enter into it with the zest and interest worthy of the professions represented.

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DISCUSSION AND CORRESPONDENCE.

THE MEXICAN COTTON BOLL WEEVIL.

TO THE EDITOR OF SCIENCE: In your issue of November 13 (p. 640) you quote from *Bradstreet's* an item regarding the loss to the cotton crop of Texas through the ravages of the Mexican cotton boll weevil. In the course of the article *Bradstreet's* states that six months ago it advocated a careful consideration of the subject by congress. From this quotation alone *Bradstreet's* seems to be singularly misinformed as to what actually has been done by the government, and the quotation will, therefore, mislead your readers.

In 1894 an investigation of this insect was begun by the Division of Entomology of the U. S. Department of Agriculture, and in 1896 and 1897 circulars were published which indicated the great danger to the future of cotton in the United States and proposed remedial treatment. The governor of the state and the legislature were advised by the department of the condition of affairs and the dangerous prospects, and the legislature was urged to pass a crop pest law, the enforcement of which would have resulted in the confinement of the insect to a restricted region in extreme southern Texas, and possibly in its extermination even in that region. The legislation proposed was not enacted. For the past three years the Division of Entomology has been carrying on further investigations through appropriations from congress of $10,000 in the fiscal year 1901–2, $20,000 in 1902–3, and $30,000 in 1903–4. It has resulted from this work that, while no method of extermination has been discovered, it has been demonstrated beyond a doubt that it is possible, even under present conditions and in the worst infested portions of Texas, to raise a fair crop of cotton in spite of the weevil. Experimental demonstrations have been made the past summer on several hundred acres of cotton lands at six stations under the control of the Division of Entomology, and on this controlled land from a half of a bale to one bale per acre of cotton has been already harvested, while in adjoining territory the average crop has not exceeded one bale to from six to fifteen acres.

L. O. HOWARD.

SHORTER ARTICLES.

SOME INSECT REFLEXES.

IN the course of some experiments on the sense-reactions of honey-bees, I have kept a small community of Italian bees in a glass-sided, narrow, high observation hive, so made that any particular bee, marked, which it is desired to observe constantly, can not escape this observation. The hive contains but two frames, one above the other, and is made wholly of glass, except for the wooden frame. It is kept covered, except during observation periods, by a black cloth jacket. The bees live contentedly and normally in this small hive, needing only occasional feeding at times when so many cells are given up for brood that there are not enough left for sufficient stored food supplies. Last spring at the normal swarming time, while standing near the jacketed hive, I heard the excited hum of a beginning swarm and noted the first issuers rushing pellmell from the entrance. Interested to see the behavior of the community in the hive during such an ecstatic condition as that of swarming, I lifted the cloth jacket, when the excited mass of bees which was pushing frantically down to the small exit in the lower corner of the hive turned with one accord about face and rushed directly upward away from the opening toward and to the top of the hive. Here the bees jammed, struggling violently. I slipped the jacket partly on; the ones covered turned down; the ones below stood undecided; I dropped the jacket completely; the mass began issuing from the exit again; I pulled off the jacket, and again the whole community of excited bees flowed—that is the word for it, so perfectly aligned and so evenly moving were all the individuals of the bee current—up to the closed top of the hive. Leaving the jacket off permanently, I prevented the issuing of the swarm until the ecstasy was passed and the usual quietly busy life of the hive was resumed. About three hours later there was a similar performance and failure to issue from the quickly unjacketed hive. On the next day another attempt to swarm was made, and after nearly an hour of struggling and moving up and down, depending on my manipulation of the black jacket, most of the bees got out of the hive's opening and the swarming came off on a weed bunch near the laboratory. That the issuance from the hive at swarming time depends upon a sudden extra-development of positive heliotropism seems obvious. The ecstasy comes and the bees crowd for the one spot of light in the normal hive, namely, the entrance opening. But when the covering jacket is lifted and the light comes strongly in from above—my hive was under a skylight—they rush toward the top, that is, toward the light. Jacket on and light shut off from above, down they rush; jacket off and light stronger from above than below and they respond like iron filings in front of an electromagnet which has its current suddenly turned on. What produces the sudden strong heliotropism just as the swarming ecstasy comes on? That is beyond my observation.

Dr. Loeb tells me that he has observed and recorded a strong positive heliotropism in the winged male and female ants at mating time. They rush from their underground nest and take wing directly toward the strongest light. With both bees and ants this flying toward the light at swarming and mating time, respectively, has obvious advantages. It keeps the swarm together and it takes them away from the old community. Swarming and mating flights are distribution and migration of the species to new ground where are food and space unneeded by the old community.

