microscope, *first*, whether all the eggs laid by the queen are alike in shape or form, and especially whether the eggs producing the several sexes were all furnished with the micropyle apparatus; and *secondly*, whether spermatozoa are found on the micropyle or in the interior of recently laid female eggs, and are wanting on or in male eggs. Providentially, two of the most competent and skillful living naturalists, Prof. C. T. Von Siebold, of Munich, and Prof. R. Leuckart, of Giessen, took a lively interest in my investigations. Both visited me at Seebach, and used their microscopes for a minute examination of the material which my apiaries placed at their disposal.

Prof. Leuckart, who was the first to visit me, was only able to determine that the male, as well as the female eggs, were furnished with the micropyle apparatus, and were precisely alike in shape or form; but he did not conclusively ascertain the second point of the inquiry, for he succeeded only in detecting spermatozoa on the micropyle of two worker-eggs. Having restricted himself to searching for spermatozoa only on the *exterior* of the eggs, he failed to obtain any decisive result. In the case of bee's eggs, the spermatozoa are not constrained, like those of other insects generally, to force a passage through a dense albuminous stratum before reaching the chorion; but are deposited almost directly on the micropyle apparatus, and can enter the micropyle with little delay or difficulty.

Prof. Von Siebold honored me with a visit in August, 1855. He was able not only to substantiate Prof. Leuckart's observations, but to demonstrate the presence of spermatozoa in female eggs and their absence in male eggs—thus solving this interesting problem scientifically, and settling the question.

Prof. Von Siebold was mainly indebted for his success to a novel mode of preparing the eggs for exami-

nation, which occurred to him after numerous failures in his efforts to prepare them in the ordinary manner. He crushed them slowly and gently by means of a thin plate of glass, in such a manner as to rupture them at the lower end or pole, opposite to that on which the micropyle is situated, thus causing the yolk to issue below, leaving a transparent vacant space above, between the chorion and the receding yolk. He directed his attention specially to this vacant space which he thus saw gradually forming under his microscope, as the yolk of the egg was passing out of the ruptured envelopes; and in each of thirty, out of forty, of his prepared female eggs, he found from one to five spermatozoa. In three of these eggs the spermatozoa were still active. On the other hand, on examining twenty-four successfully prepared male eggs, he could not discover a single spermatozoa either on the exterior or in their interior. So late in the season we could not procure a larger number of drone-eggs; but those used were of the same age with a portion of the worker-eggs, and were laid by the same queen. Prof. Von Siebold, in his treatise on "*Parthenogenesis in Bees and Butterflies*," gives a detailed report of this discovery, so startling to all physiologists and so subversive of the hitherto received doctrine of animal reproduction.

X. If a queen remain unfecundated, she ordinarily does not lay eggs. Still exceptional cases do sometimes occur; and the eggs then laid produce drones only.

If the eighth proposition may be regarded as satisfactorily established by what I have submitted in relation to it, hardly any one will be likely to question the correctness of what is advanced in the one now to be considered. The two are in fact in complete accordance with each other. It will be sufficient to show that unfecundated queens do occur, which actually lay eggs from which living drones, and drones only, are developed. Dzierzon mentions a number as having come under his observation. Hitherto I have found only one, which was a remarkable case.

On the 13th of March, 1852, I examined a colony of bees which had been confined to the hive, by stress of weather, since the 16th of January, and found it contained a very large amount of drone-brood. I was thus induced to transfer the colony to an observing

hive. The operation was undertaken by Gunther and myself, and at an early stage of it, we found an active and apparently healthy queen, whose wings were slightly defective. We placed her and the major part of the remaining workers in the observing hive, which had been furnished with an empty comb; and gave them about half a pound of slightly-diluted honey, which was carried up in the course of the night. On the following morning we found some eggs in the cells, and soon after saw the queen laying. She had deposited twelve eggs—which she accomplished in three minutes and two seconds—when I removed and immersed her in alcohol, designing to send her for dissection to Dr. Barth, the editor of the Bienenzeitung. The hive from which she was taken contained 1132 living drones, 892 sealed drone chrysalids, 499 larvæ, and 122 eggs. The worker and drone-cells had been alike supplied with great regularity. The queen was of the preceding summer, having issued with a second swarm. I never before saw a hive so largely and exclusively stocked with drones and drone-brood.

The queen reached Dr. Barth on the 18th of March, and he reported the following as the result of his examination:

"Her external form and appearance were those of a perfect queen. The defect in her wings noticed by you, must have been very slight, as it could no longer be observed when she was taken out of the liquor in which she was immersed. On opening the abdomen, the ovaries were seen fully developed, and the ovarian tubes were densely charged with eggs, which had the size and semblance of those laid by a fecundated queen. No difference whatever was observable even under the microscope. I next proceeded to search for the spermatheca, using the utmost care in dissecting the parts, but after the most minute scrutiny, *none could be found.* This fact is of special significance, as showing that this important organ was either totally wanting, or so slightly developed that it could not be detected. This was a remarkable peculiarity in a queen producing drone-progeny exclusively."

