TRANSACTIONS

OF THE

CITY OF LONDON Entomological & Natural History Society

[19]

FOR THE YEAR 1909.

PUBLISHED BY THE
CITY OF LONDON ENTOMOLOGICAL SOCIETY,
THE LONDON INSTITUTION, FINSBURY CIRCUS, E.C.

Price Two Shillings.

CITY OF LONDON

Entomological & Natural History

SOCIETY,

Established 1858.

MEETINGS HELD AT

THE LONDON INSTITUTION

FINSBURY CIRCUS, E.C.

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Hon. Secretaries — *Reporting*	S. J. Bell, "Wyndcroft," Farnborough, Kent.
Hon. Secretaries — *Corresponding*	Thos. H. L. Grosvenor, Walldeans, Gloucester Road, Redhill.

AND

Rev. C. R. N. Burrows and Messrs. Edward Harris, J. Riches, A. Sich and G. H. Heath.

TRANSACTIONS

OF THE

City of London Entomological

AND

Natural History Society.

PART XIX.

(1909.)

WITH LIST OF MEMBERS.

THE SOCIETY'S ROOMS, LONDON INSTITUTION,
FINSBURY CIRCUS, E.C.
NOVEMBER, 1910.

Names and Addresses of Members.

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Willsdon, A. J., 46, Dover Road, South Woodford, Essex.

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REPORTS OF MEETINGS.

December 15th, 1908.—Sesia andreniformis and its Parasites.—Mr. E. A. Cockayne exhibited stems of *Viburnum lantana*, showing various phases of the larval borings, together with the imago, and two ichneumons bred from *S. andreniformis* pupæ—*viz.*, *Bracon variator* and *Meniscus pimplator*.

Melanic Argynnis selene.—Dr. G. G. C. Hodgson, four specimens from East Sussex, early June, 1908, in one example, the melanic tendency was exhibited in the marginal area of the wings, in another in the basal area, while the most extreme form had the marginal half of both primaries and secondaries clouded with black broken up by fulvous dashes.

Geometridæ from Peru.—Mr. L. B. Prout, in exhibiting a box of geometridæ from Peru, drew attention to the fulvous patches present on the underwings of many species, and remarked that, as these are invisible when the insect is at rest, they must apparently serve as warning colours during flight.

Paper.—Mr. E. A. Cockayne read some notes on *Sesia andreniformis*, which are included in this volume.

January 5th, 1909.—Death of Member.—The President announced, with deep regret, the death of Mr. J. A. Clark, and proposed that a letter of condolence be sent to Mrs. Clark; this proposal was supported by Rev. C. R. N. Burrows and duly carried.*

Cidaria reticulata.—Mr. H. M. Edelsten, a series bred from Windermere pupæ.

Cœnonympha pamphilus, abs.—Dr. G. G. C. Hodgson, a specimen with ocelli on upperside of primaries obsolete, and many others with the ocelli only faintly indicated.

Polyommatus phlœas abs.—Mr. V. E. Shaw, an imago with hindwings entirely black, Darenth, July, 1908, and another with band on left hindwing, straw coloured, Bexley, August, 1908.

January 19th, 1909.—Vars. from Mucking.—Rev. C. R. N. Burrows, *Polyommatus phloeas* with right upperwing mottled with yellow, also a black *Xylophasia monoglypha*, an almost white *Orgyia gonostigma* ♀, and *Odonestis potatoria* var. *berolinensis*, and ab. *intermedia*, all from Mucking, 1908.

Gynandromorphous Bupalus piniaria.—Mr. A. W. Mera, on behalf of Mr. H. B. Williams, a specimen with ♂ rightwings and antennæ, and ♀ leftwings and antennæ.

Abraxas grossulariata var. varleyata.—Mr. L. W. Newman, examples from second brood, part of seven specimens ex a brood of 31 imagines from typical parents ex *varleyata* and type.

Melanippe fluctuata ab.—Mr. V. E. Shaw, an imago from

* An obituary notice appeared in the 1908 volume of *Transactions*.

Bexley, with central band carried uniformly across the whole width of the upper wings.

Camptogramma fluviata.—Mr. A. J. Willsdon, a fine series bred from Bournemouth ova.

Paper.—Mr. L. B. Prout read some notes on the *Fidoniid* group, which are included in this volume.

February 2nd, 1909.—Member Elected.—Mr. H. B. Williams, of Stoke Newington, was elected to membership.

Coenobia rufa, abs.—Mr. H. M. Edelsten, a long series from various localities, including abs. *lineola*, *pallescens* and *fusca*.

Noctua glareosa.—Mr. W. J. Kaye, a series from Richmond Park, September, 1907.

Phlogophora meticulosa, late emergence.—Mr. H. Leach, an imago taken when drying its wings, December 11th, 1908.

Dianthoecia conspersa.—Mr. L. A. E. Sabine, several imagines bred from Bude larvæ.

Tephrosia biundularia, treble brooded.—Mr. A. J. Willsdon, three broods reared during 1906; of the third brood, three pupæ went over the winter, the resulting imagines differing in appearance from the rest of the brood, and being more like the spring emergence.

Paper.—Mr. H. M. Edelsten read some notes on *Coenobia rufa*, which are reprinted in this volume.

February 16th, 1909.—Election of Member.—Mr. Rowland T. Smith, of Stoke Newington, was elected a member of the Society.

Melanic Aplecta nebulosa.—Mr. A. Harrison, a long series from Delamere Forest, bred from ova resulting from a pairing of var. *robsoni*; the brood comprised grey form 25 per cent., *robsoni* 51 per cent., and *thomsoni* 24 per cent.

Paper.—Rev. C. R. N. Burrows read a paper on *Pseudoterpna pruinata*, intended for ultimate publication in the *Entomologist's Record*.

March 2nd, 1909.—Donation.—Rev. C. R. N. Burrows presented an interleaved copy of Staudinger and Rebel's Catalogue.

Polyommatus phlœas was the special subject for the evening. Mr. S. J. Bell, a series including a specimen with pale ground colour and black spots much reduced, and another with copper marginal band on hindwings broken up into alternate dashes of copper and black. Dr. T. A. Chapman, series from Spain and Galicia (? summer brood), and Sicily (? spring brood). Those from the latter district were lighter in colour, and with black spots much less pronounced than in normal English specimens. The Spanish series, on the other hand, consisted mainly of var *eleus*. Examples from Japan and India were also shown; the Japanese series included some with clear ground colour and others with same suffused, but the Indian specimens had the ground colour completely obscured by a smoky suffusion.

Camptogramma fluviata.—Mr. H. M. Edelsten, a series bred from S. Devon ♀, taken October 20th, 1908; the larvæ, with two exceptions, pupated on the same day, and the imagines, with similar exception, all emerged during one day. Mr. J. Riches, larvæ of this

species, hatched on February 4th, and feeding in a hothouse on dandelion.

Melanic Phigalia pedaria.—Mr. A. J. Willsdon, a series bred from wild melanic ♀ from Yorkshire; about 10 per cent. were melanic, a few quite pale, and the remainder intermediate.

March 16th, 1909.—Camptogramma fluviata.—Mr. A. J. Willsdon, a series bred from Bournemouth ♀, including many specimens with interrupted fasciae, which in some examples was reduced to a central spot.

A Discussion as to the cause of occasional exceptional abundance of a species, followed by its absence or scarcity in ensuing years, was opened by Dr. T. A. Chapman, whose remarks are included in this volume.

April 6th, 1909.—Donation to Library.—Mr. A. W. Mera presented the 1908 volume of the *Entomologist's Record*.

Tapinostola fulva.—Oviposition.—Mr. H. M. Edelsten exhibited photos of the ovipositor, showing the special processes used to force apart the curled leaves of the sedge, within which the ova are laid.

Discussion.—Dr. G. G. C. Hodgson opened a discussion as to whether the appearance of the upper- or the undersides of the wings is of the greater importance to *Rhopalocera*.

April 20th, 1909.—Melitæa aurinia was the subject of a special exhibit. Mr. T. H. L. Grosvenor showed a long series, including specimens from Glamorgan and Kent, lacking the usual paler markings, the whole ground colour being tawny. Dr. G. G. C. Hodgson, similar examples from Surrey, and very dark imagines from Cumberland. Mr. L. W. Newman, series from E. Kent, S. Devon, S. Wales, Scotland, Isle of Wight, and Ireland; the Irish specimens were much brighter in appearance than the rest, being most nearly approached by those from Isle of Wight.

Plusia chrysitis, ab.—Mr. E. Turner, a specimen with the metallic blotches copper coloured instead of the usual green tinge.

May 4th, 1909.—Melitæa athalia.—Larvæ.—Mr. L. W. Newman, larvæ bred ab. ovo from Tavistock ♀, and fed on plantain; attention was drawn to the resemblance of the larvæ to the flower heads.

Hybernia marginaria from Epping Forest.—Mr. H. M. Edelsten, a series showing a wide range of variation, and including some dark suffused specimens.

May 18th, 1909.—Japanese Abraxas.—Mr. L. B. Prout, *Abraxas miranda* from Japan, with British *A. ulmata* for comparison.

June 15th, 1909.—New Member.—Mr. J. Morris, of Barnes, was elected a member of the Society.

Euchelia jacobeæ, ab.—Mr. A. W. Mera, a specimen with part of the body of a pinkish colour.

September 7th, 1909.—Acidalia emutaria.—Larvæ.—Mr. L. B.

Prout, larvæ from Sandown (Isle of Wight) ♀; the exhibitor drew attention to the extreme development in this species of the elongated form peculiar to Meyrick's genus *Leptomeris*, which appeared to him to be a very natural one.

September 21st, 1909.—Senta maritima, abs.—Mr. L. B. Prout, a series from Sandown (Isle of Wight), about one-third being var. *bipunctata*; the series included two specimens with spots on right forewing confluent, both of which were taken on the same night.

Callimorpha dominula, abs.—Mr. L. W. Newman, three examples of the yellow form from Deal.

Abraxas grossulariata, abs.—Mr. L. W. Newman, var. *varleyata* with white wing rays, and ab. *axanthoe*, both from Barnsley.

Lycæna argiolus.—Foodplant.—Rev. C. R. N. Burrows, a series reared from ova laid on flowers of Portugal laurel on which the larvæ fed throughout.

October 5th, 1909.—Nonagria sparganii from Sandown (Isle of Wight).—Mr. L. B. Prout, a fresh ♂ taken at light, the first authentic record for the locality.

