

outfit in the laboratory, and we can use it or not, and we don't have several hundred dollars laying idle in special apparatus, which is, perhaps, used only three or four days in the year.

CHAIRMAN BRITTON: If there is no further discussion, we will proceed with the next paper on the program, by Mr. F. M. Webster.

THE IMPORTANCE OF PROPER METHOD IN ENTOMOLOGICAL INVESTIGATIONS

By F. M. WEBSTER, *Washington, D. C.*

It is not my purpose to lay down a series of rules and regulations governing details in the work of any one; therefore the use of the word "method" instead of "technique." As a matter of fact, variety in insect life, and the conditions under which these must be studied, is so great that, beyond general principles, each investigator is forced to accept situations as he finds them and to make the utmost of his opportunities.

The spirit that prompted this paper came from the fact that the older entomologists of the country have, almost all of them, passed away, and those of us who, a quarter of a century ago, were young men are now also passing away, and a decade hence we shall nearly or quite all of us have practically given place to the younger men who now constitute by far the majority of the membership of this association. I thought that it might be possible for me to tell you of some of the things learned during nearly forty years' study of insects and insect problems, beginning at a time when entomologists were few, with no such training as is offered today available, and with precious few publications dealing with insect binomics. Strenuous days those, but in later years I have come to look back upon them with greater leniency and indeed feel thankful for them. Unfortunate indeed is the man who is denied the opportunity to show what his abilities really are, something that even he may not himself have learned.

With all of us who have the management of men there comes a time when an emergency arises and some one must be detailed to a most difficult piece of investigation, where only the most resourceful, persevering and trustworthy are to be employed, and lucky is the man who gets the opportunity. Now if I were to be asked to indicate some of the most essential qualifications for such a man I should say, first, honesty, and, second, common sense. Without these all of the training and equipment in Christendom will avail nothing. Some of us do not see as clearly or as broadly as others, it is true, which is of

course a personal misfortune, but not to be able to translate the truth, exactly as we see it, is a fatal defect. If there is a profession, the ministry not excepted, where clean men are more essential than in scientific research, it would be difficult to name it. Entomologists are not angels, and are not likely to become so in the future, in this world at any rate, but the man who goes after the truth for truth's sake is industrious, ingenious and persevering, will be about as sure of success as one day is sure to follow another. Incidentally, however, success does not consist of getting your name in the papers with the greatest frequency; the merest charlatan does that; nor does it mean exploiting yourselves before a few farmers who are seldom posted, for the untrustworthy have a monopoly in that direction. The man who beats the bass drum makes the most noise, but he is not the leader of the band. There is all the difference in the world between becoming famous and becoming notorious. You may secure fame or notoriety, but never both. A really good entomologist has no need of a placard advertising the fact, because in the process of his development he has been obliged to exhibit qualifications and combinations of qualifications that are never to be found in an inefficient man. No single qualification alone leads one to success, but one must needs be well balanced; be sufficiently pessimistic to be able to justly but fearlessly deal with defects in the work of others as well as his own, and yet be optimistic enough to see in advance sufficiently clear to formulate working theories and hypotheses without prematurely adopting these as fixed truths. Deficiency here is the reason for so many failures, and, often, too, failures that seem almost like the frivolities of chance. But it is not so. There is some defect, something lacking that is essential to success. Naturally the unfortunate himself will say it is due to a lack of opportunity or proper appreciation. Not every one is capable of grasping an opportunity when offered him, and as to appreciation, the world is not such an unjust judge after all, though it is sometimes very tardy in rendering a decision, and while the lines

