

Insects

THE ENTOMOLOGIST'S MONTHLY MAGAZINE

EDITED BY
R. B. BENSON, M.A., F.R.E.S.
K. G. BLAIR, D.Sc., F.R.E.S.
PROF. H. G. CHAMPION, C.I.E., M.A., F.R.E.S.

REV. PROF. L. W. GRENSTED, M.A., D.D., F.R.E.S.

B. M. HOBBY, M.A., D.Phil., F.R.E.S.

R. W. LLOYD, F.R.E.S.

H. SCOTT, M.A., Sc.D., F.R.S., F.L.S.

VOLUMES LXXX & LXXXI

[FOURTH SERIES—VOLS. V & VI]

J'engage donc tous à éviter dans leurs écrits toute personnalité, toute allusion dépassant les limites de la discussion la plus sincère et la plus courtoise.—Lahoulbène.

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The Editors would be very grateful if collectors would send to them any observations of interest, notes of special captures, and names of insects from particular districts. This information is very much appreciated by our readers and your cordial co-operation is requested.

The new collecting season is now in full swing and the Editors hope their subscribers will keep the above request well in mind.

AUTHORS ARE REQUESTED TO SEND THEIR COMMUNICATIONS TO
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(Contd. from p. xxiii of cover.)

One or two minor points may be noticed. Is there any definite evidence that, as is stated on p. 18, the Large Copper occurred in ancient times at Wicken? And is it certain that the Mr. W. Wigan, of Canterbury, was merely Mr. Parry, dealer, of Canterbury, under another name? This is implied on p. 244, and much of the indictment of the dealers depends upon it, but no proof is furnished. And there is probably no basis for the author's implication (p. 237) that records of *Pachetra leucophaea* Vien. from the Canterbury district in 1873 were fraudulent, for the moth is not uncommon within twenty miles of Canterbury at the present time. Many dealers in the last century were no doubt very wicked, but real difficulty in working out the past distribution of British Lepidoptera arises because an unknown proportion of their wares were genuine.—R. F. BRETHERTON.

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VOLUME LXXX

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PRELIMINARY NOTES ON CAPT. GOFFE'S ARTICLE ON THE SYNONYMY
OF SOME GENERA OF SYRPHIDAE (DIPTERA)

BY J. E. COLLIN, F.R.E.S.

Three things are essential for the correct elucidation of all problems in Zoological Nomenclature: (1) A correct knowledge of facts. (2) Correct interpretation of the Code of Rules. (3) Logically correct, and uniform application of the interpretations to the facts. In any publication on such problems, a correct (and not misleading) statement of all the facts is an essential preliminary, but the preparation of such a statement is particularly difficult, involving as it does a close study of all the relative literature. Difficulties also occur in regard to the other two essentials. It was because of the knowledge of these difficulties, and the fact that this question of the Syrphid genera was under consideration by the Dipterological Subcommittee of the Royal Entomological Society's Committee on Generic Nomenclature, that an effort was made by the present writer, upon receiving a 'proof' of Goffe's article (*antea*, pp. 109-117), to arrange for either (1) immediate collaboration between Goffe and the present writer so that all criticisms could be dealt with before publication or (2) collaboration between Goffe and the Dipterological Subcommittee mentioned above on all the problems involved. However neither of these offers was accepted.

Only those who have attempted to apply the Rules to the many tentative proposals for a classification of the Diptera made by early writers can appreciate the obstacles confronting Goffe on the road he has ventured to tread, and these difficulties have been enhanced rather than diminished by some recent decisions of the Zoological Commission. All scientific work is a search for *Truth* by the aid of *Reason*, and as the personal element enters into nomenclature one must add *Justice* when dealing with nomenclatural work. Justice to the first describer is the only logical reason for the law of priority. One thing is quite certain, that the only decisions on questions in nomenclature (as in all other scientific work) likely to be permanent, are those based upon this triple foundation, and, with the provision for applications for suspension of the Rules, such decisions are always possible.

It is unfortunate that Goffe has worked at a disadvantage in not having direct access to much of the literature. This probably accounts for a certain number of the incorrect, or misleading, statements of fact and faulty conclusions, found in his paper, which affect the use of some of the names. As all these will be dealt with in a report by the Dipterological Subcommittee mentioned above, it is proposed at the present time to offer only some very necessary criticism of statements in Goffe's 'Introduction,' and on the supposed identification of the genus *Zelima*.

Introduction (pp. 109, 110). The reason why little was heard of Meigen's 1800 paper until the beginning of the present century was because it was then universally accepted that such generic names were unrecognizable until validated by their author. It was indeed made clear in Baumhauer's Introduction to this paper that Meigen's generic names were intended to be unrecognizable, until such time as he decided to make them recognizable.