Iodis lactearia.—Late Emergence.—A fresh specimen taken at Sandown (Isle of Wight), July 19th, 1909.—Ibid.

October 19th, 1909.—Luperina guenii.—Mr. G. H. Heath, on behalf of Mr. Wm. Yates, specimens of a *Luperina* taken at rest on a local grass on the coast marshes of Fylde, Lancs., between September 7th and 25th, 1909. At a subsequent meeting, these were declared by Rev. C. R. N. Burrows to be *L. guenii*.

Sesia culiciformis, ab.—Mr. L. W. Newman, a specimen with yellow banded body from Bexley Wood larva.

Leucania favicolor and L. pallens, in cop.—Mr. H. M. Edelsten, a *L. pallens* ♀ found paired with *L. favicolor* on Essex marshes.

November 2nd, 1909.—Melanippe montanata, ab.—Mr. L. B. Prout, a specimen from Westwell with dark marginal band.

Zygæna filipendulæ, abs.—Mr. L. W. Newman, specimens with usual red colouration replaced by yellow or salmon tinge.

Euchelia jacobeæ, ab. with hindwings almost completely suffused with greyish-black—Ibid.

Acidalia aversata.—Mr. A. J. Willsdon, a series bred ab. ovo from Wansted ♀, of red form most commonly met with in this district. The brood included most of the usual forms.

November 16th, 1909.—New Member.—Mr. A. E. Gibbs was elected to membership of the Society.

Acidalia mancuniata and A. circellata.—Rev. C. R. N. Burrows, specimens ex the collection of the late Mr. J. A. Clark with *A. subsericeata* and *A. straminata* for comparison.

Cidaria immanata from Forres.—Mr. G. H. Heath, a long series taken at random during August, 1909; the forms least in evidence were those with unbroken white band and with white band on brown disc, only one example of each occurring among about 160 specimens.

Angerona prunaria, abs.—Mr. C. P. Pickett, deep orange ♂s with yellow patch on hindwings and ab. *pallidaria* ♀.

Dasycampa rubiginea.—Mr. A. J. Willsdon, a series bred from Bournemouth and Torquay. Those from the latter district were redder and less speckled than the Bournemouth specimens, approaching *Cerastis vaccinii* in general appearance.

A discussion on *Thecla rubi* was opened by Dr. T. A. Chapman, whose remarks are reproduced in this volume.

December 7th, 1909.—Annual Meeting.—New Member.—Mr. J. A. Wright, of Bushey, was elected to membership of the Society.

Cœnonympha typhon, abs.—Mr. T. H. L. Grosvenor, a long series from Westmoreland, Cumberland, and Aberdeen. Those from the former locality included two very dark ♂s, an ashy-grey ♂, and another ♂ with elongated ocelli on upperside of underwings. In the Cumberland and Scotch specimens the ocelli showed a tendency to obsolescence on the undersides.

Melanthia bicolorata from Forres.—Mr. G. H. Heath, a series taken during August, 1909, the majority being var. *fumosa*, Prout; the type and var. *plumbata* were comparatively scarce, while var. *parvula* was the commonest of the light forms.

Election of Council for 1910.—The result of the election was as follows:—

President.—Mr. A. W. Mera.

Vice-Presidents.—Rev. C. R. N. Burrows, Dr. T. A. Chapman, and Messrs. F. J. Hanbury and L. B. Prout.

Treasurer.—Mr. P. H. Tautz.

Librarians.—Messrs. H. M. Edelsten and V. E. Shaw.

Curators.—Dr. G. G. C. Hodgson and Mr. A. J. Willsdon.

Secretaries.—Messrs. S. J. Bell and T. H. L. Grosvenor.

Non-Official Members.—Messrs. A. Bacot, F. B. Cross, G. H. Heath, J. Riches and A. Sich.

Secretary's Report, 1909.

At the conclusion of this year the President has been relieved of a task which has fallen to his lot so frequently as to have doubtless become monotonous, and we are to have this evening a Vice-Presidential instead of a Presidential Address. Perhaps it would have been as well if the Secretary's Report had also thus been made the channel for possible new points of view; judging, however, by the ungrudging way in which members usually permit, and even insist upon, the undisturbed occupation of the Secretarial posts, and the exercise of the privileges *and duties* attaching to same, by those who have in a rash moment allowed themselves to thus have greatness thrust upon them, it might prove difficult to find a volunteer. We commend the suggestion to members' consideration in the future, and in the meanwhile once more lay before them the Annual Secretarial Report. The customary number of meetings have been held during the past year, but the attendance has, alas! been far from customary; the average for 1909 is only 14·45 members and ·3 of a visitor, as compared with 17·05 and ·7 in 1908, 17·55 and ·6 in 1907, and 16·55 and ·8 in 1906.

In the matter of membership we have only gained four recruits; on the other hand, we have to deplore the loss of one of our oldest members, the late Mr. J. A. Clark, while grave misfortune in the shape of loss of sight has overtaken another of our senior members, Dr. J. S. Sequeira, who has the heartfelt sympathy of all his fellow members in his affliction. To the death of one regular attendant at our meetings, the incapacitation of another, and the enforced absence during several months of one of the secretaries, also (if it may be mentioned) usually a regular attendant, the falling off in the attendance may doubtless be at least in part attributed.

As regards donations, the Society is once more indebted to the generosity of Rev. C. R. N. Burrows, who has presented to the Society an interleaved copy of Staudinger and Rebel's Catalogue, while Mr. Mera has again added the latest volume of the *Record* to the library.

Three field meetings were—or, rather, should have been—held during June and July; the weather is seldom kind to us on these occasions, and it is therefore not surprising, in view of the exceptionally bad summer of 1909, that all three meetings were rendered impossible by heavy rain.

The usual annual volume of *Transactions* has been issued, and has, we believe, achieved a record in bulk, which has unfortunately caused a corresponding enlargement of the debit side of the Treasurer's account. Some inconsistencies of nomenclature appearing therein have incurred criticism in the *Record*. The critique was doubtless welcomed by readers of the magazine as affording some change from their usual somewhat monotonous monthly mental pabulum of records of Alpine jaunts; it should also lead to reform of the Society's methods, in the direction of relieving the Secretaries of the task of compiling this volume, and placing it in the hands of those more competent to deal with it, and possessing the temerity necessary to revise members' various nomenclatural idiosyncrasies, even at the risk of making the papers more or less unintelligible to their authors.

The programme for the Winter Session, details of which are annexed, embraced many interesting items in the shape of Papers, Discussions, and Special Exhibits. The number of papers read was perhaps less than usual, owing to one member being unable to fulfil his engagement to the Society in this respect.

1908, Dec. 15.	Notes on Breeding "*Sesia andreniformis*	Mr. E. A. Cockayne, F.E.S.
1909, Jan. 5.	Pocket Box Exhibition.	
,, 19.	Exhibition and Discussion. "*The Fidoniids*" ... Opened by	Mr. L. B. Prout, F.E.S.
Feb. 2.	Life-history of "*Coenobia rufa*" ...	Mr. H. M. Edelsten, F.E.S.
,, 16.	"*Pseudoterpna pruinata*"	Rev. C. R. N. Burrows.

Mar. 2.	Special Exhibit. "*Polyommatus phloeas—vars.* and *abs.*	
,, 16.	Discussion.—"*Re* the cause of occasional abundance of a species, followed by scarcity in ensuing years" ... ... Opened by	Dr. T. A. Chapman, F.E.S.
April 6.	Discussion.—"Which is of greater importance to the *Rhopalocera*—the upper- or underside of the wings ... ... Opened by	Dr. G. G. C. Hodgson.
,, 20.	Special Exhibit. "*Melitaea aurinia.*"	
May 4.	Special Exhibit. "*Arctia caia.*"	
Nov. 2.	Exhibition of Members' Duplicates with a view to exchange.	
,, 16.	Nomination of Council for 1910. Appointment of Auditors. Exhibition and Discussion. Notes on "*Thecla rubi*" Opened by	Dr. T. A. Chapman, F.E.S.
Dec. 7.	Annual Meeting. Election of Council for 1910. Vice-Presidential Address	Mr. L. B. Prout, F.E.S.

S. J. Bell,
T. H. L. Grosvenor. } *Hon. Secs.*

Treasurers' Report.

In deference to a suggestion which I received in the fore part of the year, I have in this year's balance sheet reverted to the old formation. The suggestion was to the effect that the publication fund should be kept under a separate heading as it would serve to bring it more before the notice of members and possibly induce them to subscribe more generally towards it. I trust this will be the case, as you will notice that our subscriptions do not cover half our expenses under the heading, and our annual income from all sources falls considerably short of our annual expenditure, which makes us to some extent dependent upon voluntary donations to the Publication Fund.

Generally speaking, the financial position of the Society is healthy, as in subscriptions there is only a sum of £5 17s. 6d. unpaid, and of this sum two members between them account for £3 7s. 6d.

I have made considerable efforts to be able to report to you to-night that all subscriptions were safely harvested, but I regret to say that in some cases my most earnest appeals have been unavailing.

The balance I have in hand is only half of last year's amount, but this is accounted for by the large arrears in subscriptions that I was fortunate enough to gather last year, and a treasurer, like other people, cannot eat his cake and have it too.

I therefore ask your acceptance of the following balance sheet:—

TREASURER'S ACCOUNT, DECEMBER, 1908, TO DECEMBER, 1909.

Dr. | *Cr.*

	£	s.	d.		£	s.	d.
To Balance in hand from 1908	18	2	1	By Subscription to South-Eastern Union for 1908	0	5	0
,, 1 Subscription at 7s. 6d. from 1909	0	7	6	,, Subscription to South-Eastern Union for 1909	0	5	0
,, 54 Subscriptions at 7s. 6d. from 1909	20	5	0	,, Attendance	0	10	0
,, 2 Entrance Fees at 2s. 6d. from 1909	0	5	0	,, J. W. Tutt's British Butterflies	0	17	6
,, 1 Subscription at 5s. from 1909	0	5	0	,, Rent July 31st, 1908, to July 31st, 1909 ..	12	12	0
,, 5 Subscriptions at 7s. 6d. from 1910	1	17	6	,, F. N. Pierce Genitalia of Noctuidæ	0	5	0
				,, Surrey Mirror	1	2	6
				,, Entomologist Monthly Magazine	0	6	0
				,, Printing, Stationery, and Postage	2	19	11
				,, Repairs to Cabinets ..	0	8	3
				,, Carter Paterson	0	0	6
				,, Subscription to Entomologist	0	6	0
				,, Copies of Rules and Receipts	1	0	9
				,, Dr. Balance of Publication Fund	10	12	0
				,, Fire Insurance	0	6	3
				,, Balance in hand.. ..	9	5	5
	£41	2	1		£41	2	1

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VICE-PRESIDENTIAL ADDRESS.