This examination, by so competent a person, I conceive, fully settles the main question. But when first promulgating his theory, Dzierzon was of opinion that such unfecundated drone-egg laying queens were of

rare occurrence. Though, as before stated, I have myself hitherto discovered only one, I am strongly disposed to doubt this, and inclined to think that they are by no means uncommon. In the course of the numerous experiments I made some years ago, to determine whether the drones are males, and to ascertion whether the fecundation of the queen takes place within the hive or not, I remember distinctly that many of my experimental colonies became drone-producing *in the presence of a queen.* But as I then firmly believed that the drone-eggs in every colony were laid by a special class of workers, it never occurred to me to investigate how those drones originated. But now, since numerous and incontrovertible facts have constrained me to adopt a different view, and convinced me that *ordinarily* no fertile worker is tolerated in a hive having a queen, whether she be fecundated or not, I cannot but surmise that those drones proceeded from eggs laid by unfecundated queens; and that, consequently such drone-producing queens are of more frequent occurrence than is commonly supposed. Every intelligent and curious apiarist may, however, readily determine the matter for himself. Let him take a dozen second swarms and clip the wings of the queens; see how many of the colonies become drone-producing; and then carefully ascertain whether the eggs from which those drones proceed are laid by the queen, or by a fertile worker in her presence.

XI. If, in consequence of superannuation, the contents of the spermatheca of a fecundated queen become exhausted; or, if from enervation or accident, she lose the power of using the muscles connected with that organ, so as to be unable to impregnate the passing egg, she will thenceforward lay drone-eggs only, if she lay at all.

The evidence of this is furnished by the following facts:

On the 30th of June, 1857, Mr. Kehrhahn sent to Prof. Leuckart a queen fecundated in July, 1854, which, thenceforward to the fall of 1856, displayed an extraordinary degree of fertility, having in the interim laid several hundred thousand eggs. But in the spring of 1857, she laid drone-eggs exclusively. On dissecting her, Prof. Leuckart could not discover a single spermatozoid in the contents of her spermatheca—the supply having been so completely exhausted, in the course of

three summers, that the queen could no longer impregnate an egg.

In May, 1854, when placing a queen in a cage, I accidentally compressed her abdomen violently. Though she appeared to have sustained no external injury, and speedily regained her usual activity, and recommenced laying in worker-cells with her accustomed regularity when replaced in her hive, the eggs produced drones only. Either her spermatheca was crushed by the compression, or severed from the oviduct; or she was thereby deprived of the use of the muscles connected with the parts.

This occurrence subsequently induced Dr. Donhoff to make an experiment, to ascertain whether the same effect could be produced by intentional mechanical compression of the rings of the abdomen. The queen thus operated on, though previously producing worker-eggs, thereafter produced drone-eggs only.

This will suffice to show that queens may still be sufficiently vigorous to lay eggs, though utterly incompetent to lay any which will or can produce a worker.

XII. As some unfecundated queens occasionally lay drone eggs, so also in queenless colonies, no longer having the requisite means of rearing a queen, common workers are sometimes found that lay eggs from which drones only proceed. These workers are likewise unfecundated, and the eggs are uniformly laid by some individual bee, regarded and treated more or less by her companions as their queen.

1. In queenless colonies, workers sometimes occur which lay drone-eggs.

This is no longer doubted or denied by intelligent apiarists who have had ordinary experience in bee culture, and have properly used the opportunities for observation which they have enjoyed.

2. But drone-egg-laying workers are found only in colonies which no longer possess the requisite means of rearing a queen.

There may be some rare exceptional cases, but ordinarily this is true. In colonies which have no queen, but have worker-eggs, unsealed worker brood, or larvæ in royal cells, no drone-eggs are laid. In colonies which remain or become queenless after sending out a swarm, and in swarms which become queenless, drone-

eggs are not laid immediately after the loss or deprivation has occurred, even though they ultimately become drone-producing stocks. Generally some considerable time elapses before drone-egg-laying begins, as will be shown when considering the next proposition.

3. The eggs laid by fertile workers produce perfect drones.

I firmly believe this, because the drones thus produced, precisely resemble, in every respect, those bred in colonies having fully fertile queens. I have no direct and positive evidence to adduce, indeed, though I can indicate a process by which it may be ascertained whether they really possess the attributes of virility. So soon as it is observed that a queenless colony contains drone-eggs or larvæ, let the combs be taken out successively, brush off the bees into a suitable box, immerse them in water, and carefully pick out all the drones, so as to be sure that all those which may have been hatched from eggs laid by their late queen are removed. Then replace the combs in the hive and return the bees after they have become dry, adding to them, if they be few in number, a reinforcement of workers taken from some other colony, and carry the hive to some isolated location, where neither wild nor domestic bees are found. Ere many days, drones emanating from eggs laid by a fertile worker will make their appearance in the cells. Now take out the combs again, brush off the bees, return the combs to the hive and introduce in it an unfecundated queen with a suitable number of workers, from among which all drones have been carefully removed. Then immerse in water the bees which were brushed from the combs, pick out the drones and place them in the hive containing the unfecundated queen. Confine and carry away the immersed workers. Leave the hive in its isolated location; and if under these circumstances the queen becomes fully fertile—that is, capable of laying worker-eggs—then the perfect virility of drones hatched from eggs laid by fertile workers, will be at least *highly probable.* I say highly probable, for it would still not be absolutely certain, because various unanticipated casualties or occurrences may throw doubt on the reliableness of the result, and render a repetition of the experiment, under more favorable auspices, necessary or desirable. I would

suggest to bee-keepers favorably situated, to institute such an experiment for their own satisfaction and for the elucidation of the truth.

