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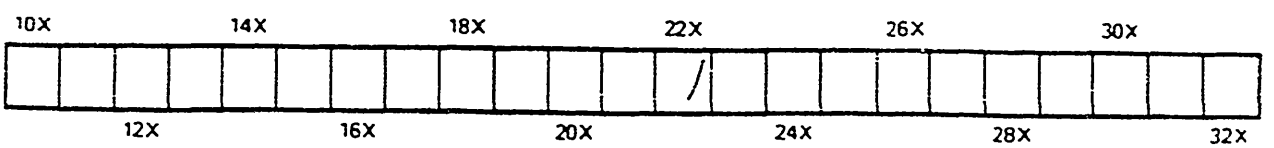

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TORTRICIDÆ.

BY PROF. C. H. FERNALD, STATE COLLEGE, ORONO, ME.

The present is the first of a series of papers on the *Tortricidæ* of North America which the writer hopes to be able to prepare from the material now in hand, and collections that may be made hereafter by Entomologists in various parts of the country.

I was first led to the study of the *Tortricidæ* by the advice of Mr. A. R. Grote, who, with a generosity rarely met, placed his entire collection of Tortricids in my hands to work up, and further gave me his collection of European *Tortricidæ* for comparison, and loaned me his types for study. I fear I may never be able to make anything like an adequate return to this gentleman who has placed me under so great obligations.

I am very deeply indebted to Mr. E. T. Cresson, of Philadelphia, who gave me every facility for a careful and critical study of the types of Clemens, and also those of Robinson. I think it would have been impossible to have recognized some of Clemens' species without having seen his types. Some of his descriptions were made from very much damaged and mutilated specimens, others from single specimens which prove to belong to very variable species, and his descriptions in some instances are insufficient. Dr. Clemens' great mistake was in attempting to make descriptions from imperfect and insufficient material. His genera have also proved a stumbling block to those who have attempted to make out his species. Notwithstanding, this pioneer student of the North American *Tortricidæ* did an admirable work, and his papers will remain a monument to his zeal in the study of Entomology.

It is useless to deplore that he did not have access to the works of the continental authors; had he lived to revise his work, no doubt it would

have been far more satisfactory; or had he lived and done his work in our time, and with the facilities now afforded, no doubt it would have been very different from what it now is.

I refrain from expressing any opinion on the work of Walker upor the North American Tortricids till I have an opportunity to examine his types in the British Museum.

Robinson's paper is a valuable contribution, and his excellent plates and descriptions leave but little to desire so far as he carried his work. It may be necessary, in the light of more recent knowledge, to re-place some of his species.

The next most important and pains-taking paper on the North American *Tortricidæ* is that of Prof. P. C. Zeller. I take pleasure in acknowledging the great obligations I am under to this veteran Entomologist for good advice, suggestions and valuable assistance in my work. Prof. Zeller has without doubt added considerably to the synonymy of our Tortricids, but as I have already shown, it was not possible to prevent it without access to the types.

I would also acknowledge the many favors I have received at the hands of Dr. Hagen, of Cambridge, who gave me an opportunity to examine the types of Prof. Zeller, and has aided me in other ways in my work.

Some time since I saw a request that workers on special groups would give directions for collecting and preserving the insects they are working on, and it may be well for me to act upon that suggestion, since very few collections sent to me are in as good condition as I could desire. Without doubt, the most perfect specimens can be obtained by breeding, and I should be very glad if collectors throughout the country would breed and send to me as many as possible, not only of different species, but different individuals of each species, together with notes on their early stages.

For some time I collected Micros in a cyanide bottle, and they became so denuded by rolling over in the bottle that even with the best of care they were nearly worthless. I have since adopted the plan of carrying a supply of pill boxes, into which I put the Tortricids alive. The advantage of this is that they cling to the inside of the box and are not injured by rolling over. One roll over the bottom of a box or bottle is sufficient to remove the thoracic tufts or other characters of great importance.

When one is ready to pin and spread them, they can then be put into the cyanide bottle, or killed with chloroform, pinned and spread at once. I cannot urge too strongly that *Tortricids should not be touched with the thumb and finger*, but should be handled with a pair of fine forceps, laid upon a piece of pith held between the thumb and finger, and the pin inserted and passed down through the middle of the thorax so as not to injure in any manner the thoracic tuft.

For the larger and medium-sized Tortricids I prefer japanned pins, leaving one-fourth the length of the pin above the insect in case the long or German pin is used. For the smaller ones silver wire may be used, which, with the insect upon it, should be inserted in one end of a neatly cut, brick-shaped piece of fungus, through the other end of which a pin of any desirable size may be passed, and the specimen put in its place in the cabinet. By far the best, and the only fungus known to me, suitable for this purpose, is the *Polyporus betulinus* Fr., which grows in abundance upon decaying white birch. This fungus should be thoroughly dried, after which it may be cut with a razor into pieces of any desired form. A collection of Micros mounted upon neatly cut pieces of this clear, milk-white fungus, shows to very good advantage.