(Read by Louis B. PROUT, V.P., December 7th, 1909.)

Our highly-respected President, Mr. A. W. Mera, in his laudable desire that the Society shall on the present occasion enjoy something novel in its mental pabulum, has imposed upon me a task which I am unlikely to be able to carry out with any real satisfaction either to you or to myself. I can honestly say that I have had neither the time nor the ability of late to keep abreast of those philosophical questions which might have interested our more advanced thinkers, nor of the doings of British field-entomology during the past season, a topic on which I used to be able to hold forth at some length a decade-and-a-half ago, when I was in the habit of devouring the contents of *The Entomologist* and *The Entomologist's Record* with the utmost avidity. A specialist is proverbially more or less of a bore, and my time for the study of entomology is now so fully occupied with my specialism that I have necessarily let go some of the threads which I used to find a pleasure in holding—and which perchance I should still find a pleasure in holding, were there more than eighteen or nineteen hours' work in the average working day.

This prologue is not intended to prepare you for a dissertation on the classification of the *Geometridae* or on the distribution of the genera and species of the *Oenochrominae* in Australia—subjects on which I have indulgently determined to abstain from addressing you, though they have been a good deal in my thoughts; but it *is* intended to prepare you for a very poor substitute for those interesting summaries of the year's doings among British entomologists which have been a valuable feature of our present President's annual addresses.

I can only say regarding the season of 1909, that the little I have read in the magazines or gathered from correspondents or members of the Society, gives the impression that it has been below the average in general success for the collector. Of course, the bad weather is held responsible for a great deal, but it is a question how far we are justified in blaming it for the absence of good captures; certainly many of the common species of lepidoptera have been plentiful enough, and indeed the abundance of autumn larvæ in many of our suburban gardens has been almost phenomenal. Perhaps not quite enough of our collectors are out-and-out enthusiasts, oblivious to the discomforts of constant wettings or of collecting in spots that are almost under water, to do full justice to the harvest which might be reaped even in such a weeping summer as that of 1909. At any rate, as some notes from the Isle of Wight state, which I contributed to the last number of *The Entomologist's Record*, I had nothing to complain of in the way of scarcity of insects there. By the way, I was told quite recently that the meteorological reports for the year so far, showed the actual rainfall to be by no means excessive—perhaps rather *below* the average, and no doubt it has been the "continual dropping" that has worn out

the enthusiasm of the insect-hunters, whereas it would rather have been more torrential rains which would have been detrimental to the insects.

A few of our recent British discoveries, such as the two plumes (*Stenoptilia graphodactylus* and *Leioptilus carphodactylus*) and the true *Nonagria neurica*, or species which we have only recently learned how to work for (such as *Sesia andreniformis*), have been well in evidence, and our member, Mr. Edelsten, has done good service to science in working out the early stages of the *Nonagria*. There is more real satisfaction in the study of these local sedentary species than in the capture of the rare immigrants, of which there seems to have been a great dearth this year. Speaking of the sedentary species, reminds me that another of our members, Mr. A. Sich, has been admirably working out a *Depressaria* new to our British list—*D. putridella*, Schiff., which is clearly no new importation, but an overlooked species. In another direction of research, we are still expecting a few further novelties to be revealed in the study of the genitalia, in which our friend, Mr. Burrows, is now vying with the long-time enthusiast, Mr. F. N. Pierce, and already we are in the throes of the elucidation of a new *Luperina*, or possibly a new form of *L. nickerlii*. Close microscopic observation of early stages has also still something to teach us in this respect, and I have private information that we are threatened with another new, and probably well-grounded, British "*Oporabia*," which, though differentiable in all stages, unless it be the pupal, seems the most *palpably* distinct in that of the egg. Finally, I must mention the mysterious capture of a conspicuous Noctuid species (and genus) new to science by Mr. L. G. Esson, quite near to Aberdeen, at sugar on a fir tree on July 12th. That there could be no imposture about this seems pretty clear, and the only plausible conjecture seems to be that the specimen—which is in perfect condition—had been introduced at some early stage with some imported conifer, say from British Columbia, where Noctuids new to science are still of no uncommon occurrence. In view of these records, and possibly others which have escaped me, I do not think the year 1909 can be regarded as having been entirely barren of interest to British lepidopterists.

So far as my limited knowledge extends, no startling new theories of insect biology have been launched during the year. A very valuable contribution to bionomics has been made by Mr. Guy Marshall in his paper on "Birds as a Factor in the Production of Mimetic Resemblances among Butterflies" (*Trans. Ent. Soc. Lond.*, 1909, pp. 329-83), and I hope his appeal for help in continuing these investigations will meet with a wide response. Most of the other matter thus far published in that Society's "Transactions" for this year, is purely technical; but the "Proceedings" contain some interesting matter on Reciprocal Mimicry and other current questions.

I now propose to occupy your attention for a few minutes with some random thoughts on two evergreen subjects—or perhaps we should rather say, branches of the same subject—which, in one sense, belong to the domain of the systematist, but which also have some bearing on general theories of evolution and general studies of insect distribution. I refer to the delimitation, apparently necessary for our classificatory work, of "species" on the one hand, and of the still

more ill-defined higher grades (genera, tribes, etc.) on the other. Much that has been said on these subjects has been repeated over and over again at different Societies and in different connections, and is in danger of palling; but there are certain points which, though I cannot at all claim them as original, have not often been brought out prominently in this room—at least, not of recent years; and I will venture to inflict upon you my views of these.

The question, "What is a Species?" will never be fully answered unless we abandon evolution as our "working hypothesis." If new forms may, by any evolutionary process whatsoever, arise out of old, it is surely manifest that incipient species of various grades will have to be faced by the systematist, and that no one criterion can serve him for his differentiations in all cases. Even if it can be proved that certain variations do arise *per saltum*, it would be hard to convince careful and logical thinkers that these become instantly and finally segregated, and so become entitled to the designation "species." Indeed, many of Bateson's examples of "discontinuous variation," many of the dimorphs which are supposed to act as "Mendelian" in cross-breeding, are recognised to breed together so freely that the most ardent species-monger would not think of separating them specifically. This question of breeding together (the "Syngamy" of Prof. Poulton's presidential address on the present subject) and the cognate one of tracing faithfulness to type in pedigrees ("Epigony" of the same paper), probably suggest the best criteria that we can hope to obtain for a decision of our species-limits; but even if we had all the knowledge of these points—which is, of course, an impossible supposition—I believe we should find untold difficulties and irregularities. To give one or two purely conjectural instances: A and B perhaps would be found to breed freely together in a state of nature, and so would B and C, but A and C would, on account of some subtle geographical or physiological barrier, fail to do so; or some individuals of A would produce offspring so mixed in characters, that we should call them two forms (A and B) while other individuals would produce offspring indistinguishable from the parent type; or, in a word, a "species" in one locality, or in one generation, might become a "variety" in another. That there are any cases of this kind yet proved to absolute demonstration, I cannot positively assert, though it seems to me highly probable, that the extreme forms of *Papilio dardanus*, a species discussed in Dr. Jordan's important *Die Gegensatz zwischen geographischer und nichtgeographischer Variation* (1905), would be found incapable of cross-pairing; in any case, some species within my own domain, as a Geometrid specialist, might plausibly, I think, be regarded as suggesting problems of this nature. Even *Cidaria* (*Dysstroma*) *truncata* and *citrata*, on which I discoursed in this room not so very long ago, have relatives in Asia and in North America, which might easily be regarded as specifically distinct from both; yet, the British ones are amply distinct species by any and every criterion. Similarly, *Tephrosia* (*Ectropis*) *bistortata* and *crepuscularia*, which are certainly neither syngamic nor synepigonic, in Southern Britain, may possibly be so in some parts of Europe, as so very few non-British entomologists seem to have noticed their distinctness in dates of appearance, and constancy of facies in accordance therewith; or, at

any rate, it would not unlikely be found, that the Japanese or the North American representative of this group would breed equally freely with *either* of our British species, if given the chance, and might, with equal satisfaction, be referred to either or both.

I am going to take hold of those words "if given the chance," and point out that the only thoroughly workable lines of specific demarcation are to be found in the investigation, not of what *might occur but does not*, but rather of what *does* occur in a state of nature at the present time. In Prof. Poulton's paper on "What is a Species?" to which I have already referred, several passages prove that he has endeavoured to restrict the typical meaning of his word "syngamy" to this normal occurrence of interbreeding, in accordance with his own original definition of the term (*Proc. Ent. Soc. Lond.*, 1903, p. cx): "*Free* interbreeding under natural conditions" (the italicizing of the word "free" is my own); and it is perhaps not inconsistently with this that, on p. cviii, he speaks of "*intermittent* syngamy" in the case where species by their migratory habits get occasional opportunities for mixed crossing and thereby stifle the tendency to segregate into definitely fixed geographical races. But here and there he seems to be satisfied with a very partial degree of contact to justify the unqualified use of the word syngamy. Thus on pp. xciv-xcv, after pointing out that *Amauris niavius* of West Africa and the *dominicanus* form of the East and South-east Coasts are very constant in these regions and were formerly treated as species, he shows that the discovery of a locality (on the North-eastern shores of Lake Victoria Nyanza), where the two forms overlap and interbreed, constitutes them "but a single syngamic community." I do not say that this may not be a perfectly warrantable conclusion for one who is *au fait* with the details of this particular question, and it represents the position taken up by so high an authority as Dr. Karl Jordan. The differences, I admit, may be no greater than the racial ones of which our Palæarctic butterflies, for instance, afford hundreds of examples, and where no question of independent "species" need arise. But I wish to point out that here we are approaching the really difficult cases of which I gave some hypothetical examples just now. The point is that complete isolation or segregation spells complete asyngamy, partial isolation or segregation partial asyngamy (or "intermittent syngamy") and only the absence of every kind of barrier, geographical or physiological, a complete or perfect syngamy.