"Seven cities fought for Homer dead,
While the living Homer begged for bread,"

finds a parallel perhaps all too frequently, yet such cases are, at most, not usual, and real success is, after all, in our leaving of the world better than we found it. Besides, it is often the fear of not being appreciated that drives men to make the mistake of chasing about after notoriety and in consequence accomplishing nothing meriting success. All of this leads me to make what may seem to you to be a remarkable

suggestion, and that is: Do not try to become famous or even noted except as a conscientious man. Get close to nature and throw the whole weight of every faculty that you possess into learning the truth unmixed with error, and get as near the whole truth as it is possible for one human to secure. Your province is to get facts sifted and tried by every clarifying process you can devise, with a view of eliminating obscurity and error. Pay absolutely no attention whatever to your own individual prominence, and as fast as you secure the real truths involved, just so fast will you gain the reputation of being something far above notorious. Success will not only come to you but be even begging you to take notice thereof. The old threadbare saying that "Some men are born great; others have greatness thrust upon them" is not true. Real greatness is never inherited; and the man who has real greatness thrust upon him has always bought it in some manner with the best years of his life. Substitute notoriety for greatness and I have nothing to say. The man in search of notoriety is generally the one that succeeds in getting in the way. In fact such is his usual mode of procedure in making himself notorious. For this reason the man who is trying with all his might and main to wrest from nature her most profound truths is obstructed by the one who, not knowing what truth and accuracy really are, will place himself squarely in opposition. Thus it is that the man who sets out to devote himself to dragging forth truth out of darkness or obscurity will find that his is not a bed of roses. Not only must he hold his own faculties under a continued surveillance lest he be cheated by his own eyes and mind, but he must always be more or less hampered by the frailties of those who find it easier to adopt other less commendable methods. Within the last two months there have come to my desk two publications from as many different states relating to two different though closely allied insects. In neither one is there anything new, nothing to indicate that either insect has ever been known outside the state where it is mentioned, and not a word to show that anyone, living or dead, except the author, had ever seen or studied the species. Quite recently one of the institutions from which one of these documents emanated published a newspaper bulletin, not necessarily by, or even with the knowledge of an entomologist, giving what purports to be the results of several years' experimentation, proving certain facts that were well known and established before that institution came into existence. Any well informed entomologist could readily cite the work of investigators, some of whom are still living and some dead, who contributed to the sum of this knowledge; but in this printed document to which I refer every

word is stamped with the mark of originality. Now such things as these have no relation to either scientific research or the diffusion of truth. They are, purely and simply, bids for the cheapest sort of notoriety, and will stand, if indeed they stand at all, in future as monuments of condemnation for whoever has been instrumental in their creation. The probabilities are, however, that they will be cast aside and disappear as of no value whatever and be eliminated precisely as any other impediment. After all, this whole matter of the separation of the truth from error and falsehood may be likened to a huge system of sifting screens, each screen perhaps representing a decade of time. All of the work of human intellect is thrown into a hopper and the mass begins to be shaken downward. Gradually mistakes, misstatements, jealousies, and all of the results of human frailties, the dross if you please, is discarded and finally at the bottom we have the net results of scientific research. Your success in life as investigators will depend not so much on the bulk that is thrown into the hopper and the noise and dust arising therefrom as upon how much of this pure fact at the bottom of the screens you can yourself lay claim to. I will show you in a moment how the identification may be made. This matter of proper credit as between superior and subordinate has several times been discussed by this body, always, as it seemed to me, with a too narrow view of the subject, making it something of a personal matter. I do not wish to discuss it in this way, as the question does not seem to me to be definitely understood.

Perhaps I can best approach the matter by offering an illustration. You may become estranged from your child; you may disinherit him, disown him and refuse to recognize him in every legal way. He may even be adopted by another. But still the blood of his father and mother alone will flow in his veins. Their ancestry will be his ancestry, and no law in the universe can make it otherwise. Now every original, unprejudiced observation made is the child of one mind and one pair of eyes, and these alone can be held responsible. There is here a certain entirely natural proprietorship that cannot be either stolen, given away or disowned, any more than a child can be disassociated from its ancestry. Whoever attempts to destroy this responsibility commits a crime, not against individuals, but against science itself, and no crime against science, which is only another term for truth, goes ultimately unpunished. Not only is this true with the original observation itself but all others following thereafter, whether contradictory or confirmatory.