Another kind of pin which is very good for Micros is one of German manufacture, of silver, for sale by B. P. Mann. The great objection to this pin is its high price. A desideratum seems to be a fine silver pin as short or shorter than the English pins, of suitable size for the smaller Tortricids, to be used with the fungus as described. I very much dislike the common insect pins for Tortricids, for they corrode so much, in many instances, as to nearly ruin the specimens.

Of course the collector should spread his captures as soon as their muscles are well relaxed, or else before they become rigid, if he has time, otherwise they may be put aside, softened up and spread at leisure. I would prefer to have all sent to me for determination spread, provided the collectors are skillful at this, but if not, they had better not attempt it lest they ruin the specimens.

Paedisca Worthingtoniana, n. s.

Palpi, head, thorax and fore wings lemon yellow, inclining to straw color in some specimens; outside of the middle joint of the palpi stained with brown; costal edge of the fold of the males and a spot over the

middle of the cross-vein in the fore wings of both sexes dark brown; fringe tinged with fuscous. Hind wings fuscous, darker apically; fringe lighter. Under side of fore wings dark brown, with violet reflections in some specimens. Under side of hind wings much lighter than above. Fore and middle legs fuscous, hind legs lighter. Expanse of ♂, 35 to 39 m. m.; ♀, 43 m. m.

Described from two male and four female specimens, collected by C. E. Worthington in "North Illinois, at dusk about wild phlox on the prairie, flying like *Plusias.*" I also received from Mr. Worthington three females taken at the same time and place, which have the fore wings, especially between the veins, of a light brown color, the discal spot showing plainly through the general color of the wing. I regard this as simply a brown variety of the above.

NOTES ON LARVÆ, ETC.

BY C. G. SIEWERS, NEWPORT, KY.

Last summer, near the end of July, in skirmishing through a wood overgrown with White Snake-root weeds, I struck a large find of the *Callimorpha interrupto-marginata* moths, and collected some 50 specimens. Others collected as many more. The weeds were covered with their larvæ, of a bright yellow color, with a white lateral stripe, mottled along its upper edge with bright red, the anal end being also faced with red markings. The length about 1½ inches. I collected altogether some 200 of them, but utterly failed to bring one to pupa. As they stopped feeding they were taken with a white scouring, leaving nothing but empty skins. Others tried them with like result. The bushes were also strung with their dead bodies. Still some must have escaped, as the brood this year was just as large. But the larvæ failed again to pupate. I think the fault is in the food plant, as cattle will not touch it, and such as are knocked off may take to other food and escape. Cannot some of your correspondents explain this?

About the same time last year I found a remarkably handsome green larva on our Western Coffee-nut tree *(Gymnocladus canadensis).* Length

over two inches; lateral red and white stripes similar to *Saturnia io*, with an anal red horn and two pairs of similar horns on each of the 2nd and 3rd thoracic segments, with short red spines along the dorsal line. In appearance much like *Citheronia regalis*, though but one-third its size, of a hard stony make, evidently an *Anisota*. The pupa has the long spur and indented segments common to the species. I collected quite a number. They are two-brooded, and may be three, as I found them of all sizes at the same time. Female moth measures 2½ inches between the tips of wings, the male 2 inches. Primaries light brown in the females, with and without the central discal brown blotch; the male with darker primaries, with the outer third slightly roseate, and with two white discal superposed spots duskily fringed. Hind wings rose color on both sexes, darkest near the body. The wings of both mottled with brown on the primaries. Should like to have it named.

I had so much trouble with ground for larvæ last year that I concluded to try sand, and got a lot of fine yellow sand similar to that used by moulders, moistened it thoroughly two months since, and it is as moist and loose as ever, and if I may speak for the larvæ that have tried it, they are just delighted with it and plunge right in.

I find empty butter tubs very convenient, having wooden caps and not warping like boxes, but it is necessary to scald them out thoroughly and then lime the sides. Failing to do this last summer, I found some *E. imperialis* larvæ covered with small house ants that had eaten the epidermis full of holes nearly through; they, however, all got over it, though covered with black spots.

ON THE NATURAL HISTORY OF GALL INSECTS.

BY DR. H. HAGEN, CAMBRIDGE, MASS.

The natural history of the interesting gall insects is still somewhat mysterious. A large number of observations have been made here and in Europe by prominent Entomologists; nevertheless, a careful study

of the most detailed papers always gives the impression that something is still wanting to explain the various facts related by the authors. Among the Hymenopterous-gall insects important progress was made in the discovery by the late B. Walsh of the dimorphism of *C. q. spongifica* and *C. q. aciculata*, the latter one a parthenogenetic species. But even here new observations are wanted to fill some gaps in the history of those species. Mr. W. F. Bassett, of Waterbury, Conn., draws my attention to the fact that in a letter in the Proc. Entom. Soc. Lond., April, 1873, p. xv., he "did state most emphatically his belief that all one-gendered gall flies were the alternate of a two-gendered brood from galls of a different form."