During the last three years I have, with a student, Mrs. Bell, been rearing silkworms under quantitatively determined varied conditions of food supply, in an attempt to determine, also quantitatively, the extent and character of the induced variations. In these experiments silkworms separated into various lots (the individuals in each lot kept also apart so that each may get its rightful share of food) are variously fed through their life on an optimum of food, on one half optimum and on a minimum amount, that is, one which will just keep the worms alive, developing and growing. As the optimum amount is that which permits the larvæ to feed almost continuously, and as each under-fed worm eats up as rapidly as possible its full supply, it is evident that the half-fed and minimum-fed worms have to spend long hours of non-feeding, rest and quiet and—if it may be—meditation. Now such a period of non-feeding and inactivity is precisely a normal pre-molting phenomenon of all silkworms—and of other lepidopterous larvæ, too, for that matter. With this in mind it is interesting to note that a common phenomenon in the life of the under-fed worms was an abnormal increase in moltings; that instead of adhering to the time-honored and Bombyx-approved habit of molting four times (exclusive of pupation) during the larval life, most of these half-starved larvæ, with artificially imposed repeated periods of non-feeding, molted five and a few even six times. It may be expected that such a sapping of vitality as these starvation rations must have produced would result in a lessening of the physiological activity, and a giving up of one or more moltings rather than an increase in the number of these betrayals of growing pains. But no, there are more rather than fewer moltings. Now in actual molting, the loosening from the body of the old cuticle results from the secretion by skin glands of a so-called molting fluid; this secretion occurs during the non-feeding resting stage normally immediately preceding the molting. Can the abnormally induced non-feeding, inactive periods imposed on the under-fed worms have been the sufficient stimulus for setting up this secretion and accumulation of the molting fluid resulting, as usual, in a loosening of the cuticle? That is, do lepidopterous larvæ molt not just because molting is needed at some particular time but because the cuticle gets pushed off by a fluid which gets formed and excreted whenever the larva stops feeding? Of course the insect that wisely times its non-feeding rests so that its moltings shall come to best advantage for the necessities of growth is the insect that is represented by descendants now-a-days. But is molting any the less a reflex for that?

Loeb, in his 'Comparative Physiology of the Brain,' records certain observations on the larvæ of the moth *Porthetria* sp. with regard to their regular movements up the stem of the food plant, suggesting that this climbing up is a positively heliotropic reflex, resulting in the (advantageous) finding of the tender new leaves and buds of the food plant. I have observed the traveling behavior of the larvæ of three species of moths, these larvæ being the mulberry silkworm, a geometer found on lindens and an unknown species (the adult was not bred) which may be called No. 3. In each case only larvæ just hatched from the egg were used, thus eliminating any results of experience or imitation. Twigs of food plants were so arranged that there were for each kind of larva cases in which (*a*) the leaves were up and in the light, (*b*) the leaves were up and in the dark, (*c*) the leaves were down and in the light, (*d*) the leaves were down and in the dark, (*e*) the leaves were in horizontal plane with the twig and in the light, (*f*) the leaves were in horizontal plane with the twig and in the dark, (*g*) the leaves and twig were in horizontal plane all equally illuminated, (*h*) the larvæ were put on a part of the twig which was in the dark, (*i*) the larvæ were put on a part of the twig which was in the light. These various cases were easily arranged for by having a number of hollow cylinders about one and a half feet long, three inches in diameter, and open at both ends. The twig with leaves could be put into or partly into the cylinder, as desired, and the cylinder put in vertical or horizontal position, as wished.

Without taking space to give in actual detail the behavior of the tiny larvæ of the three different species, I may summarize this behavior as follows: The silkworms moved indifferently toward light or away from it, up or down, until they found food, and then made an end of traveling; the linden inch-worms tended strongly to travel toward darkness and when this direction was downward the tendency was greatly strengthened; the unknown No. 3 larvæ tended obviously to travel toward the light, and when this direction was upward the tendency was strongly increased. That is, in these three lepidopterous species, the larvæ of all being leaf feeders and naturally preferring tender new leaves, three different conditions of reaction to light were shown, in one a positive heliotropism, in one a negative heliotropism and in the third sheer indifference to light. This last species, the silkworm, may well be looked on as having lost its earlier sensitiveness to light through panmixia—if there be panmixia—because for generations the silkworm's food has come to him rather than had to be gone to, and there is no more nor better food up or toward light than there is down or away from light or sidewise and in light of the same intensity as that in which he first finds himself. I have reared silkworms in darkness unbroken during their whole life except at moments when the mulberry leaves were thrust into the dark cell, and in no structural or physiological characteristic was there any apparent difference from individuals bred in bright sunlight (alternating with unillumined nights).