Hence we are driven back upon the question of "isolation," which was mainly the text of an able presidential address delivered before this Society, by Mr. J. W. Tutt, exactly twelve years ago (*Trans. City Lond. Ent. Soc.*, 1896-97, pp. 40-59). Mr. Tutt treated asyngamy as an effect, rather than as a cause, of species-divergence, but took a very wide view of what kind of isolation might give the original stimulus to the divergence, or be the effective agent in the promotion of asyngamy. He would apparently not exclude the "physiological selection" of Romanes as a possible factor, but he rather emphasizes other considerations, such as phenological or geographical separation, or even segregation to particular foodplants, etc. Dr. Jordan, on the other hand (*loc. cit.*, *suprà*), considers that *all* new species have arisen by *geographical* isolation, and whether we accept absolutely the

extreme position or not, it is certain that much enlightenment has been obtained by an analytical study of the "antithesis" between the two types of variation.

With the rarest possible exceptions, two allied forms co-existing in the same area--especially if at the same period of the year, as emphasized by Mr. Tutt—can be demonstrated to be either structurally distinct or else freely syngamic; and it appears to me that the term "species" has, therefore, an absolutely definite meaning when applied to the working-out of a local fauna, but that it has no really definite meaning when applied to an assemblage of geographically sundered forms. Dr. Jordan argues at considerable length, and with a good deal of justice, that such assemblages ought to be regarded as single species, even if, at the confines of their area, they show some structural difference (as with the aforementioned *Papilio dardanus*), provided always that in intermediate localities they intergrade. He shows the apparent absurdity of the alternative course, whereby the forms inhabiting the intermediate area, might be said to belong to two different species. But is such an anomaly really avoidable in every case? Supposing the connecting link between two forms is not *geographically* intermediate, but that, on the other hand, the two allies (differing structurally) cover the same area, while the intermediate is more or less separated from them geographically—as I suggested at the outset—how can we then escape from the dilemma? Prof. Ray Lankester's "Counsel of perfection" will perhaps, ultimately, come to our aid; that is, we shall abolish the term species, give names to all the differentiable forms, geographical or otherwise, and say of this and that form that they are "syngamic," or syngamic in such-and-such a specified locality, or asyngamic everywhere.

I fear I have been rather discursive, but the impression I want to leave is this: we can, and should, hope to be able to find out how many "species" of lepidoptera breed in Britain; but we need never expect to arrive at unanimity as to how many of their close allies in North America—which never do interbreed with them—ought to be called "co-specific" with them, and how many ought rather to be called "representative species," "closely allied species," etc. Ideally, I suspect every North American form ought to have a separate name, and then, when we arrive at Prof. Ray Lankester's classificatory millennium, every individual worker will be able to say, according to the results of his own researches, "The European form, *Lycaena* (*Celastrina*) *argiolus*, and the North American form, *Lycaena* (*Celastrina*) *pseudargiolus*, agree in every morphological character which has yet been investigated," or, "The European form *Cidaria* (*Dysstroma*) *truncata*, and the North American form *Cidaria* (*Dysstroma*) *mulleolata*, differ in such-and-such point of male genital armature," and so on; instead of being expected, as at present, to say—and perhaps on data which future morphological research may overthrow—"These two *Lycaenids* (*Celastrinas*), are co-specific, and the American form, as well as the European, ought to bear the species-name of *argiolus*.

But if a "species" has so little objective existence, what shall we say of the "genus"? These criteria which have, however inadequately, helped us to determine our question regarding the units of classification in the former grade—precise morphological agreement, syngamy,

synepigony—these are no longer serviceable when we attempt the delimitation of genera. And I venture to affirm, without fear of contradiction, that no living systematist has anything really tangible to supply in their place. Even if we possessed a complete history and pedigree from the time of the first appearance of the insect world on our planet—a more wildly chimerical supposition even than that in which I indulged by way of introducing the first part of my subject—I should still be justified in doubting whether such history and pedigree would furnish us with really satisfactory data for "natural genera." Supposing we said, every group which has been syngamic within the last 10,000 years, or within the present geological epoch, shall be called a genus; is it in the least likely that the evolution of each would have proceeded by anywhere near sufficiently equal stages to give us "genera" which we could with any show of reason co-ordinate? Or if, instead, we said, the last six, or the last twelve "species" which have branched off from each stirps shall constitute our "genus"; can we believe that the result would be any more satisfactory? A stranded waif in the "struggle for existence," like *Tanagra* (*Odezia*) *atrata* or *Aplasta ononaria*, would have to go into a genus with the most ill-assorted company, while large, recent, compact genera like *Eupithecia* would be split up on the most minute characters—if, indeed, we had any morphologists competent to find "characters" serviceable for differentiation at all.

No, let it be stated most emphatically that *a "genus" is purely a grouping of convenience*—or of inconvenience, if you will. I must confess that to me it is quite un-understandable how an entomologist possessing such a grasp of biology and evolution, and such a clear idea of the "species" problem as does Mr. Tutt, should take such a singular and unorthodox view of the functions of the "genus" as we know to be the case. Mr. Tutt's ideal is that the genus ought to be the *next* evolutionary grade to the species. But, as I have pointed out on another occasion, this would really result in giving us probably no more than two species to any genus, and generally one species only; for it must be very rarely indeed that an evolution branch has been *literally* trifurcate—*i.e.*, that three species of a group have become differentiated absolutely synchronously; and of course if all, or nearly all evolution has proceeded by bifurcation, only the newest pair along each line will be congeneric, and all the uncles, grand-uncles, etc., (so to speak) will be accorded separate generic rank. Abundant illustration can be found of this point even within our restricted British fauna; and many of the illustrations will be so obvious as to appeal readily to a gathering of entomologists most of whom have not dived deeply into morphology. Naturally my thoughts fly to the *Geometridæ*, though the other families would furnish just as many examples. Take the genus *Hypsipetes* (*Hydriomena*). It is a very "natural" genus, both in the imaginal and the earlier stages. Yet I suppose we are all conscious of the closer relationship which exists between *trifasciata* (*autumnalis*) and *ruberata* than between these two and *sordidata* (*furcata*). *Ergo*, the two first-named give us the next grade to the species and consequently the "genus," and we must create a new genus for *sordidata*. Probably in actual fact even this is not all; the group is strongly represented in North America, and it may

probably be that one of the nearctic "species" is really the closest relative of our *trifasciata* in which case *ruberata*, too, will become an outcast from *Hypsipetes*, of which *trifasciata* is the genotype.

No one has pressed this position to its absolutely logical conclusion as yet, though Mr. Tutt has come rather near it in many of his recent revisions. Is the tendency to be encouraged and developed? If we want so many units, why not take the species itself as a sole unit, establish—as I have already suggested—a mononymic usage throughout the order Lepidoptera, and devise your biological groupings without imposing the almost unbearable burden of interminable generic names? If a genus is a "grouping of convenience," *can* we say there is any greater convenience in recognizing that only *trifasciata* and *ruberata* are to be called *Hypsipetes*, than that a larger related assemblage shall be so called? Manifestly, from the point of view of the frailties of human memory, it will be a greater *in*convenience. But the defence would not go along this line at all; and every earnest worker will concede that some sacrifice of mere convenience is often demanded *if it enhance the cause of truth*.

The arguments in favour of the recognition of small genera are, I take it, two, and two only; and I think I shall be able to show that both are fallacious.

(1) It is argued that to provide the "next grade above the species" with a separate name, and to use that name in binomial conjunction with the species name itself, gives the clearest and most up-to-date insight into questions of relationship; it being assumed that the higher grades of relationship will be indicated by tribal, subfamily and family names, and so on. But we cannot, I think, provide *all* these grades with names, and—as I shall show almost immediately—we are told we must not use even one of the grades which might help us, the subgenus. Barring the subgenus, the most which we can summon to our immediate service, so far as I am aware, will be: subtribe, tribe, ? supertribe, subfamily, family, superfamily. I have been trying to think how far these would go with a large, half-natural and yet very diversified genus like *Boarmia*, Tr., Hmpsn. We cannot raise it higher than the subfamily rank—*Boarmiinae*; for it is certainly part of a larger group (including *Nychiodes*, *Synopsia*, etc., etc.), which would have to be a family—*Boarmiidae*,—leaving the Geometers as a whole to become the superfamily. If we allow for "supertribes" (rather an innovation) we may possibly *just* get through our scheme, for this supertribe might limit us to the *fairly* typical *Boarmias*, such as are brought together generically by Staudinger and others, leaving outside some of the more heterogeneous oriental elements which Hampson has included as subgenera. The two "tribes" would then, perhaps be *Boarmidi* (=genus *Boarmia* of our British lists) and *Ectropidi* (=genus *Ectropis*, Hüb., = *Tephrosia* of our British lists) and we should still have a little room to disport ourselves among the inner relationships, as for instance by separating *biundularia* and *crepuscularia* generically from the rest. But what are we to do if we should discover that *consonaria* is nearer to these two than this group of three to *luridata* (*extersaria*) or to *punctularia*? Shall we try the "supergenus?" I need not labour the point further. The real answer to the argument does not lie in the almost impracticable nature of the scheme, but in the fact

that to the *very* few specialists who can master the multitudinous grades of relationship, it hardly matters at all which have and which have not, special names, and that the non-specialist knows just as much if he knows that five species are genetically close enough to bear the common appellation of *Tephrosia* (*Ectropis*) as would be the case if he only knew of that nomenclatural combination for the two closest. Nay, further, he even knows a little more; for the average field-worker readily learns—sometimes to his cost—that *Tephrosia biundularia* and *crepuscularia* are the closest relatives; while he may need a clue in nomenclature to teach him that *punctularia* assimilates structurally thereto.