Two papers by Dr. Adler, from Schleswig—"Contributions to the Natural History of the Cynipidæ," and "On the Ovipositor and on Oviposition of Cynipidæ," in Berlin, Entom. Zeitschr., vol. xxi., 1877, Decbr., which have just arrived here, are prominently remarkable. I believe the way so long sought for is found, to understand the complicated relations not only of the Hymenopterous gall insects, but probably of all other gall insects, and perhaps, also, of some other insects not gall-producing. These papers are equally remarkable both by the manner of the experiments, the judicious conclusions drawn from them, and the clear and plain description of what he has observed. Dr. Adler has raised the species through several years. In Cynipidæ the raising is less difficult, as the eggs are mature the moment the insect has passed its last transformation; the females are usually disposed to lay the eggs directly, and are, at least many of them, not disturbed by observation; therefore the experiments with them followed through several years become more reliable.

The parthenogenesis of *Rhodites rosae* was proved by direct raising through three years. The fact is, indeed, more remarkable as males exist in a very small number, about one to a hundred females; but a copulation was never observed. The females carefully separated after their transformation, laid the eggs in confinement. Moreover, a number of females were dissected and showed always the receptaculum seminis empty, therefore proving that the eggs were not impregnated.

Other series of observations lead to the interesting discovery of alternating generation by a number of species, which were considered to belong to different genera, but are now proved to be the winter form and the summer form of the same insect.

Neuroterus fumipennis was raised from the galls, the imagines placed

on oak buds, the oviposition observed, and the plants isolated. The galls originating from them were entirely different from those galls out of which *N. fumipennis* was raised. By further growth they proved to be the well known galls of *Spathegaster albipes*, which species was raised from them in due time. These two Cynips belong to two different genera, and differ in size; *Neuroterus*, the winter form, is agamous, only females known, and the receptaculum seminis was always empty; the eggs are laid deep in the buds. *Spathegaster*, the summer form, is bisexual, males and females in regular proportion and copulating; the receptaculum seminis of the egg-laying females being always filled with spermatozoa. The eggs are laid on the leaves. To complete the cycles, *Spathegaster* galls carefully confined were raised and gave in due time *Neuroterus*. The experiment was made repeatedly on a large scale and with excellent precautions, so that no doubt is possible. Now as the fact is known, it is rather remarkable that it was not sooner discovered, as it is entirely impossible for *Spathegaster* and for *Neuroterus* to produce the galls out of which they are always raised. *Spathegaster* possesses a short and somewhat degraded ovipositor, just fit to injure the superficies of a leaf and to lay the egg; *Neuroterus* possesses a long, bent and complicated ovipositor, able to perform the rather difficult act of entering the bud and laying the eggs in the basis of the bud, but would be scarcely able to injure the superficies of a leaf in the same manner as *Spathegaster*. I think this admirable discovery is of the greatest importance for further observations. If we find again a species with an ovipositor not fit to make the galls of the species, we are justified in presuming a similar alternating generation with some other species.

The difficult question how the eggs of Cynips are able to pass through the comparatively small ovipositor is described with much detail and acumen. The observation was only possible by the ingenious device of chloroforming the insects in the act of oviposition, and making an anatomical investigation of the parts. By repeating the observation many times in different stages of the act, a full series of observations, one completing the other, gave a clear and satisfactory result, and at the same time the place was ascertained in which the egg was laid. A large number of other interesting details concerning the formation, structure and the growth of the galls, are given, which must be studied in the original paper. I may only add the surprising fact of the continuous rotation of the embryo in the egg, till it is hatched. The rotation is not

made as in mollusks, by vibrating cells, but by the alternate contraction of long spindle-shaped cells, which are attached to the hypodermis.

Similar experiments with similar results followed also through the whole cycles, proved that *Neuroterus lenticularis* is the winter form of *Spathegaster baccarum*, and *N. numismatis* the winter form of *Sp. vesicatrix*. Dr. Adler adds that experiments with all three species are not very difficult, and he believes that every student will be able to repeat them.

It seemed to the author more than probable that such alternating generation would not exist alone in *Neuroterus*. Similar observations and experiments followed through all the cycles proved that *Dryophanta scutellaris* is the winter form of *Trigonaspis crustalis*, and *Dryophanta longiventris* the winter form of *Spathegaster Taschenbergi*, but for the last species only half the cycles was till now ascertained.

Direct observations proved that the egg of *Dryophanta* is laid with the egg-body downwards and the stem of the egg upwards. The situation is just reversed in *Neuroterus*, and as the egg is always hatched through the hind portion, the larva of *Dryophanta* is obliged to make its galls downwards in the cambium, and the larva of *Neuroterus* upwards on the leaves.

Another agamous genus, *Aphilothrix*, possesses an ovipositor similar to *Neuroterus*, and it seemed impossible that the large galls out of which it develops could be made by such an ovipositor. Similar observations gave the result that *Aphilothrix radicis* is the alternating generation of *Andricus noduli*, and *Aphilothrix Sieboldi* of *Andricus testaceipes*. Neither *Aphilothrix* nor *Andricus* develop in the same year; each of those genera needs two years for its development; therefore the whole cycle runs here through four years, and till now only half the cycle of *Andricus* bred from *Aphilothrix* galls has been ascertained by observation.

