over the relations of one discipline to another. But this ought not to hinder any science from setting its own limits and doing its own work, while it accepts all the aid it can get from others. No science is more widely indebted than is psychology; but psychology demands, no less than others—such is scientific selfishness!—that she be allowed to work out her own destiny in her own way.

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SHORTER ARTICLES.

THE SALT MARSH MOSQUITO, CULEX SOLLICITANS WLK.

IN SCIENCE for January 3, 1902, p. 13, under the caption 'Concerning Certain Mosquitoes' I pointed out that *Culex sollicitans* was the dominant form throughout a large portion of the State of New Jersey. Upon our ability to control this species depended the riddance of the State to any notable extent, and the life cycle of the species became, therefore, a matter of the greatest importance. I suspected even at that time that this species departed materially from the stock history given for *Culex* and assumed for this species; but my observations had been sufficient only to suggest the need of closer study. I showed at that time that, by breeding in salt water and by migrating for long distances the species had distinctive characters. And, bye-the-bye, there is no more perniciously erroneous popular statement than that mosquitoes do not fly far from the place they were bred. It is absolutely untrue of most of the species and not entirely true of any. The only case where it is practically true is where a species is limited in its breeding places, *e. g.*, the species that breeds only in the leaves of the pitcher plant. Of the salt marsh mosquito it is conspicuously incorrect.

In February and March I started a hunt for the adults on the supposition that the female hibernated. My assistant, Mr. Dickerson, searched every nook and cranny that might shelter a mosquito in a seashore locality where, during the summer, the insects had driven out all guests. *Culex pungens* and *Anopheles* were found in numbers; but of *sollicitans* not one! I had no better luck when I took up the search myself, and even a reward offered to the natives for every specimen brought to me, failed to produce returns. I concluded, therefore, that the insects did not winter in the adult stage and began a hunt for larvæ. I knew that *Aedes smithii* wintered in the larval stage and that the wigglers would stand repeated freezings. But I failed also to find larvæ in the very regions where they were abundant in 1901, and where I had also seen them in 1900.

A wintering in the egg stage was unknown for *Culex*, but I was driven to that alternative and watched carefully for 'signs.' They came as the water warmed up. First, larvæ were found in pools high up which had been filled by the winter tides. The temperature of the water was distinctly higher than that of the air in the morning and evening and several degrees higher than that of sea water. Area after area became populated and there were millions of larvæ, growing very slowly, before a solitary mosquito was seen. A hibernation in the egg stage seemed obvious; but I ran against the fact that some of the areas swarming with larvæ were dry during the summer and fall of 1901 and became water-filled only during the winter storms. If the eggs hibernated on that ground they must have been laid on dry soil or on the grasses! This then was the point to which I had arrived at the opening of the breeding season. College duties and other matters prevented a resumption of the work until July 7, when Mr. Dickerson and I spent a week at Five Mile beach; I kept him in the field another week alone and rejoined him when the experiments were expected to produce results. Our outfit consisted of a series of seven tubs sunk into the marsh so as to project only a little above the level. In five of them was placed sod from the marsh and two were left bare. Sea water was placed in all save one of the tubs in varying quantities. Two tubs were left open—one with sod, one without; the others were covered with mosquito netting. Conditions along shore at this time were very dry and breeding places were fast disappearing. I secured, however, in some remaining puddles a large lot of larvæ and pupæ and placed these, in jars, under the covered tubs. Adult mosquitoes were present in great numbers and, with pools so scarce, it was supposed that the uncovered tubs would prove very attractive.

A series of glass jars was prepared with and without water, with and without grass, and captured females were introduced. In almost every case eggs were found after twelve hours; but as they were laid under all sorts of conditions, and black mature, as well as white immature, specimens occurred, it was obvious that this was only the natural tendency of a confined female to oviposit at all hazards. But it was of real advantage in that it gave us the egg so that it might be identified under natural conditions. In color it is polished black, pointed at both ends, almost perfect spindle-shaped from one point of view, a little curved or pod-shaped from another, half turned over. In length it is less than one millimeter.

While all these preparations were made by Mr. Dickerson, I explored the marshes round about and found, to my surprise, that this immense area which had been supposed to be the very breeding stronghold of this species was as a matter of fact perfectly safe. There were no mosquitoes at all on these great marshes and no larvæ were in any of the 'salt ponds' formed by natural or artificial methods; not until the edge of the upland was reached did I find either mosquitoes or larvæ. It goes without saying that if this immense area of salt meadow can be practically ignored in mosquito extermination plans, we have made a very long step toward the simplification of the problem.