(2) The other argument is said to be historical, but is based on an insufficient study, or inaccurate interpretation of history. Linnaeus is the acknowledged father of binomial nomenclature; and he nowhere preached nor practiced that the genus should be the "next grade above the species." In the order *Lepidoptera* he distinctly indicates that there are but *three* genera— *Papilio*, *Sphinx*, *Phalaena*, yet he immediately proceeds to work out an elaborate system of subdivision for two of these. He does not actually use the word "subgenus," which Mr. Tutt abhors; but as he uses a nomenclatural grade below the genus, without giving that grade a name, it was perfectly competent to his successors to provide a name for it, and "subgenus" is quite obviously the most appropriate. But even if that name be objected to, the *fact* remains; call the names, *Bombyx*, *Noctua*, *Geometra*, etc., "sectional" if you will, still you cannot get away from the fact that they are *there*, and that they denote a grade lower than the genus. It is a mere quibble to say—as some did say in the Hampson Nomenclature Correspondence—that because the names *Bombyx*, etc,, more nearly corresponded to our conception of genera, they ought to be regarded as the Linnæan genera, and *Phalaena* as of some higher rank. Neither really corresponded at all to our modern conceptions, but the same might be said of his *un*-sectionised genera in Coleoptera, etc., whose names have always been accepted without demur. That the author himself intended *Papilio*, *Sphinx*, and *Phalaena*, to represent the true genera, and to form, with the specific names, the pure binomials, is shown by the fact that in the "Amoenitates Academicæ," he writes *Phalaena* so-and-so without the subgeneric interpretation at all; and even in the "Systema Naturæ" we frequently find what is really the same usage—"Affinis *Ph. jacobaeae*," etc. *Nowhere* in his works do we find *Bombyx* or *Geometra*, or the rest, used in a binomial. His usage is, therefore, to all intents and purposes, the same that is recommended in the most up-to-date code of nomenclature, except that he usually omits the marks of parenthesis. For a modern genus with subgenus the correct form of citation is: *Cidaria* (*Euphyia*) *picata*; for ordinary, simple citations, *Cidaria picata* alone. Finally, should it not be conceded that Linnæus had a right to decide which were his own genera, or which were the correct binomials, the claim that with him the genus was the next diagnostic grade above the species is stultified by the fact that he still subdivides even his subgenera (genera, as some would have us call them) *Phalaena geometra*, etc. In this particular group he subdivides twice, first according to the pectinate or non-pectinate ♂ antenna, and secondarily according to the angulated or non-angulated hindwing.

Thus we see that the exception taken to subgenera is, on every ground, a "fond thing vainly imagined," and I trust that they will play an important part in the systematic work of the future. Though splitting up of genera seems almost inevitable with increasing knowledge, it ought to proceed very slowly and cautiously, and the longer we can continue to retain defensibly the name of *Agrotis* for the assemblage with which it is already associated (purged, no doubt, of the most notoriously discordant elements), the better satisfied we ought to be.

I have already exceeded the time limit which I intended to set myself, and must reserve my remarks on appropriate generic characters and differentiations for some other time or place. I will merely conclude by wishing the Society continued prosperity.

REPORTS RE FIELD MEETINGS.

June 12th, 1909.—The excursion to Brentwood was—in accordance with the undesirable precedent of the previous year—almost entirely ruined by deplorable weather. The leader, who had appointed to lead two or three for the whole day, meeting the rest in the afternoon, found only one awaiting him at Bethnal Green Station, and the two decided not to proceed further unless conditions improved. In the late afternoon—far too late for the "official" train—rain ceased, and one or two plucky members and friends made the journey and essayed a little collecting. Few insects were about, and the more interesting, such as *Erastria venustula*, did not show themselves at all.—Louis. B. Prout.

June 20th, 1909.—An excursion to Leith Hill was arranged for this date, but no one put in an appearance at London Bridge station at the appointed time, owing to the morning having been wet.—V. E. Shaw.

July 9th, 1909.—The excursion to Wendover was rendered impossible by unfavourable weather.—H. R. Leach.

SESIA ANDRENIFORMIS.

♀ DRYING WINGS.

STEM WITH CAP OF AVERAGE SIZE.

♀ RECENTLY EMERGED.

Right hand top figure shows stem before emergence.

♂ RECENTLY EMERGED.

STEM WITH UNUSUALLY LARGE CAP.

PAPERS READ BEFORE THE SOCIETY.

NOTES ON SESIA ANDRENIFORMIS.

(Read December 15th, 1908, by E. A. COCKAYNE, F.E.S., F.L.S.)

I am venturing to read these notes of my own experiences with this insect, not pretending that they are anything but a very incomplete account of its life history, but rather to provoke discussion and elicit further information. Since the publication of the Hon. N. L. Rothschild's note in the *Entomologist's Monthly Magazine*, 1906, and in the *Trans. Ent. Soc.* of the same year, it has been found over a very wide area. I have taken it in Essex, Kent, Surrey (many localities), Gloucester and Somerset, in fact, wherever I have found the food-plant growing suitably, except in the Mendip Hills in the last named county, in which it is either very scarce or absent. The wayfaring tree (*Viburnum lantana*) is its usual food-plant.

Our other species of this genus, the guelder-rose, *Viburnum opulus*, grows in such different situations, choosing marshy rather than dry and exposed ground, that one has few opportunities of seeing the two side by side. In Somerset, where both were growing freely in one wood, many of the bushes of *V. lantana* had a few exit holes of the moth; but though, in some cases, branches of the one species, containing these, were interlaced with those of the other, I could find no signs that the insect had ever attacked *V. opulus*. It is, however, recorded as a food-plant at Bleiburg in Austria by Max Bartel, at Tring by Rothschild, and Mr. Edelsten mentioned at the meeting that he had found old borings in the Fens. The common elder, *Sambucus nigra*, belongs to an allied genus, but I was unable to find any trace of the insect having fed on this tree, even where it was growing amongst bushes riddled by *S. andreniformis*. The favourite home of the clearwing is a sunny slope of chalk or limestone, with many scattered bushes of *V. lantana*, but it is also to be found in uncut and occasionally in trimmed hedges, or more rarely in damp shady woods, where their food-plant has quite a different habit of growth, with long straggling stems and fewer, but larger, leaves of thin texture. It is not particular as to which quarter a slope faces; nor have I been able to confirm Mr. Percy Reid's statement that the exit holes always face in one direction. I have found them facing in all directions, even on opposite sides of the same stem.

The larvæ may be discovered in the smallest or largest bushes, boring, in some cases, into a stem only just big enough to hold them, in others into one thicker than one's wrist; twice I have found the cap on the thick main stem, while the pupa was in a tiny side twig. They may be at any height, from the level of the ground to the terminal twigs of the largest trees.

Of the ovum, I know nothing, nor of the very young larva, which may be, as Tutt has suggested in the case of other Sesiidæ, an external

feeder. Soon it bores into the bark and forms a small chamber beneath the outer pellicle, which is raised like a blister. Gradually this is enlarged, and there the larva lies snugly curled up, and so passes its first winter. Considerable activity is shown the next spring, and frass is freely extruded, though the bark still remains raised, even when the chamber is extended down to the wood. By the middle of April, about half the larvæ have bored a small hole into the solid wood; others are much more backward, and may still be found in the outer layer of bark as late as the last week in May or the beginning of June. The hole is first bored horizontally towards the centre of the stem, then vertically, usually in an upward direction.

The passage is gradually enlarged and always kept of the same calibre throughout. The larva is enabled to do this by its extraordinary power of turning in a narrow space.

In the autumn most larvæ complete their tunnels and make a cocoon ready for emergence the next June or July, but some of the more slowly growing ones pass yet another year in the larval state, as is proved by one which I found in April and kept through the winter. Instead of pupating it lived on, still quite small, past the time of emergence of the moth, and would have no doubt produced an imago the next summer, after passing two whole years and part of two others as a larva.

The completed gallery runs upwards or downwards, rarely the latter. I have notes of nine, of which three produced imagines and six parasites, for a distance varying from three quarters of an inch to three inches—usually about two and three-quarter inches. The blind end is packed with frass for about a quarter of an inch; next comes the thin cocoon made of silk and frass, then the smooth passage running at first parallel with the long axis of the stem, then curving almost at a right-angle out to the exit hole. The tunnel is always carried down beyond the curve, sometimes as much as a quarter of an inch, but the prolongation is filled with frass and the surface towards the tunnel made quite smooth. Across the part where the tunnel leaves the wood is a curtain of silk and frass. External to this is the old larval chamber varying in size and shape and partly filled with frass, and finally in the most perfect dwellings the cap. This is formed in the autumn and is a round or oval piece of bark, 5 to 13 mm. in diameter, cut out by the larva from within and kept in place by silk, or silk and frass. In many it fits the hole in the bark perfectly, in others there is a very obvious ring of frass and silk filling the gap, showing that some bark has been eaten away, and that the cap is not merely formed by the drying and shrinking of the undermined bark. This is also shown by a stick, in which the cap is formed of two separate bits of bark, united to one another and the rest of the bark by frass and silk. In other cases there is a ring of small holes in the bark, each filled with frass; in these the formation of the cap has been started and left incompleted. In some, the caps fall off, and in others, the larva goes a stage further and eats the bark, which should have formed it leaving a mass of loosely bound frass in its place. The proportion of perfect caps to the total number is small, I counted 20 out of 100 on one occasion, but a much larger proportion of imagines is produced from these than from the others. Variations in other directions from

the usual type are met with. In one stem there were two galleries parallel, with openings touching one another, of which one was only ¾ inch in length. Both produced parasites.

In two others a larva had bored into the wood and entered half-way up an old tunnel. This was prolonged upwards and the part below the new hole blocked up with frass. In yet another, a larva entered by an old exit hole and made its cocoon half way up the old tunnel. This must have fed up elsewhere, and moved when full-grown. In many of the stems, which I kept through the winter, I found evidences of larvæ having moved from time to time, probably owing to the wood becoming too dry, but I doubt its frequent occurrence under natural conditions.

The exit hole is often situated at a knot, perhaps because such a situation offers more cracks suitable for the deposition of the egg. I do not know the date of pupation, but on April 22nd I opened some sticks which I had kept through the winter, and found four larvæ pupating. I tied the split halves together, and looking again on May 16th, found three dead larvæ and one living pupa.

My imagines appeared between June 11th and July 13th, most in the last week of June. They began to emerge about 10.30 a.m., and continued through the day until evening. I have only witnessed the emergence twice. In the first, at 10.30 a.m., I saw the pupa push its sharp nose out. It rapidly pushed itself further, with ventral surface uppermost, the black and yellow markings of the moth showing clearly through the transparent skin. It rested with more than half its length protruded for two minutes, and then split and allowed the moth to escape. The latter looking very damp ran up and down the stem for two or three minutes. Then it remained quiet, with head upwards, and the expansion of the wings began, and was completed in three minutes. At 10.55 it put down its wings, dried and ready for flight. The second was much slower. The pupæ pushed its head out at 10.30 a.m. Half-an-hour later it moved again and protruded half its length. Five minutes after this the moth emerged, and expanding much more slowly was not dry till an hour after its emergence.

Closely bound up with the life-history of the moth, is that of its parasites. Of these I know seven, of which I have bred five myself. Their importance in its economy may be gauged by the fact that they destroyed between 70% and 85% of my full-fed larvæ.