4. The drone-egg-laying workers are not fecundated.

Prof. Leuckart dissected a large number of fertile workers sent to him by Dr. Donhoff and myself, and though he found perfectly formed eggs in the ovarian tubes of all of them, the spermatheca in each was merely rudimental and so imperfectly developed that it required the aid of a microscope to detect it, and contained no trace of sperm or seminal matter. It was thus clearly demonstrated that these bees were neither fecundated nor susceptible of fecundation. Prof. Von Siebold, who likewise dissected some fertile workers, and fully corroborated the statement of Prof. Leuckart, remarks that "the peculiar structure of the worker's abdomen precludes concourse with the male."

5. The drone-eggs found in queenless colonies are all laid by one individual.

This may not be universally true; but according to my observations, it is generally the case. Last September, I divided a queenless drone-producing colony into two nearly equal parts. Eggs continued to be laid in only one of them.

6. The bees of the colony regard and treat the fertile worker more or less as their queen.

This is evident from the circumstance that it is, in many cases, almost impossible to detect the queenless condition of a colony from external appearances or the general deportment of the workers. These frequently will labor as industriously, carry in pollen as plentifully, and defend their hive with as much spirit and pugnacity, as if they had a healthy and prolific queen. They will neglect to rear a queen from eggs or larvæ when introduced, and reject and kill a fertile queen when offered to them. This can only be explained on the supposition that the bees regard the fertile worker as a genuine queen.

7. The fertile workers which make their appearance in queenless colonies, are not designedly reared by the bees, but occur casually only, and must be regarded as abnormal or exceptional productions.

They originate mainly when a colony undertakes to rear young queens, especially when the old queen has

been suddenly lost or removed. A number of royal cells are then hastily started, and some of them afterwards abandoned, the larvæ having meanwhile fed with royal jelly. The development of the sexual organs, and more especially of the ovaries, is thus probably stimulated, and though supplies of such nutriment are subsequently withheld, the capacity to lay eggs more or less freely has been conferred. I have frequently marked the worker-cells thus incipiently transformed, and know that they were reconverted to their original form and character, their inmates being again treated like simple worker-larvæ as at first, though I saw that royal jelly had been lavishly introduced. Such cells may readily be distinguished even after their reconversion, as the bees cap them with a broader and more convex cover than ordinary worker-cells.

Huber conceived that workers reared in the vicinity of royal cells, may occasionally receive a portion of the royal jelly, and thus be stimulated to fertility. Dzierzon concurs in this view, in so far as it may account for the production of fertile workers in some instances; but believes also that they may and do originate in various other modes. Every hypothesis, however, yet submitted from any quarter, rests chiefly on the assumption that development has by some means been over-stimulated for a brief period; and as the result affects the sexual organs more especially, the quantity and quality of the food administered have been looked to as the exciting cause.

XIII. So long as a fertile queen is present in the hive, the bees do not tolerate a fertile worker. Nor do they tolerate one while cherishing the hope of being able to rear a queen. In rare instances, however, exceptional cases occur. Fertile workers are sometimes found in the hive immediately after the death or removal of the queen, and even in the presence of a young queen, so long as she has not herself become fertile.

On the 17th of July, 1852, the young queen reared in a colony from which a swarm had issued was lost; and on the 18th another colony in similar circumstances lost its queen. The first was in a common straw hive, from which the combs could not be lifted. On the 29th of July, I cut them out and found neither eggs

nor larvæ in the cells. I transferred the bees to a small hive furnished with combs in frames, and on the 9th of August, eggs were seen in the cells—being 23 days after the queen was lost.

The second colony was in a movable-comb hive. I examined the combs on the 29th of July, but found no eggs; nor could any be discovered on the 9th of August; but on the 20th, the cells contained a number of larvæ. Here, five or six days more must have elapsed, than in the first case, before egg-laying began.

I might cite a number of other instances showing, like these, that usually egg-laying by fertile workers does not commence till some time after the bees cease to cherish the hope of rearing a queen.

I have, however, one instance showing that a fertile worker must have commenced laying at an earlier period, and have continued it for some time after the bees had begun to rear a queen. On the 29th of May, 1853, I drove out a swarm from one of my hives, and twenty days thereafter sealed drone-brood was found in the parent hive, which emerged on the thirty-third day after the driving. But on a further examination of this hive on the same day, I found a fine fertile queen, plenty of eggs, and some newly-hatched larvæ. Allowing twenty-four days for the development of a drone, the drone-eggs must have been laid within nine days after the driving; and, as it would seem, several days after the bees had begun to rear a queen, because the worker-larvæ selected for that purpose must have been in a cell not yet sealed, when the transformation was undertaken. This, indeed, cannot but be regarded as an exceptional case, but it shows that a fertile worker may commence laying almost immediately after the loss or removal of the queen, and may be continued during the rearing of another, and probably till the one so reared has herself become fertile.