The full report of the discussion by the Zoological Commission on Opinion 28 shows that the only point considered by them was whether

Meigen's paper had been published. They did not 'decide the issue' raised by Dr. Aldrich (v. 1911, *Canad. Ent.*, 43: 35 and 108), and only indicated (as a natural corollary of publication) that the names must be used in every case where they 'are found valid under the Code,' thus indicating that it was still necessary to test their validity; indeed the Commission themselves made this obligatory two years later by the promulgation of Opinion 46. It has not since been disputed that Meigen's paper was published, but it has been maintained that Hendel's guesses at the identity of the genera cannot be accepted as validating the names in accordance with the Rules, and that the names must remain unrecognizable as originally intended. Those who protest that the Rules must be strictly followed in all cases may well consider that Bezzi's tacit admission that this was not the case in Hendel's interpretation of the 1800 names (1908, *Wien. Ent. Z.*, 27: 252) settled the point, though Hendel subsequently (1909) published a naïve denial that Bezzi really meant what he published (*Wien. Ent. Z.*, 28: 36), a denial which could deceive no one. Aldrich accepted Bezzi's admission as final in 1908 (*Canad. Ent.*, 40: 432). Evidence that the Zoological Commission had come to the same conclusion was provided four years later (1912) when Opinion 46 was promulgated, an Opinion indicating the *only means* by which genotypes could be assigned to generic names such as those in Meigen's 1800 paper, yet there is no mention of this Opinion in Goffe's article though it has been recently analysed at some length (1942, *Ent. mon. Mag.*, 78: 97-103).

It is evident that in paragraph 4 of his Introduction Goffe has based arguments and made submissions (including a charge against 'a number of Entomologists') upon an entirely incorrect conception of Opinion 28.

The genus Zelima Meigen 1800 (pp. 110, 111). This attempt to identify the un-named species originally included in the genus *Zelima* is a perfect example of the fault in reasoning known as '*petitio principii*,' or 'begging the question.' The whole argument is based upon the false assumption that *Zelima* was a genus of Syrphidae, and proves nothing. Though Meigen's 1800 paper was called a 'Classification,' no references whatever were given to families, therefore to state categorically that 'Meigen . . . raised the number of Syrphid genera to twelve' in this paper, gives the entirely false impression that he stated which were Syrphid genera, whereas in all cases the family to which any genus belonged must be recognized from the brief generic diagnosis only, and there is *nothing whatever* in the original diagnosis of *Zelima* to indicate that it was a Syrphid genus, it might equally well have been a genus in the Muscidae Acalypterae. Meigen did not even arrange his genera in order of families; for instance, a known genus of Tachinidae is placed between the two known Syrphid genera *Chrysogaster* and *Syrphus*. It would appear that the genera were purposely misplaced as part of the intention, indicated in Baumhauer's introduction to the paper, to make them unrecognizable. This intention is now well known, therefore it is incorrect to state that the generic descriptions were 'intended to be contrasted with one another.'

Even if the utter impossibility of recognizing *Zelima* as a Syrphid genus be overlooked, *Xylota segnis* could not have been one of the originally included species. Only five characters were given as distinguishing the two

species originally included under *Zelima* from all other *Diptera*, and the only important ones were: (1) Third antennal joint 'en palette oblongue' and (2) Hind femora 'armées d'un rang de piquans.' One is certainly not entitled to claim to recognize species as belonging to this genus unless they possess these two characters exactly as described. With regard to the first character, the meaning of Meigen's 'en palette oblongue' can only be decided by analogy with other descriptions by him in which his meaning is known; we are not entitled to place our own interpretation upon it. The third antennal joint in both *Xylota segnis* and *sylvarum* is very similar in shape to that in species of *Syrphus*, viz. slightly longer than wide; this joint in *Syrphus* was described by Meigen in the 1800 paper as 'en palette' (without qualification), and in his 1803 paper as 'länglich rund.' If 'en palette' equals 'länglich rund,' then the 'en palette oblongue' of *Zelima* must indicate a joint much longer in proportion to its width than is indicated by the 'fast kreisrund' of *Eumerus segnis* and *Heliophilus sylvarum* of the 1803 paper. This alone is sufficient to make Goffe's suggested identification and synonymy impossible, but it is supported by other evidence: *segnis* has two rows (almost equal in length) of spines beneath the hind femora, and we are not entitled to assume that Meigen overlooked the second row.

Finally, Goffe states that *Xylota segnis* and *sylvarum* are 'widely distributed species,' the former 'abundant everywhere,' the latter 'common,' apparently in an attempt to prove that they were known to Meigen 'en nature' in the year 1800, for proof of this is just as necessary in the case of species selected as originally included, as proof that they were natives of Europe, because of Meigen's restriction of the included species in the 1800 paper to those possessing both these qualifications. A reference to Meigen's descriptions of these two species in 1822 proves that even after more than 22 years' collecting he could only state that *segnis* was 'nicht sehr häufig,' and *sylvarum* 'hier nicht gemein auf Blüten.'