Without going into details here it may be said that the evidence from the tub experiments was negative. Under none of the conditions artificially presented to them did the insects lay eggs. Yet egg-laying females were present in abundance and some were sent to New Brunswick by Mr. Dickerson about July 15.

From the dissection of these specimens I obtained the following record:

No. 1. Black eggs 46, gray eggs 17, white eggs 101; total 164.

No. 2. White eggs 117, and a little undigested blood in crop.

No. 3. Black and gray eggs only, 148.

No. 4. White eggs only, 135.

No. 5. Black and gray eggs 47, white eggs 35; total 82. It is probable that this last specimen had been ovipositing.

July 20, when the tub experiments were closed, all the evidence pointed to an oviposition on the sod, or in the dry bottom of old breeding pools. Material from dried up pools, old and recent, was obtained and examined carefully in basins. A few eggs were found almost everywhere, but not enough to make it at all certain that these were normal points for oviposition. Finally in our examinations we reached the sods of long marsh grass, forming the upper edge of a pool that was then and for a time had been entirely dry.

This sod was simply a mass of interlaced roots and on the surface was a layer of soft, black mud. In this mud I saw undoubted mosquito eggs in such numbers that I washed the surface into a basin and left the material until next morning. Then the basin was swarming with larvæ and the eggs must have been at the rate of from 50 to 150 on one square inch of sod.

After determining the place of oviposition the next questions were under what circumstances do the insects hatch and how long may they remain dry during the summer.

Two sods approximating six inches square were cut from the marsh and carried to New Brunswick, July 21. One sod was placed in a deep glass dish in the bottom of which was half an inch of water. The sod was two inches deep, so the surface was well above the water, which was renewed from time to time to keep it at about the same level. The other sod was put into a porcelain evaporating dish and left dry. It fitted loosely and air could get on all sides and under it. Absolutely no moisture was added at any time.

July 31, ten days after the sod was taken, a small piece—about two square inches—was cut off late in the afternoon and the surface mud was washed into a dish. Next morning 117 larvæ were counted and some of these were carried to maturity, so that the complete life cycle from egg to adult was under observation.

August 10, duplicated this experiment but began about 8 A.M., to determine the time of the appearance of the first larvæ. Before 10 A.M. the dish was swarming with wigglers; all the eggs had hatched in less than two hours.

On the same day I cut off a small section of the sod that had been kept continuously moist and washed this into a dish. Twenty-four hours later no larvæ had developed and I submitted the mud to close examination. Numerous eggs and egg fragments were found, making it certain that the absence of larvæ was not the natural result of the absence of eggs.

August 11, this last experiment was duplicated with the same result. The sod had been so wet as to induce development and perhaps hatching while there was no water to support the larva. The latter suggestion is due to the large number of broken eggs that were found.

August 12, Mr. Dickerson washed the mud from a small square of dry sod into a large dish and began transferring the eggs into a watch glass. It was a slow job because the black eggs are not readily differentiated from the black mud, and in about half an hour he began to find broken eggs. Transferring the watch glass from a white to a black backing he saw several pure white, minute wigglers, just out of the egg. Half an hour's submergence, then, was enough to start out the larvæ, and soon afterward it became impossible to find unbroken eggs. The piece of washed sod was placed in another dish and, an hour later, more larvæ were obtained. When the larva is ready to emerge, about one fourth of the egg lifts or breaks off, giving it exit. The small end may remain attached to the larger for a time by a sort of hinge; but it is detached by the least shaking of the water.

We have then, briefly, the following life history. The adult lays eggs, singly, in the mud of salt meadows above ordinary high tide and where the sod is not soaking wet. It probably lays them elsewhere as well, but I am stating the rule, as I believe. The eggs remain here an unknown length of time until an extra tide or a heavy rain covers them with water. The eggs hatch almost immediately and the larvæ find their food in the soft, decomposing mud. It makes little difference whether the water is fresh or salt, so long as the proper food is present. The stay in the larval and pupal condition varies according to temperature, but is not less than a week. The males emerge first and rarely, if ever, leave the immediate vicinity of the place of hatching. It is probable that copulation takes places soon after the female emerges, but I have made no direct observations, and have been unable to secure a pairing in captivity. It is also probable that the females do not fly to any distance until they have been impregnated and it is certain that there is no development of the eggs until the insect has fed. A long series of specimens collected as they came to the attack and afterward dissected, all showed an empty alimentary canal and undeveloped ovaries. Another series, collected by sweeping in the natural breeding places showed that wherever the ovaries were developing the alimentary canal showed food remnants, greater or less, according as the eggs were undersized or approaching maturity. When the eggs were fully developed the food remnants disappeared. So far as determined the food was blood in all cases; but of what kind was not made out. As the collections were made near a settlement, it might have been of horse, cow, dog or man.