FERTILITY OF THE QUEEN

We restrict our inquiry to three points:

First—What is the climax of fertility to which a queen may attain—that is, how many eggs can she possibly lay in a given time, as for instance, in a day? Second—What conditions her fertility? Third—What causes it to diminish? Fourth—What causes it to cease?

1. As to the utmost attainable extent of oviposition, it it sufficient to tilt up a common hive at different seasons, to become satisfied that the fertility of the queen reaches its acme of development in the months of May and June, when the weather is warm and moist and pasturage abundant; and that at this period the number of eggs laid daily must be very great. But the question is, how many eggs can she lay in a day? Dzierzon says about three thousand.

In 1846, I experimented as follows: On the 7th of May, a populous swarm issued from one of my largest colonies. I placed it in a hive fully furnished with empty combs, and at 10 o'clock set it on the stand of the strongest colony in my apiary, which was removed to make room for it. I had weighed the hive and its contents before introducing the swarm, and in the evening found that the swarm weighed 15 ℔s. 1 oz. Though the population was immensely increased by bees from the removed colony, I assume only 10 ℔s. as the net weight of the bees, because the stores gathered by them that day certainly weighed 5 ℔s. On the 8th, they carried in 9 ℔s. 6 oz.; and on the 9th, 11 ℔s. 2 oz.—the hive being completely covered with bees. The weather and the pasturage could not have been more propitious. The air was warm, moist and perfectly calm, and a large rape field within 200 yards of the apiary was in full bloom—a yellow sea of flowers. The weather continued so till the 10th, inclusive, and at 10 o'clock on the forenoon of that day, I took out all the combs, brushed off the bees, and transferred the former to an empty hive. I then carried them to my chamber, and began to count the eggs, marking each row of cells, when counted, by inserting a needle. It was an arduous task, and lasted fully six hours; for I could not place the combs on a table, but had to keep them suspended vertically, to prevent the honey from flowing out of the cells. Whenever a comb was examined and the eggs counted, the number ascertained was at once noted down, having previously tallied the hundreds to guard against error. It was near 4 o'clock in the afternoon when the enumeration was ended, and the aggregate was found to be 4813 eggs and larvæ, for a few of the eggs were already hatched. Consequently the queen must have laid, on the average, 1604 eggs per day. There may have been from fifty to a hundred more than I counted, but certainly there was not one less.

It might be supposed that Dzierzon was in error in assuming that a queen can lay 3000 eggs in a day. I believe that he is right, but wherein was the discrepancy? Clearly in this—that I used for the experiment a queen issuing with a *natural* swarm, instead of one accompanying a *forced* swarm. I am now satisfied by the most decisive evidence, that under certain circumstances, the most fertile queen will, even when weather and pasturage are most propitious, lay very few eggs in a day. I have examined the interior of hives at the height of the honey season, when the bees were carrying in most abundantly, and found honey stored in the upper portions of the combs, then followed brood in every stage of development down to the bottom board, with scarcely a cell left empty. Marking a certain space occupied by brood nearly mature, I observed that when the young bees emerged, the cells were, for the most part, filled with honey on the following day, leaving few comparatively—hardly fifty in all—at the disposal of the queen, to be re-supplied with eggs by her. Thus, while the weather was favorable, and pasturage most ample, the queen laid few eggs per day, obviously *from the want of empty cells.*

I frequently shook forced swarms on the ground, in order to let the bees enter the hives intended for them, and noticed, in almost every instance, that the queens, though their wings were perfect, were not in a condition to fly. I tried repeatedly to induce them to take wing by placing them on my hand or casting them in the air. They would either crawl about slowly and clumsily, or drop almost perpendicularly to the ground. Now, it is a well-known fact that the old queens which accompany first swarms, fly with great readiness and ease, if their wings are perfect—because, for some time prior to the issuing of the swarms, they were constrained to repress the development of egg-germs, by the want of empty cells in which to oviposit. They are, consequently, more slender and lighter than the queens of forced swarms usually are, in whose hives empty cells were plenty, and there was no necessity for repressing the production of eggs. A queen, taken, as it were by surprise, while laying with all the vigor of her natural ability, and suddenly transferred to a hive furnished with plenty of empty comb, will continue to lay in her accustomed manner; whereas, one issuing with a first swarm requires time to recover her

powers,and will probably not re-commence laying with her pristine vigor till after the lapse of from 36 to 40 hours. It can generally be told at a glance, whether a fertile queen can fly or not. For such experiments, therefore, forced swarms should be taken from colonies in which egg-laying was prosecuted with full vigor; or if natural swarms be used, the eggs and larvæ should be counted, not on the third day after hiving, but on the tenth or twelfth. Had I done so in 1846, I should probably have obtained a different result, and one more closely approximating the estimate of Dzierzon.

