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SOME FURTHER COMMENTS ON THE GUATEMALAN
BOLL WEEVIL ANT.

IN his reply to my remarks on the feasibility of establishing the Guatemalan boll weevil ant in Texas, Dr. O. F. Cook shows how dubious are the claims for the much-advertised efficiency of this insect. Clearly there are two distinct problems involved in the discussion; first, the establishment of the boll weevil ant in the southern states, and second, its efficiency as a boll weevil destroyer. My paper dealt largely with the former, since it is, of course, the *conditio sine qua non* of the latter problem. Dr. Cook calls my remarks a '*post facto* prognosis,' wishing, I suppose, to create the impression by this *contradictio in terminis*, that his *Schmerzens-kind*, the kelep, to which the Department of Agriculture has been standing sponsor, is doing remarkably well. I will pass over the fact that this implication is hardly borne out by the latest reports from the field of experimentation, and consider some of Dr. Cook's statements.

He says that 'it was obvious to Professor Wheeler from the first that the case was hopeless.' This statement is false, inasmuch as neither I nor anybody else outside of the Department of Agriculture could have had *any* opinion on this subject till very recently, for the very simple reason that the scientific name of the ant was not made public by the Department till its great value as a boll weevil destroyer had been boomed in all the newspapers of the country. The kelep, as

Dr. Cook informs us, was discovered 'on the cotton April 20, 1904, in Alta Vera Paz, Guatemala, and its efficiency as a destroyer of the Mexican cotton boll weevil was demonstrated the following day.' But even in his official report, which could hardly have been published before August 1, the scientific name of the ant was not given and it was several weeks later before I could ascertain it.

Dr. Cook further says that I have disregarded 'several facts which might have mitigated the confidence of the prophesy.' The first of these is a straw fact of Dr. Cook's own manufacture, namely the supposition that I am of the opinion that *Ectatomma* is very much like *Odontomachus*.* I am at a loss to

* Dr. Cook is 'ready to follow Mayr and Ashmead in assigning these genera to separate families.' In other words, the genus *Odontomachus* should be separated from the Ponerinæ (or Poneridæ as Cook and Ashmead persist in calling the group) and made the type of a distinct family, the Odontomachidæ. This was Mayr's opinion many years ago, but it is probable that he now believes with the eminent myrmecologists Emery and Forel that *Odontomachus* (together with *Anochetus* and *Champsomyrmex*) can not be separated as a distinct family, but has hardly more than tribal value. The only characters on which such a separation could be effected are the peculiar shape of the petiole and mandibles. But the very same kind of a petiole is found in certain undoubted Ponerinæ, like the South American *Leptogenys unistimulosa*, and if the shape of the jaws is such an important character, we should have to make several families out of such genera as the myrmicine *Strumigenys*, some species of which, like *S. louisianæ*, *grandidieri*, etc., have mandibles very much like *Odontomachus*. But this would be absurd, hence it is best to let well enough alone. Moreover, the shape of the mandibles in different genera of the Ponerinæ (e. g., in *Harpegnathus*, *Thaumatomyrmex*, *Mysrium*, etc.) is so diverse that this subfamily would have to be resolved into a great number of

know how I could have given this impression, as the only time I associated these genera directly was when I considered *Odontomachus* to be more dominant, more variable and more widely distributed than other Ponerinae, *Ectatomma*, of course, included. This statement has not even been met, to say nothing of having been refuted, by Dr. Cook.*

It is, in fact, Dr. Cook himself who should be glad to have the kelep more like *Odontomachus*. At any rate, he makes a futile attempt to show that the kelep is a dominant, 'enterprising' ant, with large colonies (*i. e.*, prolific) and highly adaptable. But closer examination shows that the kelep is like the other Ponerinae in being below par in all of these respects. It is 'dominant' only in the cotton fields of Guatemala, and very rare or absent elsewhere in that country. It is 'enterprising' although 'compared with the nervous haste of many other species, its motions are slow and deliberate (*sic!*), and, like the so-called praying mantis, it stands for long periods quite motionless, with the antennae and mandibles extended, ready for something to come that way and be caught.' This must be 'enterprise' as understood by the Jewish tailor of the comic papers who stands in the doorway of his shop waiting for customers.

It seems that I was mistaken in supposing that the colonies of the kelep contain only

families, if we were to follow Cook's example with *Odontomachus*. Undoubtedly this would give a fine opportunity for a display of the *mihi* itch, but the cause of science would be little furthered thereby.

* The larva of *Ectatomma* is much more primitive in its characters than that of many other Ponerinae, whereas the larva of *Odontomachus* is much like that of the typical genus *Ponera*. This fact, too, has an important bearing on the taxonomic position of *Odontomachus* discussed in the preceding foot-note.

from 20 to 110 workers, though these numbers were taken from Dr. Cook's own statement concerning the colonies introduced into Texas. We are now told that they (not the colonies in Texas!) comprise between 200 and 300 individuals and that 'there are seldom less than 100 and sometimes 400 or more.' Now even if we put the number at 500, these are still very small colonies, as ant colonies go, and show conclusively that the kelep, like other *Ponerinae*, must be either short-lived or much less prolific than other ants, or both.

The adaptability of the kelep, according to Dr. Cook, is 'shown by its association with the cotton for the sake of its nectar, as well as by its skill in stinging the boll weevil.' If this shows anything it does not show adaptability but adaptation, which is a very different matter. The first part of Dr. Cook's statement, together with several of his previous statements, implies that the cotton plant and the kelep live in a state of symbiosis, like that which has been claimed to exist between the South American *Cecropia* tree and the ant *Azteca instabilis*, and between the African and tropical American acacias and the species of *Sima* and *Pseudomyrma* respectively. These classical cases, however, have never been demonstrated to the satisfaction of either the botanists or the myrmecologists. Any one who observes without bias the insects visiting many plants with extra-floral nectaries, like our species of *Cassia*, *Ricinus*, *Stillingia*, *Populus*, etc., will find that certainly in such cases no symbiosis exists. Not only do all sorts of ants, mutillids, bees, wasps, beetles, flies, etc., visit the extra-floral nectaries, but caterpillars, chrysomelid larvæ, etc., may be found feeding with impunity on the lacerated foliage of the plants thus 'protected.' It is possible, of course, that some of the cases of so-called ant and plant sym-

biosis may be genuine, but before any such statement can be made of a particular case like the cotton plant, we need much more concise, abundant and painstaking observations than have been published hitherto.

I fail to see, therefore, that Dr. Cook has produced any facts that could lead me to 'mitigate' the statements made in my former paper. The kelep is a typical ponerine ant, with all the disadvantages of a fixed and archaic constitution in the presence of experiments that require for their successful execution a plastic and adaptable species. When the kelep has succeeded in becoming a thriving component of the Texan ant fauna there will be time enough to determine whether its strenuous and enterprising efforts can 'add even ten per cent. to the cotton crop'—we will not expect it to chase all the boll weevils into the Gulf of Mexico. Dr. Cook himself admits that 'the chances are still very much against it, no doubt.' This is exactly what I have maintained. Neither I nor any one else blames the Department of Agriculture for following every clue till some 'concrete conclusion' is reached, but the premature and persistent booming of a conclusion which is far from being 'concrete' and has 'chances very much against it' can only discredit the Department of Agriculture, Dr. Cook and the unsuspecting kelep in the eyes of the general public, the Texan cotton grower and the scientist. I shall have no further remarks to make on the kelep and am satisfied to await patiently the concretion of the conclusions—even till the Greek calends.

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