I think there can be no doubt that the agamous *C. q. aciculata* is the winter form of the bisexual *C. q. spongifica*, just as *Neuroterus* and *Dryophanta;* but here both species have the same kind of gall, and both species are less different than those above quoted.

The remarkable success of Dr. Adler's experiments with the Hymenopterous galls induced me to compare the rather large collection of Dipterous galls from Europe and America belonging to the Museum. I found directly some forms entirely similar and corresponding to the alternating galls of Cynips; but the collection is rather poor in bred specimens of the

insects. Just in the presence of the judicious and sober observations of Dr. Adler, it would not be proper to say more than that it is not difficult to point out galls of *Cecidomyia* similar to both forms of the alternating *Cynips* galls. It is to be presumed that in *Cecidomyia*, as well as in *Cynips*, the form of the ovipositor will be different, but such investigations can scarcely be successful with dry specimens.

I may add one observation made by myself, by which it is at least probable that bisexual species of *Cecidomyia* may also propagate by parthenogenesis. Some twenty years ago, occupied with the study of insects obnoxious to agriculture in Prussia, I had stalks with cocoons of *Cecidomyia destructor* in corked glass tubes. In one of them I raised a single female, and was sure that no other cocoon was present. The female laid a number of eggs on the glass, which after a few days began to develop so far that the embryo and the segmentations of it were clearly visible. By some mischance the glass tube was left in sunlight and the development stopped. Though I have not been able since to repeat the observation, I am sure that I was not mistaken. I think it is justifiable to presume a possible parthenogenesis for *Cecidomyia*, which, if proved, may lead to successful results concerning the destruction of this dangerous pest.

After having studied Dr. Adler's papers, I remembered directly some similar facts given by Mr. Lichtenstein in Stettin. Entom. Zeit, 1877, on the Hemipterous genus *Phylloxera*; the alternation is here very remarkable. The bisexual form originates from pupæ, which are produced by larger winged forms, which possess no external sexual organs and can therefore not copulate. Mr. Lichtenstein calls this form of propagation anthogenesis. A certain similarity with *Cynips* consists in the fact that the different forms of *Phylloxera* emigrate in spring and return in the fall. So the well known *Ph. vastatrix* emigrates from the leaves to the root of the same plant, and needs therefore no special winged forms for the purpose of emigration. But *Ph. quercus* changes to another tree, and needs therefore two winged forms, which are different one from the other. One parthenogenetic form brings the summer colonies from *Ilex* to *Robur*, and another anthogenetic fall form brings them back from *Robur* to *Ilex*. The fact that some species possess two different winged forms will probably reduce the number of the described winged species. Till now it is only known that *Ph. Lichtensteini* is the anthogenetic form of *Ph. quercus*, and *Ph. Signoreti* probably the same form of *Ph. florentina*. Mr. Lichtenstein presumes that many *Pemphigus* and *Adelges* will possess similar

forms, and observations made by myself on one species in the last year seem to favor his opinion.

Such alternations stated without doubt for Hymenoptera and some Hemiptera, and probable by analogy for some Diptera, will certainly not fail to occur in other orders, and are probable in some Lepidoptera heterocera.

A paper by Mr. P Cameron, in the *Scottish Naturalist* for April, 1878, the substance of which is incorporated and fully approved in the President's Address to the Entomological Society of London, arrives at conclusions entirely unfavorable to Dr. Adler's memoir. To corroborate my views about the memoir I wish to give a few statements.

Dr. Adler gives the facts upon the discovery "of the Parthenogenesis of *Rhodites rosae*" on ten pages, about two-thirds of the first part of his memoir.

In May, 1872, *Rh. rosae* in large numbers was observed; some few males appeared, which were put, together with several females, in the breeding cabinet; but no copulation was observed. For further experiment were chosen females appearing later, of which, by careful observation, it was certain that none of them had been with any male. Those females were put on bushes of *Rosa canina* on May 10th—12 wasps, May 13th—16 wasps, May 26th—10 wasps, June 2nd—6 wasps; together, 44 wasps. Of those 26 wasps were observed in the act of oviposition, and the twigs were marked with a thread around each. The first formation of a gall was observed June 5th, and in all only nine twigs formed galls; giving the positive result that unimpregnated eggs had developed. The experiment was tried again for the purpose of having surer results by repetition and to investigate why the first experiment was without result in so many cases. In 1874 there were put, June 22nd, on rose bushes 8 wasps, 4 of which were observed in oviposition, none producing galls; June 23rd, of 10 wasps, 4 observed in oviposition, 2 producing galls; June 27th, of 12 wasps, 5 were observed in oviposition, producing 3 galls.