On the 28th of June, 1851, the immense amount of brood in one of my hives attracted my attention, and I undertook, with the aid of Gunther, to take an accurate census of it. The hive was 18 inches high, 15 inches broad, and 11 inches deep, and was occupied by two forced swarms united and introduced on the 10th of June. It was nearly full of comb, and the weather during the month had been peculiarly favorable to breeding. We took out comb after comb, brushed off the bees and counted as we did in 1846, though with more ease, as we were not incommoded by honey—of which the combs contained very little. There were 38,619 cells occupied by brood, and if we assume 21 days as the average time required for the maturing of worker, the queen must have laid 1839 eggs per day, on the average.

When we consider, however, that the bees had, since they were hived, built nearly all the combs (the frames having simply been provided with guide pieces), and that, consequently, the queen had at the outset hardly cells enough at command, I cannot longer entertain a doubt that many a queen, in peculiarly favorable circumstances, does sometimes lay 3,000 eggs and upwards per day. Though Dzierzon found in one of his mammoth colonies, 60,000 cells filled with brood, so prolonged a continuance of profuse oviposition must still be regarded as a rare occurrence. And if I were asked how many eggs a queen lays per day on the average, *during the period* including the months of May, June and July, I should probably not be willing to name a higher number than six hundred. For there is a wide difference between what a queen *can do*, *occasionally* and *under favorable contingencies*, and what she *usually and regularly does*.

2. By what is the fertility of the queen conditioned?

(1) By the personal vigor and health of the queen. There is often a great difference in this respect between queens of the same age, and while all other circumstances are alike. We frequently find two colonies equally populous in April, and in May the one shows no marks of improvement, while the other is ready to send off a swarm.

(2) By pasturage, weather and season.—In the northern sections of the temperate zone, where in May and June the face of nature displays "one boundless blush," and her form seems veiled in an "empurpled shower of mingled blossoms," the fertility of the queen reaches its highest development, and the hives are almost literally crammed with brood. But that pasturage and weather do not alone produce this result, but that the *season* likewise contributes its influence, is evident from the fact that in July, however favorable the weather and abundant the pasturage, sometimes even excelling the condition of June, there is almost always a smaller amount of brood produced.

(3) By the age of the queen.—Old queens are never as fertile as when they were when young. They are most fertile when one year old, or in their second year; and this accounts for the usually thriving condition, in the ensuing year, of second swarms and old stocks which have sent forth swarms.

(4) By the character of the combs.—Colonies having new combs with worker-cells, to the exclusion of drone-cells, will produce much more brood than those which have old combs or such as contain principally drone-cells. Where a large quantity of drone-combs are built, in process of time the bees become discouraged and disinclined to labor—doing so with their wonted energy, only for a few days in the height of the honey season. The production of drone-comb can be regulated or prevented altogether, when desired, only by the use of movable-comb hives.

(5) By keeping bees in warm hives, which contain plenty of store honey; and especially by stimulative feeding.—Warm hives and a large stock of honey, within the reach of rich supplies of pollen, exert a powerful influence on the production of brood in the spring. Nothing has so encouraging an effect as a judicious resort to stimulate feeding, when a hive is populous and its queen vigorous. Dzierzon rather

discountenances the process, but experience has taught me that a proper use of it will cause the cells to be crammed with brood in an incredibly short time, and that colonies so treated produce the earliest swarms.

(6) By populousness.—The queen of a strong colony will lay much more fully, and commence doing so much earlier, than one associated with a feeble body of workers. The successful development of brood also requires a certain degree of heat in the hive; and this can only be seasonably generated and adequately diffused by a dense population.

3. What causes diminished breeding?

This question is in the main, answered by what has already been stated. In August, a diminished production of brood is perceptible in almost all colonies. Stocks which have yielded no swarms, and the earliest first swarms, will contain very little brood about the middle of September; and from the middle of October, hardly any will be found; except, in cases where diluted honey has been liberally fed—the results of which have no bearing on this question. Strong colonies will, in *mild* winters, or when they have been protected from severe cold, contain some brood at about New Year, and very generally about the middle of January. I have ascertained these facts by repeated inspection of such colonies. Feeble stocks commence breeding about the middle of March, or not till the beginning of April. A strong stock, which I examined on the 18th of January, 1846, contained 162 chrysalids, 280 larvæ and 136 eggs. A month later, the same stock contained 716 chrysalids, 1204 larvæ, and 918 eggs.

4. What causes a total cessation of a queen's fertility?

The queen's fertility decreases with advancing age. She lays more and more sparingly, and no longer deposits her eggs in close and regular order in the cells, but in a scattering, irregular manner. Finally, when the contents of her spermatheca are exhausted, brooding either ceases entirely, or only drones make their appearance. I reserve my remarks on this interesting condition of matters for a future occasion.

I will add a few remarks involving theory only incidentally, and having a more direct practical application.