Though synonymy seldom has any bearing on the question of genotype fixation, because synonyms do not automatically have the same type, one cannot honestly maintain that *Eumerus* is a synonym of *Zelima* when, of the only two important characters distinguishing *Zelima*, one is obviously not the same in *Eumerus*, and the other is not even included in its diagnosis, so that this latter character (the single row of spines) is presumably one peculiar to *Zelima* only.

Further mistakes on p. 110 include the limitation of Meigen's '2 espèces' under *Zelima* to species 'recognized up to 1800'; they might also have been undescribed species known to Meigen. There are two false assumptions in connection with the list of species considered referable to *Zelima*: (1) that there were twelve Syrphid genera in the 1800 paper, and (2) that it is possible to apportion species to all these genera. It is overlooked that *Syrphus annulatus* Pnz. was listed by Meigen in 1822 as a synonym of *Eumerus tricolor*, and why omit species of *Sphegina*, *Ascia* and *Eumerus* from the list? Also, why assume that Meigen included *Zelima* in his 1803 paper under another name? Even Hendel did not maintain that all the 1800 genera were redescribed in 1803. The question whether it is possible (or impossible) to recognize from

the original generic publication any one of the species originally included under Meigen's 1800 generic names, and thus decide, in accordance with Opinion 46, whether any one of these generic names can be validated (or must become a *genus dubium*), is not one to be settled by assumptions such as those initiated by Hendel, but by reason based on truth.

Everyone is liable to make mistakes, some more than others, and that is why all scientific work involving changes must be diligently checked before the conclusions are adopted. This checking is a duty (a disagreeable duty when it means publicly correcting the mistakes of a personal friend) which every specialist must undertake without hesitation, otherwise confusion will only become more confounded when the mistakes are ultimately discovered.

It would not be correct for the present writer to anticipate any decisions on the generic names to be used in the Syrphidae which the British Committee on Generic Nomenclature may finally agree to adopt; he can only advise students of British *Diptera* that they can avoid causing unnecessary confusion by using the old names until the Committee's report is published.

Raylands, Newmarket.

June 15th, 1944.

Apamea (= *Xylophasia*) *monoglypha* Hufn. (Lep., Agrotidae) as prey of bats.—The note by Prof. Heslop Harrison (*antea*, p. 120) recalled to me an observation which I wrote down at the time but which has remained unpublished nearly fourteen years. On July 10th, 1930, when visiting the ancient church at the little village of Iden, near Rye, Sussex, I found the floor in several parts of the building, nave, aisles and chancel, sprinkled with fragments, mostly wings, of a large number of moths, and with excreta of bats. I also had little doubt that I heard bats rustling and squeaking among the roof-timbers, where they were apparently firmly established, though the church was not in a neglected state. All the moth-fragments I could find were put in a match-box and sent to Prof. Poulton, whose paper on 'British insectivorous bats and their prey' had been published the preceding year (*Proc. Zool. Soc. Lond.*, 1929: 277-303). I thought that Prof. Poulton might include my record in a supplement to his paper, but (so far as I know) none was written. I later recovered the pieces, which Mr. W. H. Tams kindly examined and pronounced to be nearly all referable to one species, the Dark Arches (*A. monoglypha*). This moth is evidently eaten in great numbers by British bats, for in the table of remains of specimens on pp. 284-5 of Prof. Poulton's paper it has the second highest total, being exceeded only by the Large Yellow Underwing (*Triphaena pronuba* L.). I do not know what species of bat were present in Iden church, and in many cases it is uncertain how far bats are selective, or which bat-species devour any particular species of moth. But *A. monoglypha* is mentioned in the same paper (p. 297) as preyed on by the Greater Horseshoe Bat (*Rhinolophus ferrum-equinum* Schreber) at the Cheddar Caves, and (p. 295) as being almost certainly eaten by the Whiskered Bat (*Myotis mystacinus* Kuhl).—HUGH SCOTT, Ancastle Cottage, Gravel Hill, Henley-on-Thames: June 16th, 1944.

Oxyptera pilosicollis Bernh. (Col., Staphylinidae) to be deleted from the British List.—'Taken by Dr. Joy in a log of wood in Windsor Forest, Easter, 1926. This is the only known British specimen, and is now in the National Collection' (Donisthorpe, 1939, *Prelim. List Col. Windsor Forest*: 27). An examination of the specimen in question (which bears a label with Bernhauer's writing) shows it to be nothing more than a rather large and brightly coloured example of *Ischnoglossa proluxa* Grav. Mr. Donisthorpe and Dr. Blair agreed that this was so. The species must therefore be erased both from the above list and from the British fauna.—A. A. ALLEN, 27 Churchfield Path, Cheshunt, Herts.: June 19th, 1944.