Of the 13 wasps observed in the act of oviposition, 4 were dissected, and the receptaculum seminis stated to be entirely empty (without spermatozoa). Every one of the wasps spoken of was carefully observed and not lost sight of till the wasp had begun the oviposition, in which act some persevered for more than 24 hours. Of course observation was not followed through this whole time, but every few hours it was again observed

that the wasp was continuing the oviposition. The five produced galls were in November preserved for the experiment of the following year. In the spring of 1875 there were raised from them 35 wasps, all females, and a large number of parasites. These wasps were put again on rose bushes as soon as they appeared on May 26th, June 2nd, 5th, 7th, and oviposition observed on 11 twigs. After 11 days some of the eggs were examined, and the embryo found in different stages of development (more details are given). Of the 11 twigs 6 produced galls, out of which were raised in spring, 1876, 28 wasps, all females. Those wasps were put on rose bushes June 26th, July 2nd, July 4th, and oviposition observed on 13 twigs, which produced 8 galls. Therefore, through three years parthenogenetic-propagation was observed. The objection that in experiments thus made in the open air oviposition could have been made on the same twigs by other wasps, can not be refuted directly; but if it has been noticed so many times that only galls were produced in the observed and marked places, I believe it is allowable to conclude that none except the observed oviposition had been made.

The other question, if the unimpregnated eggs are regularly developed, was answered in the affirmative by repeated experiments, which are very simple and very easy to be repeated. I put female wasps, raised by myself, which had not been with a male, on shoots of rose put in a jar in water. As soon as the wasps began oviposition, the shoot was placed in a breeding cabinet. After oviposition had ended, the wasp was examined anatomically, and the vesicula seminalis found to be empty. The eggs were examined after 12 hours, and the peripherical layer of cells around the dark yolk was seen; in the following days the development advanced in the regular manner.

Dr. Adler gives on five pages more the most interesting details upon the formation and growth of the gall. As *Rh. rosae* is common here, this part of the memoir is of great value for American students. If we look in Mr. P. Cameron's paper, we find about this matter on p. 156, as follows:

"With the bisexual Cynipidæ the males are in some species nearly as common as the other sex; in others, as in *Rhodites*, they are very rare." That is all!

On the alternation of generations in Cynipidæ the first part of Dr. Adler's memoir contains only (6 pg.) the beginning of the experiments

with *Neuroterus fumipennis*. The second part (24 pg.) was published three months later than Mr. Cameron's paper, which contains, nevertheless, some of the names of the species (5 out of 7) treated in the second part, probably out of a provisionally published notice, unknown to me.

Dr. Adler having raised in 1874 out of all *Neuroterus fumipennis* galls nothing but *Spathegaster albipes*, decided to observe them more exactly. The experiments with *Neuroterus fumipennis* were made in 1875 in the following manner:

The galls were collected in the autumn before. When the wasps began to appear, they were put on a small oak tree in a tub in a cool room. When a wasp began oviposition each twig was enclosed in a glass tube, after the bud had been marked with a thread. There were marked March 14th—12 buds, March 19th—10 buds, March 24th—8 buds, March 26th—6 buds; in all, 36 buds. Besides those, other wasps were put on cut oak twigs placed in damp earth or sand (in which manner the twigs keep well three weeks and longer as good material for observing the eggs), and after oviposition was observed, the isolated twig was covered with a glass bell. The wasps were examined after the oviposition and the receptaculum seminis found to be empty. The receptaculum is in the agamous species always somewhat atrophied; in the bisexual species the external membrane is pigmented, and even if empty, forms a ball.

Besides those experiments, others were made at the same time in the open air. The oviposition was observed March 31st on 5 buds, April 1st on 7 buds, April 2nd on 12 buds, April 7th on 12 buds, April 10th on 40 buds, April 12th on 38 buds; altogether on 114 buds. A curious fact is related that in 42 buds the ovipositor of the wasp was found left in the bud.

The results of the different experiments were as follows: The small oak tree in the tub was kept in the room and began to bud in the beginning of May, and the leaves were developed enough May 14th to see if galls were forming. Of the 36 marked buds, 13 had no galls produced; on the others in all 36 galls were counted. It is to be remembered that here oviposition was made and observed in the room, the twigs carefully isolated by glass tubes, and kept in the room till the tree began to bud. Therefore it is impossible that eggs could have been laid by other wasps. The galls were those of *Spath. albipes*. Concerning the experiments in the open air, the leaves of the tree were May 20th so far

developed that the formation of galls could be observed, and again the galls were those of *Spath. albipes.* Out of the 144 buds marked, galls were found on 68, with about 300 galls. The oak chosen was a small shrub four to five feet high, easy to be examined, and showed no where any other galls.