One is a dipteron, the remaining six hymenoptera. The dipterous parasite is *Thryptocera* (*Digonochoeta*) *spinipennis*, a Tachinid bred from various species of lepidoptera, but uncommon in this country. Of this I bred three from Surrey, between June 5th and 11th, and found two empty pupa cases in a stem at Box Hill.

Of the hymenoptera, the first to emerge is *Bracon variator*, a small insect with black head, thorax, and legs, orange abdomen, and smoky wings. I have bred it from Surrey, Kent, and Gloucester, and found one or two on the stem of a viburnum in Somerset, near an emergence hole. I bred four from one stem, in which their cocoons were packed at the end of the gallery, near the head of a full-fed larva. In a very slender stem, I found the dead body of one which had tried to escape the wrong way, and eating into the pith, died miserably. Two stems had empty cocoons, from which the parasite escaped by making a

minute hole through the bark, and in both were heads of half-grown larvæ of *S. andreniformis*. This makes it probable that the parasite may be much commoner, than the numbers I bred would suggest, since I take few stems containing half-grown larvæ. This ichneumon emerges between May 28th-31st, and probably oviposits on those larvæ which are latest in boring into the wood. Its ovipositor could scarcely reach the others. The parasite is abundant in South England, especially on the chalk, and has been bred by Dr. Chapman from seedling heads of cistus, in which were larvæ of *Mesophlips corsicellus*. Rothschild bred the allied *B. roberti* from *S. andreniformis*. I bred a single specimen of another Braconid, *Phanerotoma dentata*, Pant. ♂, on June 7th, but could not find its cocoon.

Rothschild has bred *Demophiles caliginosus*, Grav., and *Cubocephalus brevicornis*, Tasch. Of the latter I too bred a male on June 2nd, but failed to find its cocoon.

Capron records it from Shere, Surrey, a locality in which this host is found. By far the greatest mortality is caused by the attack of the solitary ichneumon *Meniscus pimplator*, Zett., which I have found wherever I have taken larvæ of this clearwing. It destroys the full fed larva after it has made a cocoon, its own transparent whitish or pale brown cocoon being found inside that of the larva, and it escapes by boring a hole through the cap, where that remains. It appears from June 11th to the beginning of August, and must attack the larva in its gallery in the wood, for which purpose the long ovipositor of the female is well adapted.

For the identification of the dipteron I am indebted to Mr. C. J. Wainwright, and for all information about the hymenoptera to Mr. Claude Morley.

CONTRIBUTION TO THE LIFE-HISTORY OF COENOBIA RUFA.

(Read February 2nd, 1909, by H. M. EDELSTEN, F.E.S.)

Two or three years ago I was in the Norfolk Broads with Mr. Bowles, at the beginning of June, and not having very much to do during the day, we thought we would try and find larvæ of *C. rufa*, about whose life-history very little appears to be known in Britain. As regards the continent, Wilde says "larva unknown," but he describes the pupa, and Hoffman describes the larva, and says it feeds in *Juncus lamprocarpus*, so we went to a spot where there is an abundance of this rush, and where we had often taken *C. rufa* in numbers. We started by looking for sickly rushes, but there were so many stems of all colours, that it was like searching for a needle in a haystack, so we gave that up, and kneeling down in the fen we pulled every stem within reach. At last, when we were nearly giving it up, I happened to pull a sickly-looking one, which came up quite easily, and, on examining the lower end, we saw at once something was feeding within. On cutting it open, we found a small pinky-white larva of the Nonagriid type, and felt sure we had got the right thing. We set to work hard after this, going for all the unhealthy-looking

Abdomen of Coenobia rufa, Haw., ♀, showing apparatus for cutting leaf before egglaying — × 27.

stems, and managed to get about a dozen, some of which duly produced imagines. Since then several facts have been accumulated, of which the following appear to be among the most interesting.

Ovum: The ovum is about ½mm. in diameter, round, whitish, and quite devoid of markings, becoming darker before hatching, when the pinkish segments of the young larva can be seen through the shell. The eggs are laid in the centre of old rush stems, and the way they are placed there is most interesting. The ♀ possesses two sharp spines on the last segment (see plate), with which she makes a longitudinal slit in the stem; the spines are then forced apart, whilst the ovipositor is thrust between them into the pith, and the eggs are deposited in bunches of three to eight. The ovipositor and spines are then withdrawn, and the cut closes, and is hardly to be seen without a lens. The eggs were laid from July 20th to August 1st, and began to hatch about August 10th.

Larva: When first hatched, about 3½mm. long; shining creamy-white; head, thoracic plate, and anal-plate, blackish-brown; meso- and metathoracic segments rather flattened, and pinky-white in colour, other segments rather raised and pinky-brown, giving the larva a rather banded appearance, the body having a few hairs arising from tubercles; true legs slightly dark at tip; ventral surface and legs whitish. The fullfed larva examined on June 3rd, 1908, measured about 16mm. long; head yellow-brown; prothorax and anal segments with straw-coloured, shining plate; colour of body pinkish-white, ventral surface dirty-white; head and body emitting some small whitish bristles; segments bearing also thirteen to fourteen small blackish tubercles. The thoracic segments are rather flattened, rest swollen, anal segment flattened, true legs yellowish, prolegs dirty-white, dorsal line whitish.

Habits of larva: The young larvæ remain in the old stems, making galleries in the pith towards the root, and keep more or less together. They appear to hybernate in these old stems and early in the spring bite their way out, when each one enters a growing stem. The larvæ appear to feed in several stems. They bite an oval-shaped hole about a quarter of they way up the stem, and, entering, feed head downwards, ejecting frass through the hole; on reaching the root they eat their way out, and enter another stem. The infested stems quickly turn yellowish-green and wither. The ejected frass can be seen on the moss, etc., round the tufts of rushes, and a little searching reveals the infested stem. When about to pupate the larva enters an old stem low down just where the sheath ends, beneath the mossy surface of the fen, eats out a chamber, leaving a thin skin over its emergence hole, which is just on the fen surface, and pupates head upwards. They are fullfed from the end of May to the middle of June. The food-plant is *Juncus lamprocarpus*. Mr. Bankes believes it feeds in *Juncus effusus* in his district, and it may feed in some of the other *Juncus* species also.

Pupa: 11½mm. long, with a distinct "beak;" anal end of pupa rounded, with dorsal surface ending in two sharp spines; colour light brown, segments slightly darker, also the ocellar area (beneath glazed eye).

Habits of imago: The imago flies just before dark, and the flight is straight and just above the herbage; they frequently settle, and if

alarmed drop to the bottom of the fen and sham death. They fly very little after dark. The ♀ is very rarely met with, and seems to sit about on the rushes, and seldom flies.

Habitats: The insect is very local, even in its favourite haunts, which are fens and rushy meadows, and it seems to keep to the rushy patches in the broads, and not to be all over the fen. Mr. Prout tells me he has taken the species in the Isle of Wight, where there is a small trickle of water down the cliff, with a few reeds and rushes growing in it, the whole spot being only a few feet square. It occurs throughout Britain towards the end of July.

Parasites: The following parasites were bred from the larvæ: *Barichneumon lepidus*, *Aritranis carnifex*, and *Bracon fulvipes*.

Variation: The perfect insect varies a good deal, and in addition to the forms mentioned in *Brit. Noct.*, vol. i., p. 48, *viz.*, (1) the type (2) ab. *lineola*, Stph., (3) ab. *pallescens*, Tutt; there is (4) an interesting reddish-brown form = ab. *fusca*, Bankes.

I am indebted to Mr. Main for the photographs.

DISCUSSION AS TO CAUSE OF OCCASIONAL ABUNDANCE OF A SPECIES FOLLOWED BY SCARCITY IN ENSUING YEARS.

(Opening remarks by Dr. T. A. CHAPMAN, F.E.S., March 16th, 1909.)

I must claim your indulgence for the crudeness of the few remarks I propose to make; the gross defects, in fact, are only to be excused, if you can find it possible to excuse them, on the ground that this is not a paper, but merely a stop-gap. I should not have ventured to submit this communication as a paper to be entered in the programme of the Society. The chief defect, a serious one from a scientific point of view, is that I ask you to take the data, in some degree, for granted, for the rest, to supply them yourselves. In fact, one of my objects is, if possible, to get you to supply these data from your own observations or from other sources. I believe that I have met with, both in reading and in written and oral communications from many friends, the data I assume, but not always in more than detached observations instead of completely verified detail.

Insects, and especially lepidoptera, perhaps more than other animals, vary very much from year to year in the number of individuals that exist. As a rule, the fluctuation in numbers is restricted within moderate limits, but every now and then some particular species over a larger or smaller area is found to be excessively abundant.

This abundance is associated, if my memory of what I have read and heard be at all trustworthy, not infrequently with two other circumstances. One is, that variation is more common amongst such swarms than in ordinary years, not variation in the direction of extreme and remarkable aberrations, but in the presence of many specimens that deviate somewhat from the type, specimens that are not remarkable, but nevertheless are not common in ordinary years; the other is, that such years of plenty, when one would expect that in

the following year the species would eat up the whole country, are followed by scarcity of the species for some years, or even its apparent absence in the district where it was so abundant. Some accurate observations and records on these two points are much wanted. Such occurrences are mentioned in conversation and forgotten, and no record is made unless the species be a very rare one, or some extreme aberration is taken, and even then precisely what interests us just now is apt to be left out.

Assuming then, that our data are all right, though very probably they are not entirely so, it is interesting to speculate how they came about, and what is the connection between them.

Lepidoptera lay large numbers of eggs, but still very different numbers in different species, ranging from say 50 to 1500 (*A. betularia* lays about the latter number and *Z. pyrina* perhaps nearer 2000) by one ♀. Taking 200 as a fair average to work with, and allowing half to the ♂ and half to the ♀, we find, of course, that if all the eggs laid became imagines, the species would increase a hundredfold in each generation, say annually. This means, that taking one brood and one season with another, 99 out of every 100 perish, not necessarily before reaching the imago state, but before continuing the species. This means a very severe process of pruning away every individual that does not come up to a certain standard of perfection in a great number of very various qualities. The agencies that inflict this loss are no doubt equally various, some taking a large, some a small toll, but amongst them destroying 99 per cent. of individuals.

Now in a given area, a flower pot, a garden, a field, or a whole country, our hypothetical species has—let us say—1000 individuals; next year it would have 100,000 but for the fact that 99,000 are destroyed. Now in one particular year we find instead of the usual 1000 there are actually 100,000 or 1,000,000 individuals. How could this happen? It means that for two or three years certain of the pruning agencies were much less active than usual, and instead of taking 99 out of every 100, they took only, say, 90; if this happened for two successive years, then our normal 1000 would be 100,000, or if for three, it would be 1,000,000.