When we come to the subject of publication, we are dealing with

the results of observation and investigation. The custom is, I believe, almost universal in all countries, which gives to the employing institution, for purposes of publication, all of the results of the labors of those in the employ of such institution. This seems entirely just, and there can hardly be serious criticism, so long as the observer is held responsible, given proper credit, and is not obliged to state what he does not believe to be true. There will probably always be occasional disregard of the rights of the observer, but these are not common and becoming less and less so every year. As the injury is not to individuals but to science itself, there is a certain losing of caste among those who do these things, which tends to prevent their occurrence. The question as to just when results should be published is a somewhat complicated one, and I sometimes wonder if the opinion of the investigator, even though a good one, is always the better. If we were to gather together all of the best of American investigators, I doubt if there would be one among them who would not recall instances where he had published prematurely and regretted it afterwards. To those who are in quest of notoriety, it of course does not matter. In my own experience, after working with a problem until there seemed no possibility of serious error, the results were printed and almost before the printers' ink was dry there would be sudden and unexpected developments that would completely upset previous conclusions. Indeed, it is strange how frequently such things will happen, even with what seems proper caution and the best of intentions. The older I get the more it seems to me that just here the investigator should be able to hold himself well in hand. Many a basis for a good piece of work has been spoiled by rushing it half seasoned, as it were, into print. Just here, too, is a point that younger men are prone to overlook. So long as matter remains unpublished all revisions or corrections constitute presumable added perfections; but once the matter is printed all of these become criticisms and reflect on the accuracy and conservatism of the author, even though he may himself make the revisions. Then again, these revisions of published papers may not in every case reach every one of those who have received the original and thus the misinformation at first diffused may not all of it be overtaken and rectified. While, then, admitting that there are cases where the matter of publication is in the hands of those who do not know what really constitutes an investigation and are mentally unfitted for judging the value of results, yet it seems to me that these instances are exceptional and that the prevailing custom is with these exceptions as good as we can at present devise.

There is, however, another feature of the question that should not be lost sight of, viz., one's duty to his colleagues. By this I mean he has not, morally, the right to hoard up what he learns and thus prevent its being placed on record where other investigators may profit by the information. But, it may be suggested, this will flood our literature with unfinished work. It will inject into our entomological publications the many fragments of information that all good observers will and must accumulate in carrying on any other larger investigation; and the publication of these places them at the service of other workers. Possibly there may be one who is engaged on a kindred problem in another part of the world but who finds himself practically at a standstill for the lack of one of those fragments of science which he is unable to secure,—the key stone to the arch, as it were. Besides, we must remember that no entomological work is ever completed. The best that can be done only remains the best until some one with improved facilities and technique, or with added biological knowledge, shall be able to do better. I sometimes liken this to an endless stairway, with baskets placed on each step. The stair is progress and the basket the problem. An investigator, as he makes his way forward and upward, takes up a basket and carries it forward one or more steps, then sets it down and passes on into the unknown. Another follows and perhaps does the same, and in this way is advancement accomplished. I never take up a problem that was begun by Harris, Fitch, Riley, Lintner or others without a mental picture of the stairway and baskets coming to my mind's eye, and with a feeling of reverence for the good that these men, working with crude instruments, primitive technique and almost no literature at all, were able to accomplish, and wonder how far I shall be able to lift and carry the basket onward and upward before, like these men, passing to the great unknown beyond.

I have referred to a more perfect equipment because all of our instruments used in scientific research are continually being improved. Also I have referred to a wider and deeper knowledge of binomics. This leads me to say what you already must have remarked, that economic entomology today is not at all what it was ten years ago and it will not ten years hence be what it is today. If I mistake not, upon you younger men will devolve the duty of making many and diverse revisions. Our present system of classification will frequently be found wholly inadequate for your necessities, and our laws of priority are too ridiculous to stand except as an element of discord.