It is interesting to note the decided character of the negative heliotropism of the linden inch-worms, as this is the kind of heliotropism which is distinctly unexpected of larvæ which have to find for themselves tender fresh leaves. In a glass cylinder, lighted in its upper half and darkened in its lower half, with linden leaves placed at the very bottom, 92 just hatched larvæ were placed in the lighted half at 11:20 A.M. At 3:30 P.M. 17 larvæ were still in the light half, but 75 were in the lower darkened half, most of them being in the darkest place, that is, at the very bottom *under* the leaves.

Equally marked was the more familiar positive heliotropism of the unknown No. 3. When only two or three inches from leaves put into the darkened half of a glass dish, the whole group of tiny caterpillars, certainly hungry even to death, would keep steadfastly in the light half.

The behavior of these various kinds of

caterpillars, while contradictory if we were inclined to form too hastily a generalization based on the behavior of the *Porthetria* larvæ —a generalization that would explain the going up to light as a reflex which had persisted among all leaf-feeding lepidopterous larvæ because of its advantageous leading of them to the most succulent food—is not at all contradictory of the point of view of the biologist who believes in reflexes. Personally, while still inclined to see more wit in ants than Bethe's extreme confidence in the reflex theory of their behavior would admit, and while recognizing the reasonableness and legitimacy of the query, does the reflex basis of behavior really simplify our conception of the springs of animal behavior?—I am willing, on the evidence of the accumulating observations, to see much of the credit which insects have long enjoyed for the possession of unusual intellectuality and elaborately developed instinct, go by the board. Immediate physicochemical stimuli undoubtedly produce as direct reflexive reactions many of the activities which we have been long interpreting on a basis of complex instincts and associative memory.

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NOTES ON THE VEGETATION OF THE TRANSVAAL.

Before coming to the Transvaal I was informed by a botanist who had some knowledge of the South African flora that the flora of the Transvaal was entirely xerophytic in character, and that it was largely composed of succulent plants—*Euphorbias, Aloes, Mesembryanthemums, Cotyledons, Crassulas* and the like. In my informant's mind it was apparently a continuation of the flora of the Great Karroo.

Imagine my astonishment, therefore, after crossing the Karroo, with its dreary plains so like those of the Great Basin of North America, even in the general aspect and color of its vegetation, to find myself, on waking up one morning, crossing a vast, grassy plateau, the high veldt, practically destitute of trees or shrubs, but producing masses of tall, thick grass, recalling the prairies of the far west. Later I found that this was a fair sample of the vast stretch of country extending from the confines of the Kalahari desert in the west to the summits of the Drakensberg in the east.

Grasses form the most conspicuous features of the Transvaal flora. This is true, at least, of the high veldt. They are only little less abundant in those parts of the bush veldt which I have seen.

Succulents are rare, being practically confined to rocky kopjes, *i. e.,* buttes, and the randjes, *i. e.,* ridges, which cross the country from east to west. Mesembryanthemums are extremely scarce. Bulb and corm-producing plants abound among the grass. A few bushes and small trees, evergreen *Proteas,* caulescent *Aloes* and *Cereus*-like *Euphorbias,* deciduous *Combretums,* etc., also occur on the kopjes and randjes. Patches of diminutive woodland, composed of Doorn-boom (*Acacia horrida*) five to fifteen feet high, are occasionally seen at long intervals in crossing the high veldt, usually in the vicinity of water.

As a rule, however, trees and shrubs are entirely absent. I have driven all day, a distance of sixty miles, without seeing more than one colony of bushes, that composed of about twenty-five individuals in all, and they not more than eighteen inches in height. In the moist vleis, on the other hand, some of the grasses—species of *Andropogon*—would be eight and even ten feet in height. In the absence of woody plants Kaffir and Boer alike fall back on 'mist,' *i. e.,* dried ox-dung, for fuel.

Grasses being, as I have said, the most conspicuous feature of the high veldt flora, one is naturally desirous of knowing what grasses occur, and in particular, which are the most abundant.

Although but little work has been done on the flora of the Transvaal, as compared with that done on the flora of California, for instance, a few good collections have been made, particularly by Wilms Rehmann, Nelson, Galpin and Rand, and sufficient grass material has been gathered by the three first named collectors to give a general idea of the occurrence of the genera. Much still remains