Of course, if the 99, instead of becoming 90, became some smaller number, then this increase would be still more rapid. It is probable that each pruning agency takes not exactly so many, but a certain percentage, so that the failure of one would be made up to some extent by the greater activity of another. Therefore we may assume that such phenomenal increases as we are contemplating are probably due to the failure simultaneously of two or more destructive forces.

It is sometimes stated, or assumed, that such destruction is not selective, but fortuitous, and that the individuals that perish are as good as those that survive. This can hardly be so; the insect destroyed by an ichneumon may be as well protected against birds as one that the ichneumon leaves, and one that is killed by frost may be as well protected against birds or ichneumons as any survivors, but it is surely certain that whatever danger proves fatal to an individual, is a danger against which that individual is less protected than its fellows.

It follows then, that when, say, ten individuals survive instead of

the normal one, we have one individual protected to the average degree, and nine not adequately protected against the particular dangers that are in abeyance. Nay, it is very possible that these special dangers left not ten, but twelve or fifteen individuals, and that the other destructive agencies reduced the twelve or fifteen to ten, including in the destruction the one specimen that was protected to the ordinary survival standard against the absent dangers; in that case all ten individuals would be below normal survival standard in this direction. It is obvious that supposing one in ten of the more numerous ten-fold population were really of the highest selected type, it would nevertheless be nine to one against its pairing with a similar individual, so that instead of one in 100 being of this type in each generation as normally, now there will be only one in 10,000 in the following year, and in the third year the proportion would be infinitesimal. The next generation would inherit this reduced protection, and the 100 that then survived would be again much less protected than their parents. They would all be individuals that would usually have perished, and to a greater degree than nine of their parents (or of the whole ten).

Whatever may be the precise rate at which this process would go on in any individual case, whether it took one, two, or half a dozen years, and whether the increase in the abundant year was 10 or 100 fold, a process of the nature I have just suggested must have occurred, and the whole swarm probably does not present one individual of the highly selected character of an ordinary year.

Should it so happen that one of the selecting forces temporarily in abeyance selected on a matter of colour or markings of the imago, say the absence of some bird, or some similar condition, then these unselected moths would show more variation in these colours or markings than occurs in an ordinary year. In the following year, the individuals would be still less protected; now let the destructive agencies, against which the individuals that exist are protected to a degree much below the normal necessity, resume their full vigour, the whole race will be practically wiped out, and instead of the normal 1,000 survivors, there will be only a few, say half a dozen, that have, as it were, escaped by accident. It will take some time for this degenerate race to be selected up to normal condition again, in fact, one would expect that if the destructive agencies, against which protection has been so much diminished, resumed their full power suddenly, and continued at concert pitch, extinction must be the result, and the district affected would therefore only be repopulated by immigrants. If the enemy were a bird or ichneumon, it would hardly resume full vigour suddenly, but a climatic variation might have the full effect of a sudden resumption of destructive energy—the result would then be a complete wiping out of the whole race.

If this speculation, or rather calculation, of the effect of selection being temporarily slackened be correct, it must of course be in continuous action, and assuming such fluctuations of selective forces, it would seem to follow that whenever a species became in this way more abundant, it would eventually be rendered proportionally less abundant in the next or some early year, unless the variation of the selective forces were very gradual, extending over some years, and were not great in amount.

What we want to have observed, then, are the fluctuations in the numbers of a species from year to year. We should expect to find any excess of numbers would be closely followed by a rather greater fall.

A speculation of this sort applies, of course, to any living being, and it is interesting to reflect that the beginning of some such cycle as we have been considering, has been progressing amongst the nations of Western Europe for the last century or two. The pressure of selection has been immensely diminished by two great discoveries—the industrial use of coal, and vast regions of great mineral and agricultural wealth thrown open for occupation. How it will be with us when coal is exhausted and these new territories are filled up, and selection again presses with its old force, is still, fortunately for us, a long way in the future, but it will inevitably come. Already we are told, but are perhaps too close to the facts to observe them accurately, that there is an undue increase of undesirable (*i.e.*, unselected) individuals, especially at the extremes of the social scale.

WHICH IS OF GREATER IMPORTANCE TO THE RHOPALOCERA—THE UPPER OR UNDERSIDES OF THE WINGS?

(Read April 6th, 1909, by Dr. G. G. C. HODGSON.)

In attempting to suggest some lines for the discussion of the question proposed for to-night, one has probably been unusually unsuccessful in the matter of confining the attention to a definite point: the point (defined as having no magnitude and no parts) on observation assuming the appearance of a gigantic radiating organism of almost unlimited dimensions. Some tentacles are buried in such questions as those of sexual selection, and the inheritance of acquired characteristics—two possibly of the most debateable topics of the times. Other tentacles are wound among the evolution of butterflies; their sexual and their seasonal dimorphism or polymorphism; the effects of surroundings (climatic or otherwise); mimicry; and when further other tentacles are found entangled in such a study as their enemies, nature, habits, and predilections, where are our guides with the tentacles? Our authorities on these topics, where?

Probably in this connection, more than in any other, we are "up a tree," though, unfortunately, metaphorically only. For physically the position might have a practical and useful result. In that position valuable observations might be carried out, as it seems a scarcely avoidable assumption that, with regard to the enemies of butterflies, no other of their foes has just the range of space, to which our vision (unless through specially adopted attitudes) is absolutely limited, *viz.*, from three to six feet above the solid ground. So that it would seem fair to assume that, so far as we have succeeded in our estimation, any realisation of harmony with their surroundings is not *under*-estimated by us in the matter of butterflies' undersides, but that, conducting ourselves in an average human fashion, we are heavily handicapped in this matter—are perhaps fairly incompetent to judge.

Thus, in the case of imagines in flight, by observations from a tree or from a hole in the ground, or from underneath such herbage as they frequent, and especially of imagines at rest from below, much suggestive material of which we are ignorant might be acquired.

It is hoped at least that these few remarks will justify the imagining that our "point" opens up many lines of discussion.

In suggesting a separate consideration of the opposite surfaces one can appeal to nature as doing the same herself.

Even in such cases as *L. arion*, and some *Polyommatids*, where a median row of black spots traverses the coloured disc on both surfaces of the primaries, there is no necessary correlation. An alteration of shape or size of these spots on upperside, is not necessarily accompanied by a similar condition on underside, or may be represented by a less extent of the same, *e.g.*, striation.

That this is so is noteworthy, inasmuch as this striate condition of spots of underside is a comparatively common condition in allied species. *L. bellargus*, *L. corydon*, *L. icarus*, in all of which a black spot on the upperside of primaries is extremely rare, or unknown in ♂ s. the converse being true of *L. arion*. And in the ♀ s of these species also, with exception of the discoidal spot, any black spots on upperside are extremely rare, whereas in *L. arion*, with its extremely uncommonly striate underside, striate forms of the upperside are abundantly common, or reach to an extreme degree.

The lack of correlation, in any accurate degree, of upper- and underside markings in the case of *L. arion*, *L. bellargus*, and *P. phlaeas*, are only special instances of a common condition in butterflies, *viz.*, that where the markings of the upperside are (as it may be said) reproduced on the underside (or vice versa as it may be), the markings seldom if ever exactly correspond, the limits on one surface are different from those on the other.

Sometimes they would appear to have dropped on one surface, as *V. cardui*, where the upperside shows the extra-median spots of lower wing are nearer to the border than are the compound concentrically ringed ovals of the underside.

In the ♀ s of *P. napi*, *P. rapae*, etc., the black spots of the upper-wings are frequently (between the nervures and past the nervures) more extensive than on the underside. The white bands of *L. sibylla*, on the contrary, are narrower than the same on underside.

In other species the markings have the appearance of having been pushed further away from bases of wings, and this in many (? all) species of a group. In *C. pamphilus*, the apical spot of the upperwing on the upperside occupies principally the outer part of that area opposite the underside marking, and frequently extends beyond it. The same may be said of *C. typhon*, *C. tithonus*, *E. ianira*, etc.—of all in the upper wing. In the case of *C. typhon* var. *rothliebii*, it is on the underside, on the contrary, that the ringed spots are nearer the margin than on the upperside of the hindwings.

And in practically every species examined most specimens show analogous markings to be more extensive on one surface than on the other, *i.e.*, they only partially overlap, *e.g.*, pale lunules of margins of *A. euphrosyne* and *A. selene*, etc., on the underside, tend to be encroached on by the surmounting dark crescents, and even the separating nervures, less than on upperside.

The orange parts of the peacock spots of ♀ blues on the underside, are seldom reproduced to same extent on uppersides.

It stands to reason that probably this would be so; but one confesses that for it to prove a fact of such unmitigated severity came as a surprise.

With regard—in passing—to the phraseology of the title, one may remark, in explanation of this loosely worded heading, that it was intended to refer to the markings and colours of the one surface as compared with those of the other. And one justifies one's choice of subject (in spite of one's consciousness that there are those who turn aside from him who would speak of undersides—perhaps this consciousness is the best justification) on many grounds:—

Firstly, the more one gives one's consideration to the undersides at home or abroad, at rest or in flight, alive or dead, the higher one rates them in Nature's works, and the more one feels one has neglected them, even after one has arrived at the stage when one commences each series with the undersides, when one has decided that, in future, in cases where a specimen shows equally marked and equally rare form of aberration on upper- and undersides, the specimen shall be set as an underside.

Secondly, one justifies the choice of subject on the ground that our little knowledge of their enemies does not put us in the position where we are able to adjudicate on the value to the butterflies of the forms of their undersides; we probably underrate them, and fairly generally neglect this point of view.

Thirdly, one justifies one's choice by reference to the general lack of knowledge, in any way, concerning the undersides as compared with the knowledge obtained of the uppersides, that is to say, on the ground of a general neglect.

Before commencing the detailed consideration of the underside, one begs indulgence for a few terms almost necessary to this discussion, but not in use. The intention is to be able to shortly designate the different portion of the underside, usually contrasted by butterflies (*Vanessa*, (*Pyrameis*), *Coenonympha*, *Colias*, *Euchloë*), where one part is the apical portion of primary, and the whole surface of the secondary—*viz.*, those parts exposed in sleep, which may be termed the phaneron; the other, which may be called the crypton, is the part of the upper wing, approximately, which, in sleep, is "hidden" by the hindwing.