In dealing with the work of the systematist, I first wish to call at-

tention to the ~~fact that~~ this work has been, almost without exception, a labor of love, carried on by men with no thought of monetary compensation, and in the midst of lives exceptionally greatly pre-occupied with human affairs around them. They did their best, considering the difficulties under which they were obliged to labor, and the monuments to their self-sacrifice and zeal are not to be ruthlessly torn down and obliterated. But we shall here have a case parallel with the old, low, weather-beaten, historic building giving place to the modern structure of concrete and steel. You to whom will fall the duty of this revision will need to look well to it that you leave these things more advanced than you found them. If I mistake not the time will come when no one will be allowed to describe a new species or revise a group of old ones without being able to present also something in the way of descriptions and explanations of the developmental stages or studies of habits, going to show, beyond a reasonable doubt, that the forms with which he has dealt are really what he represents them to be. So long as the science of entomology consisted in the collection and arrangement of dried corpses the system of classification in vogue was sufficient. But with the new era of entomological research, where insect binomics and the interrelations of different species are more and more generally and fully entered into, the structure is too frail and defective, so that almost as soon as we begin to build upon it we find it full of defects and inevitably it must be discarded and reconstructed on a much broader basis. These are problems that will be forced upon you and which will not for a moment permit slovenly or inefficient work. Please let me explain the use of this last sentence. In some quarters university people seem to be confirmed in the opinion that a graduating student can only make himself and his *alma mater* famous by describing something. It does not seem to matter much what, but once he has done this he has almost smothered himself and his university in a brain storm of glory; sometimes to the discouragement of the poor fellow, who, fortunately, is unable to comprehend the desirability of such proceedings; and I hope to have said enough here to sustain the latter in his stupidity.

Some of you may wish to remind me that a few moments ago I said something about the laws of priority. Only a single instance out of many will be required to illustrate the point that was made. *Smerinthus geminatus*, a common sphingid moth, was described by Say in 1824. It is a common, somewhat variable, species, the specific name, *geminatus*, having reference to the two ocelli on each of the posterior wings. In 1773 Drury described a single moth as from

Jamaica, figuring it with a single ocellus, giving it the specific name *jamaicensis*. Now in rearing a great number of adults from the egg an occasional individual of this latter form will appear, and all gradations between it and the true *geminatus* have been repeatedly observed, and may be reared from the same lot of eggs. The species has never been since found in Jamaica, and no one now believes for a moment that it ever occurred there, Drury's specimen probably having been mislabeled. Neither description nor figure represents the species but an occasionally occurring variation, and the name is not only a serious misnomer but misleading as well as false. Yet, on account of priority of publication, if the laws of priority are followed, this must be considered as the species. This is one of the things that must either be put out of the way or allowed to stand as perpetual contradiction and a discredit to the science of entomology. In the Fifth Report of the U. S. Entomological Commission, pp. 601-602, is a footnote by the late Dr. C. V. Riley, which reads as follows:

"The law of priority becomes a nuisance and a positive injury to the science when pushed to the unnecessary extreme of attempting to solve inexplicable riddles."

I have in the foregoing pointed out some of your frailties, for the man who makes no mistakes is yet to be born, and indicated some of the problems that the older entomologists will probably bequeath you for solution. Besides these, during the years to come, many biological problems reaching far beyond the realms of entomology will be solved by closer, broader, and more careful studies of insects and insect development. Never in the history of American entomology has there been so much to do, and a greater demand for the right kind of men to do it.

Those among you will succeed who adhere closest to nature, who throw all of the weight of every faculty that you can command into your work, with an eye to bringing out the truth for truth's sake, and not for whatever temporary glory or notoriety there may be in it, remembering always that it is not the bulk that you throw into the hopper but what remains and is not rejected through the siftings of years that will stand to your credit long after you have yourselves passed away.

(During the reading of the above paper, President Forbes entered and assumed the chair.)

PRESIDENT FORBES: Discussion of this paper is now in order.