1. In how many days from the time the egg is laid is a queen bee fully developed?

Without recourse to special experiments it is by no means easy to arrive at a satisfactory conclusion; though we may readily deceive ourselves and fancy that the queen matures in a shorter time than is actually required. In pre-constructed royal cells, the queen is rarely seen to lay, and in post-constructed cells, we cannot know with certainty the age of the larva selected for the loyal dignity. Thus the matter remains, in a large degree, one of doubt and conjecture. I made two experiments in the hope of settling the question.

On the 6th of July, 1851, at 11 o'clock in the forenoon, I introduced a small forced swarm in a hive containing, among empty frames, one filled with comb. At one o'clock in the afternoon I examined the latter and found a considerable number of eggs in the cells. I then removed the queen, took out the empty frames, substituting for them others filled with comb, and placed the one containing the eggs against the glass side of the hive, so that I could conveniently watch the construction of the royal cells. Only three were built, all of which were still open on the evening of the 15th. At 5 o'clock on the morning of the 16th, two were closed—nearly ten days, consequently after the eggs were laid; and at 3 o'clock in the afternoon, the third was also closed. This last was preserved by the bees, and the first two were destroyed by them on the 21st. On the 24th, at 10 o'clock in the forenoon, the remaining cell was still closed; but at 2 o'clock in the afternoon it was open and the queen had emerged. Hence the time required for the development of the queen, in this instance, was fully eighteen days. A second similar experiment, made in 1853, furnished a slightly different result. The queen emerged in seventeen days. I neglected, in this case, to note the time when the cell was closed.

These experiments show that the opinion generally entertained, that the queens emerge between the seventeenth and eighteenth day after the eggs are laid is correct. The result obtained when forced swarms are made, also corroborate this; for, according to my observation, *teeting* is usually heard, in the parent hive, on the fourteenth day. Now, if we assume $2\frac{1}{2}$ days as the time required for the egg to hatch, and that the

larva selected was 1½ days old when chosen, we shall again have from seventeen to eighteen days as the time in which the queen matures. I have, indeed, frequently heard the *teeting* on the thirteenth day; occasionally, though rarely, not till the fifteenth; and still more rarely as early as on the twelfth. Dzierzon says he has heard it on the tenth; but this is readily accounted for by the varying age of the larva chosen, and is consequently entirely reconcilable with the normal period from seventeen to eighteen days. I will only add, in passing, that the bees do not, as is commonly stated, usually select a larva *three* days old, but in most cases a younger one. But whether they occasionally also select an egg for the purpose, is an interesting query, to which, however, I have not hitherto directed my attention.

2. How old may a worker-larva be, and still be susceptible of development as a perfect queen?

The current opinion is that a queen can only be reared from a worker larva not much more than three days old. Dzierzon says: "I have noticed that worker larvæ, so far advanced that they nearly fill their cells, will still be developed as perfect queens, if before capping, the cell be somewhat enlarged and widened, and the larvæ supplied with the appropriate pabulum."

Incredible as this at first seemed, I have found it is nevertheless true.

3. How do queen-bees emerge from the royal cells?

It is commonly said that they sever the cap of the cell by a circular cut along its periphery, allowing it to adhere to the body of the cell by a narrow strip only, which serves as a hinge, and then crawl out. This is true, yet still not entirely correct.

On the 25th of June, 1853, about ten o'clock at night, I heard a *teeting* in one of my colonies which had not yet swarmed, and also heard several *quahking* responses. The occurrence being somewhat unusual, attracted my attention and excited my curiosity. The tones were louder than those usually heard prior to the issuing of a second swarm, doubtless because the young queens were fully mature—swarming having been prevented by a protracted spell of rainy weather. The queens may, besides, have all been of nearly the same age, the old queen having probably been undesignedly removed some time before, in the course of some operations I had made. Early next morning I

took out comb after comb, to secure the queens which had not yet emerged, by removing the royal cells. We found one queen at large, and ten others still confined, though fluttering and *quahking* occasioually in their cells. The emerged queen continued *teeting*, precisely as though she were still in the hive and crept about slowly on the comb, endeavoring to conceal herself among the workers, and *teeting* at brief intervals. When uttering these sounds, she stopped, turned her head downward, fixing herself firmly to the comb with her fore feet, and appeared also to press her abdomen against the comb. Her wings remained entirely motionless. The emission of the sounds evidently required a considerable effort, as her abdominal rings became visibly distended and diverged. When teeting, she cowered very close to the comb, and the abdominal rings, though only slightly moved, still partly covered each other.

Of the queens still enclosed in their cells when I opened the hive, seven emerged in about eight minutes, and I caught and confined them as they came forth. In one of the cells which I held in my hand, and in which there was much fluttering, I saw the queen suddenly pass one of her mandibles through the feeding and breathing aperture of the cap, and almost instantly emerge after making a circular cut along the margin of the cap. She obviously turned herself round during the operation, but did not cut the cap entirely off. It still adhered to the cell by a slight slip or band, and being pushed forward as the queen crawled forth, it at once fell back and closed the orifice when she had fully emerged. Her mandibles must be exceedingly keen cutting instruments, and her strength great, or she could not perform the operation so expeditiously.