Out of the collected galls a large number of *Spath. albipes* was raised in the first half of June, and were put in a breeding cabinet with a small oak tree. As no copulation nor oviposition was observed, Dr. Adler decided to try observation in the open air. He succeeded June 3rd in finding several females of *Spath. albipes* in the act of oviposition, and secured six wasps, several leaves, and marked four leaves on which he had observed oviposition with a thread. The lens showed that an egg was deposited. The secured wasps were put on the small oak in the breeding cabinet, and now the wasps were observed ovipositing on two leaves. The next day the wasps were examined, and showed the receptaculum seminis full of spermatozoa. On June 5th and 6th several more wasps in the act of oviposition were observed, and six leaves marked. In the first fortnight no change in the leaves was seen ; in the third week the substance of the leaf where the egg was deposited was visibly thickened, the larva had left the egg, and the formation of the gall began. Then the progress was very slow ; after four to five weeks, in the beginning of July only a very small hairy disk was seen, and only in the end of July the galls could be recognized with certainty as those of *Neuroterus fumipennis.* On all ten leaves such galls were produced.

I have given here the substance of Dr. Adler's experiments only for one pair of individuals, but in the same manner the memoir contains them for seven pairs ; two of these, *Neuroterus laeviusculus* and *Spathog. tricolor*, *Aphilotrix Sieboldii* and *Andricus testaceipes*, are not mentioned by Mr. P. Cameron.

I think every student will be puzzled to find such detailed observations unmentioned in Mr. Cameron's paper, when he asks if a consideration of the biology of the species named affords *any reasonable evidence in favor of this rather startling hypothesis* (p. 154). The only evidence Mr. Cameron tries to give against it is that, if the species are correlated in the way indicated, we ought to find the two forms equally abundant, and in close proximity to each other. He states that only one of the five pairs quoted by him are found together, the other four are not. This fact, *if true*, would be certainly of importance; nevertheless, I

should have preferred in the face of apparently so carefully made experiments to examine first, if one of the two authors, either Dr. Adler or Mr. Cameron had not determined wrongly one species of the pairs. But a stronger objection against Mr. Cameron's assertion is that I possess two of the four doubted pairs from the same locality. The May number of the Ent. Month. Mag., just arrived, has on its first page a notice by Mr. J. E. Fletcher, stating that galls made by *Neuroterus numismatis* proved to be those of *Spathegaster vesicatrix.* This is the third of the four pairs doubted by Mr. Cameron. After all I may quote against such kind of evidence the following remarks of the late Mr. B. D. Walsh in his Cynips paper (p. 11):

"I once argued in print that it was impossible that the army worm moth should exist in the Eastern States, for if it did it must have been found there either by Dr. Harris or by Dr. Fitch, and that scarcely had the argument been printed, when it was proved by indubitable evidence that it did exist."

Mr. P. Cameron's objections against the fifth pair, *Aph. radicis* and *Andricus noduli* show simply that the German text was not understood.

Now where are the direct and well continued observations of facts to blow to the winds this theory? I may add that the unprecedented observation that he put some specimens of *Aphilotrix radicis* in spirits for a week (!) and that they revived, when taken out, would be rather difficult to be repeated.

If such facts, as given by Dr. Adler, are not to be accepted as true, I think they can not be called "hypothesis or theory," but simply a fiction, or in plain English, a forgery—which nobody able to understand the German text will accept.

ON EUPROSERPINUS PHAETON.

BY A. R. GROTE,

Director of the Museum, Buffalo Society Natural Sciences.

The fact that Mr. Strecker has seen fit to misstate the circumstances under which this species was named has induced me to correct the impression he may have created as far as possible.

In our Synonymical Catalogue (1865) the species is entered as follows:

Euproserpinus Grote & Robinson.

16. phaeton.

Proserpinus Phaeton Boisduval MSS.

Euproserpinus Phaeton Grote & Robinson (1865).

Habitat—Western District!

In the descriptional part (p. 30) we say: "We are indebted to Mr. J. W. Weidemeyer for the information respecting this singular little species, which, we believe, has not been hitherto described, while an excellent figure, shown us by Mr. S. Calverley, enables us to present the present description and to fix the species. It appears that Dr. Boisduval has etiquetted a specimen in his cabinet as *Proserpinus Phaeton.*"

So that both in the Catalogue itself and in the description we give Dr. Boisduval credit for the name *Proserpinus Phaeton.* What more it was possible for us to do I cannot see. There is not the faintest desire on our part to avoid giving the fullest credit to all parties known to us to have anything to do with the species. Mr. Calverley's figure came probably from Dr. Behr or Mr. Hy. Edwards with the name *Phaeton.* This figure was engraved for a hitherto unfinished work on the Sphingidæ. Of this work I have an incomplete copy; the last plate is numbered xxvi. The plates are headed: "North American Lepidoptera." The first two plates have in the lower left-hand corner: "Published by J. W. Weidemeyer and S. Calverley, New York." Plate iii. *et seq.* have the name of Mr. W. H. Edwards added. I believe the plates were lithographed and colored by Chas. Walo between 1863 and 1868; possibly these dates are not strictly accurate. To the last plates I contributed originals. Two of the plates (Nos. 18 and 19) were executed in England; W. West, imp.; E. W. Robinson, delt. On plate 19, fig. 1, is a figure of *Lapara* (not *Lipara*) *bombycoides* Walk., which I have long ago shown the reasons for believing to be *Ellema harrisii* or a closely allied species. On plate 13, fig. 5, "*Proserpinus Phaeton*" is figured. In 1868, three years afterwards, we redescribed this species from a specimen loaned us by Dr. Boisduval. The specimen was not labeled to my recollection, and as we were under the impression that Dr. Boisduval knew our Synonymical Catalogue, there could be no question as to the name. We were much puzzled afterwards by Dr. Boisduval disowning the name *phaeton*, describ-