MR. MARLATT: Mr. Chairman, I am very much pleased with and interested in the very thoughtful paper presented by one of the oldest of our members, and I wish to endorse with much heartiness his advice and suggestions. The field of entomology has increased enormously, the number of workers are a hundred times what they were not so many years ago and the amount of money expended has increased at a similar rate. When I entered the service of the Department of Agriculture the lump appropriation was twenty thousand dollars. Our appropriation now runs up to nearly half a million and the same relative increase has been seen throughout the country. With all this increase in entomological workers and funds and facilities, there has been a notable sub-division of the work. All the old problems that the pioneer entomologists attempted to solve are now attacked with a minuteness and specialization that was not then possible. The result is that as this old work is gone over the information is vastly increased, and errors are being constantly found. That is as it should be. The only point I wish to make is, that here and there you find in this new work a spirit of rather sharp criticism of the mistakes of the elders. It seems to me that this attitude is quite unnecessary. We should be charitable and remember that in those old days one man covered the field that is now covered by a score. Take, as an example, the white fly work in Florida, which has now been the subject of three years of continuous investigation by three men. Necessarily, they will find some of the older work faulty, and they will make large additions, but if the spirit of generosity and kindness prevails in these new workers, there need be no unpleasant or sharp criticism in the corrections which they necessarily make.

Looking over the work that is being put out today, the best work is by men who are most courteous and who have least in their writings that is unpleasantly critical. I think that credit should invariably be given. It is not necessary to fill pages with references to the writings of others. Credit can be given without interfering with the reputation of the writer in his own constituency and vastly increasing his reputation and standing in the field as a whole. I think entomologists may take a lesson from some of the other workers in science. There has been a good deal of controversial writing in entomology and bitter enmities have arisen through unnecessarily sharp criticism, through failure to approach one another in a spirit of friendliness and courtesy. I have quite an intimate acquaintance with chemists, and I think, as a body, the chemists are more closely knit. They get more fun out of their meetings; and they seem to have a more friendly spirit toward each other than that sometimes exhibited

by biologists. It seems to me that they, among the scientific men that I have known, have maintained that spirit of generosity, and friendliness, and courtesy which makes their meetings pleasanter and frees their publications from unnecessary criticism. Of course, there are exceptions, and the entomologists are perhaps less faulty in these particulars than others, but it seems to me that this point in Mr. Webster's address should be emphasized. I think we should endeavor to develop the spirit of leniency and courtesy rather than the reverse.

PRESIDENT FORBES: If there is no further discussion we will now take up the next paper.

BIOLOGICAL NOTES ON *MURGANTIA HISTRIONICA* HAHN.

By R. I. SMITH, *Raleigh, N. C.*

As a result of a somewhat disconnected study of *Murgantia histrionica*, I have ascertained a few points concerning the life history of this insect, which will be briefly presented, with the preliminary explanation that some of the work reported upon is not complete in many respects and may, by some of my entomological friends, be considered premature. However, it is hoped that the notes will be of some value as an addition to the knowledge of this troublesome pest.

Definite observations and notes were first made on April 4, 1908, at West Raleigh, N. C., where all the work herein recorded was done. On the date mentioned adult insects were found in abundance on turnip and collard plants, on which they were feeding and mating most actively. It was then observed that very few egg masses could be found and no nymphs were observed until ten days later, or on April 14. Hence this date is considered as approximately the beginning of the first seasonal brood for the year. Some springs may bring forth adults and young at an earlier date.

Notes on Egg Laying Habits

It is generally understood that twelve is the normal number of eggs deposited in each mass, and that these are ordinarily placed on end in two parallel rows of six each, closely cemented together, the eggs usually alternating like the cells of a honeycomb. As a matter of fact, more eggs are laid in more or less irregular masses than in two parallel rows of six each. Out of 94 egg masses laid by females under observation in the laboratory, 62 were irregular in form but contained 12 eggs each, and only 19 were regular, with 12 eggs each, while 13 masses varied in numbers from 8 to 14. Twelve eggs for each mass