I placed two newly emerged queens under a bell-glass. At first they simply crawled about, seemingly unconcerned, on the inner surface of the glass; but they soon met and engaged in combat. They seized each other with their feet and mandibles, rolled about, like two contending workers, and one of them was speedily dispatched. Two others also placed under the glass, acted exactly in the same manner. This was the first opportunity I had to witness a royal combat. Ninety-nine queens of every hundred destroyed, are killed by the workers where one perishes in royal combat. Queens cannot, indeed, readily get into conflict, because when two chance to appear simultaneously in

a colony, one or both are surrounded and imprisoned by workers. Royal combats are the exception.

4. Why do queens teet and quahk?

Young queens teet and quahk from sheer jealousy; every queen, before emerging, quahks for a time, to assure herself that no rival is at large in the hive. Not till after her reiterated calls have remained unanswered, does she feel herself safe, and release herself by severing the cap of the cell.

It has sometimes been alleged that old fertile queens never teet. Yet, when making artificial swarms, and in other operations where much smoke was employed, I have occasionally heard old queens teet. On such occasions it seems to be an expression of alarm and anxiety. But the old queens will teet also, when, in a colony ready to swarm, but detained by unfavorable weather, the embryo queen becomes so far matured as to be able to quahk before the swarm departs. In such cases the old queen is not always killed, nor the young one torn from her cell.

5. Do drones subserve any secondary purpose?

The sole design of their existence is, the fecundation of the queen bee. Some enquire, "If the fecundation of the young queens be the sole purpose for which drones exist, why are so many produced, when one or a dozen would amply suffice?" So large a number of drones is required, because, if only a few were provided, the queen might fail to encounter one in her excursions; and these excursions are indispensable, since, to free the queen from perpetual annoyance by the rivalry of drones, it has been wisely ordered that sexual concourse shall not take place within the hive, but only on the wing and in the open air.

6. Are those drones which proceed from the eggs of fertile workers, virile males?

Dzierzon first propounded this query, and answered it affirmatively—conceding their perfect virility. On the 2d of September, 1853, I broke up a drone-producing colony, which I had expressly reserved in my apiary for investigation. I transferred to frames, the combs which contained from 800 to 1,000 cells of brood in various stages of development, and placed them in a Dzierzon hive. After immersing the bees in water, and carefully picking out and destroying all the drones, I introduced the bees and placed the hive in the bushy top of a willow tree, so arranging the dense

foliage beneath, above, and around the hive, that it could not be seen. All the drones which thenceforward emerged, emanated from eggs laid dy a fertile worker, for the colony had been queenless since the middle of June; and I presumed other drones would scarcely find their way thither at that season, as the nearest known apiary was at least 1½ miles distant.

On the 14th of September, an unusually fine and warm day, I took from one of my colonies recently deprived of its queen, a sealed royal cell containing a maturing embryo, bedded it on cotton and carried it to the place where the hive was concealed. After satisfying myself that my proceedings had not been observed by any one, I ascended the tree, opened the hive, and inserted the royal cell in one of the combs. A considerable number of drones were then flying. Replacing the foliage, I left the hive till the 30th of September, when I found a queen, eggs and larvæ regularly placed in the cells. All the drone-brood had disappeared, the unmature having doubtless been destroyed by the bees, when the young queen became fertile. On October 12th young workers emerged.

7. Diminutive drones.

I have adopted the following hypothesis respecting the origination of the diminutive drones, based on what has come under my notice at various times. I suppose that when the supply of spermatic filaments in the liquid contents of the queen's spermatheca is nearly exhausted, she is no longer able to impregnate every egg she lays. But, being unconscious of her partial disability in this respect, she continues to oviposit in worker-combs as theretofore, and thus some unimpregnated eggs chance to be deposited in cells, interspersed among those containing eggs duly fertilized. The former, of course, develop as drones; but their true character being unsuspected or undetected by the workers, their cells are capped with a *flat* cover. Thus cribbed in on all sides, their growth is repressed—the more rapid and vigorous progress of the worker-larvæ by which they are surrounded preventing *lateral* expansion, while the *flat* cap precludes longitudinal extension. In this manner, I conceive, do these diminutive drones originate. The larger number, however, perish prematurely in their cells, being unable, from the peculiar structure of their mandibles, to perforate the *flat* cover by which they are confined.

THE DZIERZON THEORY

INDEX.

Bienenstöcke mit beweglichen Gestellen.

Honig-Pumpen.

Italienische Bienen u. Königinnen.

Bienen-Räucherer, Bienenschleier, Gummi-Handschuhe, Künstliche Honigscheiben-Fundamente, Saamen für Honig-Pflanzen,

sowie Alles, was Bienenzüchter gebrauchen.

Zu verkaufen bei:

Thomas G. Newman.

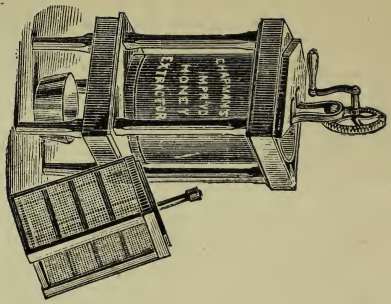
Herausgeber des

"The American Bee Journal."