I have already referred to the specimens sent on July 15. On the 20th I collected a lot of about 100 examples by sweeping, and examined for ova. Curiously enough, none of the examples had fully developed eggs. They ran all the way to full size, beginning to turn translucent as the shell became differentiated; but not a black egg was found. The number of eggs varied greatly, but rarely reached 200 and rarely fell below 125.

Not all the points of interest in the life cycle of the insect are covered, but enough is now known to understand the economic problem. We know that, so far as this species is concerned, all permanent water areas, deep or shallow, are safe. So are temporary pools in very wet salt meadows. All meadows covered by ordinary tides are safe, and so are all those low enough to be attractive to fiddler crabs.

Areas covered by the monthly high tides are safe, except in midsummer if it has been dry enough to kill out the young fish and has then rained enough to fill the low places. The danger points are such as I pointed out in SCIENCE and more at length, recently, in Special Bulletin T of the New Jersey Agricultural College Experiment Station.

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August 20, 1902.

'LATENT HEAT' AND THE VAPOR-ENGINE CYCLE.

THE discussion, some time since published in SCIENCE, relating to the vapor-engines, so-called, and the 'latent heat fallacy' led to inquiries from various sources regarding the exact distribution of the work of thermodynamic transformation in the case of the steam, and other vapor-engines. The following may perhaps make clearer the relation between the action of sensible and of 'latent' heat in such cycles. The discussion of this problem has been one of the annual topics in the classes of the writer for years past.

The usual standard form of engine-cycle, in all departments of applied thermodynamics and with the steam, and other vapor-engines employed in the industries, is that known as the Rankine cycle with incomplete expansion, as in the figure. It consists of a line of constant maximum pressure, an adiabatic expansion-line, as nearly as practicable, a line of constant volume, a line of constant minimum pressure, and the cycle is closed by a line of constant volume. Assuming unit-weight of the working substance to be carried through such a cycle, it is easy, by the adoption of one of Rankine's beautifully ingenious mathematical devices, to obtain the following expression for work of one cycle in which p, T and u are the pressures, the temperatures, absolute, and the specific volume of the charge; H is the latent heat of vaporization and J is Joule's factor. The subscripts indicate, respectively, values of p and T on the expansion line and of p on the back-pressure line:

$$ABCDE = AFG + ABCFA + CDEG$$
$$= (\text{I.}) + (\text{II.}) + (\text{III.})$$

$$U = J[T_1 - T_2(1 + \log_e T_1/T_2)] + H_1(T_1 - T_2)/T_1 + (p_2 - p_3)u_2.$$

The three parts into which the measure of net work, U, divides itself are obviously a function of temperature which measures the effect of the thermodynamic application of sensible heat, a function of temperature and 'latent' heat which is instantly recognized as the measure of the Carnot efficiency of the 'perfect engine,' and a function of the terminal and back pressures and specific volume of the charge at the minimum temperature of the expansion-line. This latter is obviously, also, the work between the terminal and back-pressures, the rectangle, $CDEG$. The intermediate term is the work obtainable from the same quantity of fluid between the same two temperatures, T_1 and T_2, in the Carnot cycle, $ABCFA$, and it is thus evident that the first term must measure the remaining area of the Rankine cycle, the triangle, AFG; which is as evidently the work alike of the compression in the Carnot cycle and that of expansion of unit weight of a mixture of steam and its liquid between the state of liquid at maximum temperature at A and that of mixed vapor and liquid at the lower limit of expansion pressure and temperature, p_2 and T_2.

Noting the proportions of the areas thus measured, it is seen that, with any fixed value of the latent heat of vaporization, the last-named quantity has a lower relative measure as the ratio of expansion and the temperature-range decrease, and, *vice versa*, that the quantity of work performed within the same temperature-range is in all cases greater in the Rankine than in the perfect engine cycle by this amount; that the work in either cycle is proportional, in some direct measure, to the quantity of the heat of vaporization; that the heat entering the fluid during vaporization is *all* converted into work and that none is employed to change temperature and thus to become stored as sensible heat. Observing, also, that the Carnot cycle is that of maximum efficiency, it follows that the work measured by the first term, and by AFG, is obtained at a comparative loss of efficiency and that, therefore, the work gained in the Rankine cycle,