ing the insect as *erato*, and overlooking our Synonymical Catalogue. In his larger work on the Sphingidæ Dr. Boisduval adopts our name for the insect and again overlooks the fact that we had described the species nearly *three years* previous to our acquaintance with himself (though he quotes our work), and supposes that we have chosen a fresh name for the species, when we had never heard of the name *erato* until Dr. Boisduval published it, and, moreover, we had credited the name *phaeton* to him in 1865! How the misunderstanding came about it is now difficult to say. Perhaps Mr. Weidemeyer or Dr. Behr can give the proper light as to where the name *phaeton* came from. I have previously suggested either that the name *phaeton* came from Lorquin or Dr. Behr, or that a transposition of names occurred between Dr. Boisduval and the Californian Entomologists. That Lorquin gave names to species which Dr. Boisduval adopted as his own in some cases is, I think, suggested in the case of the species of *Nemeophila* and others, where the insects are named after the food plants. It is evident that Walker has used Dr. Boisduval's MSS. names without credit. With regard to mistakes of names by transposition, the student need only be reminded of the error with regard to *Oeneis semidea* and an *Aegerian* (See Scudder, Proc. Ent. Soc. Phil., 1865, 13, and elsewhere). As to Dr. Boisduval's inattention to previously published papers Mr. A. G. Butler says this "author's worst fault is a too great appreciation of his own MS. names, for which he does not scruple to sacrifice both genera and species long described by other authors." I do not think, in conclusion, that there can be the slightest ground for the suspicion that we intended any wrong in the matter of the name of this species, since we gave Dr. Boisduval full credit for the manuscript name *phaeton*, giving him precedence in the synonymy, a fact which it suits Mr. Strecker to omit. There remains also no doubt that the correct name of the species is *phaeton*, since our original description is perfectly recognizable and since Dr. Boisduval himself adopts this name in his important work on the Sphingidæ in preference to his own later name of *erato*, giving us credit for the species. I do not think that it will be possible to consider the species either a *Macroglossa* or a *Proserpinus*, and that the generic name *Euproserpinus* must stand.

I feel also at liberty to state, what many of Mr. Strecker's readers may have suspected, that there is a very different reason for his personal attacks upon me than that they are called for by my publications. But I am quite confident that in all my writings I have endeavored to give full

credit to every other writer; where I have failed it has been through unacquaintance with the work of others. And I very much regret that there is an unavoidable jealousy which remains strongest with those whose mental resources are narrowed down to the field of descriptive Entomology. In conclusion, I think I can be spared a lengthy exposition of Mr. Strecker's breaches of the ninth and tenth commandments, and leave the matter to those interested in a subject which has nothing to do with the advancement of science, but rather offensively illustrates the principle of self-preservation.

ENTOMOLOGICAL APPOINTMENT.—We learn with pleasure that Prof. C. V. Riley has received the appointment of Entomologist in the Department of Agriculture at Washington, rendered vacant by the retirement of Prof. Townend Glover, whose failing health necessitated this rest from active labor. While we sincerely regret the severe illness of our good friend, that laborious worker and painstaking Entomologist, Prof. Glover, we cannot help congratulating the Department in having secured the services of such a thorough and vigorous laborer in Entomological science as Prof. Riley is known to be. His long experience and natural fitness for the work in which he is now engaged will, we feel confident, make him a most efficient officer.

CORRESPONDENCE.

ON A COVERING SUPERIOR TO PAPER FOR CORK-LINED BOXES FOR THE CABINET.

DEAR SIR,—

Four years ago I first used a white wash for covering the cork in my cabinet, and I have found it so much superior to paper that I feel induced to recommend it very highly to all Entomologists.

It is cheap and easily applied. I take French zinc (dry) and after adding a little blueing, I mix with it as much milk as will make it about

the consistence of thick cream. With this I give the first coat to the cork, rubbing the stuff with my hand well into the little holes of the cork until these are all closed up. As soon as dry I give it another coat, using the white wash somewhat thinner, and apply with a brush.

Since using it I have never found a single specimen of the tiny paper louse in my cabinet, while prior thereto these pests gave me a good deal of trouble. *Anthrenæ* also give me hardly any trouble, as their places of retreat, the cracks and worm holes in the cork, are entirely covered up with the zinc.

If applied carefully it will have just as even an appearance as paper, and the white will keep fresher and cleaner than paper. Give it a trial.

EDW. L. GRAEF, Brooklyn, N. Y.

ERRATUM, ETC.