The American Bee Journal wird monatlich herausgegeben und kostet $2 per Jahr im Voraus. Es ist die älteste Bienen-Zeitung in der Welt, die erfolgreichsten und gebildetsten Bienenzüchter in Europa und Amerika schreiben für ihre Spalten.

CHAPMAN'S

IMPROVED



HONEY

EXTRACTOR

Received the highest award from the Judges at the Centennial Exhibition. **Grand Medal and Diploma.** The best Extractor. A geared machine, nothing but the combs of honey revolve. Every machine warranted.

The thinnest and most elastic, curved-bladed **Honey Knife** made, furnished with each machine.

Price $12 and upwards, according to finish.

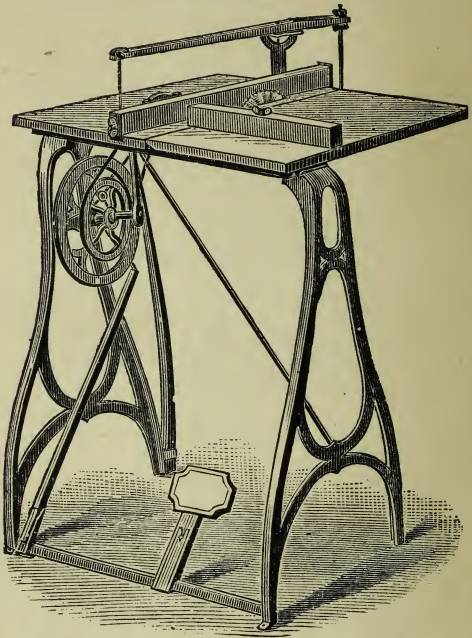
FRANK W. CHAPMAN, Morrison, Ill.

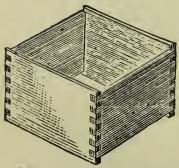
BARNES'

Patent Foot-Power Machinery,

CIRCULAR and SCROLL SAWS, LATHES, &c.,

Fancy Woods and Designs. Ten different machines suited to the wants of mechanics and ameteurs.



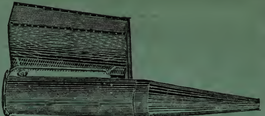


For making BEE HIVES, Section Honey Boxes (see cut), Fruit and all similar boxes, the above machine is especially adapted. The old style thrown aside where these are known. Say where you read this and send for a 48 page Catalogue FREE.

W. F. & JOHN BARNES,
Rockford, Ill.

Mr. Nelson Miles, Lapeer, Mich., says of the Barnes Machine: "The combined saw you sent me last year works well in making BEE HIVES. I have given it a thorough trial in making several hundred hives."

BINGHAM'S BELLOWS SMOKER.



Success in bee-keeping depends largely upon the fact that the apiarist understands his business, and is master of his bees. Smoke is the only ever-ready and safe means by which they can be controlled. Movable combs at certain times and seasons are almost immovable without smoke — and no apiary can be managed so as to keep the bees good-natured without abundant and manageable smoke.

The direct-draft bellows Smoker invented and made by T. F. Bingham is vastly superior to any other device for the fumigation of bees. It is made of heavy leather, with thick wooden sides. fine steel spring, and a strong tube for holding the smoke, wood or other material to be burnt. The fire tube is so arranged that while the smoke is readily forced out, the regular draft is not through the bellows, as in other bellows smokers, but is direct. This feature is new and original, and in the Patent Office for a patent. This "cut off" not only supplies the fire with air in abundance but also prevents sparks and fire from entering the bellows and enables me to use a tight-spring valve, always ready for a puff, no matter which side or end is up; the slightest tap of the finger upon the bellows sends a smoke-ring into the air.

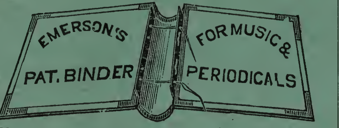
The "direct draft" Smoker will last ten years, and in that time enable the user to do an extra year's work, and will do more to keep him and his bees good-natured than ten tines its cost spent in any other way. Prices: Extra Large size, $1.75, by mail, $2.00; the Standard, $1.50, by mail. $1.60; Small size, $1.00, by mail, $1.25. **T. F. BINGHAM, Abronia, Allegan Co., Mich.**

ABBOTT POCKET MICROSCOPE

Is a useful Instrument for Bee-Keepers, Teachers, Farmers. Merchants, Mechanics, Physicians. Botanists, and others. By means of a cage, accompanying each Instrument, one can examine small Insects or Worms alive. The

EYE OF A BEE,

or other small insects can be readily seen. We have made arrangements to furnish the POCKET MICROSCOPE at the manufacturer's price, $1.50.



We can furnish Emerson's Binders for THE AMERICAN BEE JOURNAL, at the following prices, postage paid:

Cloth (to hold 1 vol.), each	.50
Leather and Cloth	.75

THOMAS G. NEWMAN & SON,
1?4 Clark St., CHICAGO, Ill.