DEAR SIR,—

P. 59, vol. 10, line 17, for *Euprepia judica* read *Euprepia pudica.*

During the latter days of March I saw *Pieris rapæ* in considerable numbers at Asheville, N. C.; and on April 2nd I saw many scores of *Æthilla bathyllus* S. & A., near the same place, the elevation of the spot being between 4,000 and 5,000 feet. These were playing around damp places by the road side. I was informed by the farmers that *Doryphora 10-lineata* had never yet appeared there.

W. V. ANDREWS, Brooklyn, N. Y.

DEAR SIR,—

A scientific friend who attended the last monthly meeting of the Entomological Society in Boston, wrote to me the next day of a very interesting communication made by Dr. Packard on the exodus of a luna moth. He "heard a rustling in the cocoon and a curious cutting sound, and saw two black points sticking out, which worked back and forth, cutting the silk until a slit was made large enough for the moth to crawl through. Then he discovered that the black points were *two spines* on the submedian nerve of the fore wings. As the wings expand these spines become covered with the wing scales and do not show." Dr. Packard said these spines exist in nearly all the Bombycidæ, but he did not find that this use of them had been mentioned in any of the treatises to which he had time to refer, it being supposed that moths work their way through the silk, first softening it by a liquid exuded from the mouth.

The information received I brought before the Montreal Branch of the Ontario Entomological Society, and it caused some surprise, as members had never heard of the process. During the evening the President, Mr. G. J. Bowles, exhibited a *Polyphemus* moth just out of chrysalis, which got away and flew into the gas, so burning itself that it had to be killed. The moth was examined, and close to the base of each fore wing a spine was found, quite long and sharp, which could certainly be used by the insect for scratching and tearing the silk of the cocoon so as to facilitate the egress of the moth. The inside of the cocoon at the opening seemed to bear marks of its work. This discovery has excited quite an interest among our members, and we await the opinion of other Entomologists on the subject.

JOHN G. JACK, Montreal.

DEAR SIR,—

During last summer we were visited by an insect which attacked our White Pine trees *(Pinus strobus)*. I refer to Abbot's White Pine Worm, *Lophyrus Abbotii*, a gregarious worm of some note in the south and west. I have not seen it noticed by any of our Canadian Entomologists, and consequently do not know whether it is a common insect in this country or not; but I *do* know that should it become very numerous it would shortly be a very heavy blight on our White Pine, either in grove or forest growth. I have not yet seen any on the imported pines, such as the Scotch and Austrian; indeed the insect seems tenaciously to prefer our native species, probably because of its soft and tender foliage. It appeared here in July and August (I neglected to note the exact date), but in such flocks that they soon defoliated the branches on which they were working, and were thus easily detected. When nearly full grown these saw fly worms measure from three-fourths to one inch in length, are heavily marked by black spots on a dull whitish ground, and have the habit of bending the fore part of their bodies backwards on being approached or disturbed. According to Prof. Riley, the parent saw fly deposits her eggs on the slender leaves of the pine in autumn, where they remain in the egg state all winter, hatching early in summer. The remedies recommended for this pest are hand-picking, the use of dry air-slacked lime or powdered hellebore mixed with water and sprinkled on the affected parts.

B. GOTT, Arkona, Ont.

DEAR SIR,—

As correspondence is invited respecting the habits, localities, occurrence, etc., of insects, I take the liberty to offer a few remarks on the *Doryphora 10-lineata*, and also to send you a list of the Geometridæ that I have thus far taken in this locality.

Wishing to ascertain if the domestic fowl were likely to be of any value in reducing the numbers of *D. 10-lineata*, I procured the assistance of a neighbor who kept fowl (I do not keep them myself), and the following is the result of our experiments:

Our first experiment was to offer both larvæ and beetles to the fowl, but they refused to touch them, and acted as if somewhat afraid. Next we mixed the insects with the corn and other food that was given them, but they refused even to eat the corn for a time; by-and-by, however, they began to eat the corn and soon lost all fear of the insects, although they still refused to eat any. After a few days, by keeping the insects in their food all the time, some of the bravest of the hens began to eat a few insects, and it was not long before the rest joined them, and in a few days more they appeared to relish the beetles about as well as the corn. Up to this time I did not observe any of the fowls eat a beetle from the potato vines, but they now began to do so, and we were obliged to put them in their food no longer. After this the beetles were so reduced in number in this garden that they did no material damage.

It would seem from the above that although the beetles were naturally repugnant to the domestic fowl, yet an appetite for them may be acquired. If the substance of the above has been published I was not aware of it, and give it for what it is worth.

I noticed in the last report of the Entomological Society of Ontario, in the experiments on the Colorado Potato Beetle, by W. Brodie, the remark that "it is very doubtful if *Doryphora*, either in the larva or imago state, will feed on *Solanum dulcamara* or *Datura stramonium*." I have found the insect in both these stages plentifully on *S. dulcamara*, which grows quite abundantly near this place, and they wholly consumed both leaves, flowers and fruit of every plant in this vicinity. They were more abundant on these plants than on my potato plants, which were not more than five rods from some of the former.

I took a few examples of *Brephos infans* Mos., March 23rd, which is some two weeks earlier than I ever made a capture of them before.

J. E. BATES, South Abington, Mass.