The crypton—the part hidden in sleep (of the underside of the upper wing) varies in different species in its extent, consequently, a name is required for the part of the upper wing which *can* be hidden by the lower wing, whether or no the sleep posture comprises this. This being called the proximon, *i.e.*, the whole upper wing except apex, then one is enabled shortly to say that in some species the crypton is a smaller portion of the proximon than in other species.

These remarks lead to a classification of the crypton as A (i.). The Crypton* may be:—1. Full, then it = proximon, where costæ of

* The terms originally proposed were "Cryptomere" and "Phaneromere," in ignorance of the fact (if it was prior) that Tschermak applied it in other senses. Of which use Prof. Bateson says (Mendel's *Principles of Heredity*) "Cryptomeres, The term is open to the objection which Zoologists specially will feel, that it may cause confusion owing to the fact that the series of words containing 'mere,' are now universally understood to refer to processes of division—or Meristic features." So discussion of priority is valueless.

primaries and secondaries are in opposition, or where even (*A. cardamines*) the costæ of secondaries project beyond those of primaries; or (ii.) limited, when more or less of proximon is exposed as in *V. io*, where crypton is very small. These might, if required, be termed respectively holocryptonic and hypocryptonic species.

There is a third class of butterflies not exhibiting features to which such descriptions apply, for it is the upper surface of the wings which is exposed. *Hesperiidae*, such as *Nisoniades tages*, partial upperside exposed, and *Casiapa porphyrops*, whole of upper surface exposed. Possibly other classes exist.

And B:—In the classification according to markings, the crypton is:—

1. Homologous to the rest of the underside.	(*a*) Fully representing the phaneron character. (*b*) Largely ,, ,, ,, ,, (*c*) Partially ,, ,, ,, ,,
or 2. Heterologous to the rest of the underside.	(*a*) Partially reproducing upperside pattern. (*b*) Largely ,, ,, ,, (*c*) Fully ,, ,, ,, (*d*) More than ,, ,, ,,

Examples of (1) are:—(*a*) *C. rhamni*, *V. io*, (*b*) *V. polychloros*, (*c*) *V. urticae*. of (2) are (*a*) *Melitaea*, (*b*) *A. aglaia*, (*c*) *M. galatea*, (*d*) *V. atalanta*.

And doubtfully under same head as (*d*) *V. atalanta* come such forms as *E. iaira* ♂ where the crypton is of same brown colour as brighter upperside colour of ♀ s. The cryptons of the sexes (with the exception of the apical spot) are fairly identical, but where scarcely any of such appearance occurs on upperside of ♂. A colour here new to the male appears, but common in other sex.

Such forms as *Ogyris amaryllis* ♀ go still further, perhaps where brilliant scarlet marks occur on crypton. Scarlet occurs otherwise not at all in imagines of this species (or ?genus).

Vanessa urticae classified as having (*c*) a partially homologous crypton almost, or quite, occupies an intermediate position, as it is also partially heterologous (*a* of II.) since, in fact, most of markings of upperside occur on crypton in the tints of the underside, the pattern being formed by the obsolescence of the closely-set linear markings of dark colour.

I trust that these terms are not offensively superfluous (we have a host) for it does seem that the mere suggestion of them at least exposes and emphasizes interesting sets of phenomena which we ought to fully recognize. Such as that in *Lycaenidae* we have grades in separate groups, from *Lycaenids* where the crypton fully (*Agriades bellargus*, etc.) repeats the plan of markings of phaneron, or almost (*Aricia astrarche*, etc.) fully does so; through various others to *Theclids* which fairly do so, to *Polyommatids* which more or less (*phlaeas* more) have heterologous phaneron, representing in some degree coloration of uppersides, to *Ogyris* with a pattern special to underside on its crypton. All these with "crypton" coterminous, I believe, with "proximon."

While when we come to other families we have varying habits, as in *Vanessidae*, with varying plans of crypton.

(i.) *Pyrameis* (*atalanta*, etc.) with heterologous crypton; *cardui*,

more rosy, but representing upperside markings of same part; *atalanta*, with added blue, ochreous, and white, but representing colours and markings of upperside otherwise.

(ii.) *Aglais* (*urticae*, etc.) with a heterologous crypton of a special colour (greatly diluted upperside colour? or greatly demelanized underside colour? or some other origin and with some of upperside markings reproduced).

(iii.) *Euvanessa polygonia* with homologous crypton; or almost (*Vanessa eugonia*) homologous crypton.

The classification is merely intended as a rough reference for the purposes of this discussion. But possibly our use for some elaboration of distinctions may depend on our desire for accuracy of phrase as regards positions assumed by butterflies. One would suggest that it is not enough to observe that the wings are closed, to leave visible underside markings only, irrespective of the butterflies purposes, irrespective of that amount of the upperwings underside that is visible, and call that position rest. There are so many conditions of "rest," so many under which, that is to say, butterflies apply the "closure." It is questionable if they do not when sunning themselves with more or less outspread wings in some degree, "rest"; but apart from this:—

(i.) After a long flight, (ii.) in intervals of feeding, (iii.) in intervals of clouding over sun, or even (iv.) in intervals of courtship, a condition of closed wings may occur; but none, at first, at any rate at all commonly, will be found exactly to conform, as to exact pose of wings, to position of sleep; and one item in the last will certainly be absent, *viz.*, the alignment with the body of the antennæ closely pressed together, to say nothing of the considerable raising of the body and hindwings from the support.

Without the expression of such details the position is not that of sleep. It is questionable whether it is advisable, therefore, to use the term rest for any but the temporary conditions and to carefully define the latter condition as the position of (v.) sleep. They ought to be carefully distinguished; as instances of these may be cited *V. io*, in which the attitude taken upon first alighting for rest, with hindwings largely hiding the forewings, slowly, sometimes jerkily, is changed to position of sleep, with underside of forewings largely exposed, *cf.* also *V. c-album*. And as instance of many others, *T. betulae*, in which opposite is the case; I have never been able to surprise the few examples of these that I have observed with antennæ separated and upperwings completely dropped behind hindwings. On least arousing, upperwings are somewhat raised from behind hindwings, and the antennæ, separated, are slightly raised at same moment.

Then (vi.) there may well be special attitudes of apprehension of differing degree; and without observation it is impossible to say that the attitude of sleep is, or is not, closely approached by positions produced by other conditions, and different species will vary, closely allied species *may* vary. As regards such attitudes of apprehension, definite movements of wings may be noticed in *E. ianira* ♀. Sitting more or less near the ground, and more or less outspread, perhaps courting, or calling, or perhaps sunning herself, she is aware of some movement around.

With a clap her wings are closed over her back, and before long she is heeled over sideways, usually immediately. Sometimes an interval occurs, but often, especially after slighter movements from the first, in a partial exposure of coverable underside of upperwing, the white-centred black "eye" (from the brightest brown colour she possesses) "peers" over the hindwing margin. This position is sometimes maintained for a long time; but the least repetition of original fright occurring, the "eye" is whisked instantaneously behind the hindwing.

The irresistible conclusion is that, with the comparatively unnoticeable remainder of the ground-coloured underside, the eye-spot on its bright ground colour would so act that if some enemy had "gone for" the butterfly in that sudden movement, it would have aimed for the eye-spot, and but little essential harm would have accrued to the *ianira*. That position "with the eye out" one would consider as the "warned" or "apprehensive" attitude of *ianira*. The second might, or might *not* be, with the exception of the antennæ, that of sleep, but I think this "attitude" of seclusion—lying on its side—is *not* the sleep attitude of *ianira*. I know that *S. semele* does *not* sleep on its side, a favourite "resting" and "sunning" attitude.

In passing, one may note that possibly the attitude with "the eye out" may be quite sufficiently noticeable by a male to lead to his pause in passing, and certainly on the least such pause the female is liable to immediate further display, it may be, of whole upperside. These attitudes are full of interest, especially where, as in the female *ianira*, they may be evidence of reconciling two apparent incompatibles, protection from enemies, and display to opposite sex.

One cannot resist the connection in one's mind between the size of the crypton and its homology (or want of it, in colour and markings), as compared with rest of underside in some allied species. In *Pyrameis* (=*atalanta*, etc.) the crypton is large, and the full possible extent of upperwing is covered by the lower in sleep; the crypton, in fact, is additionally (as compared with upperside) ornamented with blue, with white, with ochreous. In *V. polychloros* much of upperwing is exposed in sleep; the crypton is small and homologous to rest of underside.

One wonders whether differences in attitudes of sleeping, resting, etc., account for the varieties in *Papilionidae*, in the want or presence in undersides, of similarity in whole, or in parts to uppersides.

The habit of *P. ulysses* to rest at the back of large leaves on retiring for the night, suggested that a bright blue crypton, resembling upperside, would be very conspicuous, as it may settle several times on leaves for lengthy periods with wings closed, but showing almost maximum of upperwings before settling to sleep, as if giving several trials. Its homologous crypton rendered it very little conspicuous in the shade, a complete contrast to *P. machaon*, etc.

So that taking such considerations up (partial as the observations are) the crypton becomes greatly more interesting. And still more so when one finds such a fact as occurs in *Ogyris amaryllis* when on the grays and browns usual to the undersides of the group touched with blues on the crypton (blue with black and white constituting the rest of the colorings of the genus), there occur two brilliant scarlet marks on the crypton, and in the ♀ only, a ♀ which is concerned with ova of the greatest possible value to green ants, then our interest is certainly not diminished.

Whether homology of crypton has connection with advances in evolution, or whether being an outcome of certain habits, through the lack of such habits, there has been no advancement to an heterologous crypton may well be considered open to discussion. But even in absence of any decision on this point, undoubtedly the slightest study of undersides, and perhaps especially of the crypton, must heighten one's impressions as to the importance of the characters of the undersides.

But one observation bearing on the point emphasising the possibility of the crypton having especial lessons has been partially referred to under the *Vanessid* remarks. In *Aglais* (=*urticae*, etc.) the crypton is not only not nearly, in effect, of underside *or* upperside *colour*, but does not carry the same markings. Some portion of the band marking of *polaris* is much more common on underside than on upperside in this country, *i.e.*, is more nearly fixed on underside, but the spots which *ichnusa* lacks are seldom, if *ever*, present in crypton, as compared with uppersides, even where they are enlarged and touch on uppersides.

But before leaving the subject of the crypton, a passing note may be made here as to its greater liability to variation as compared with phaneron, at any rate in some groups. As in *Polyommatus phlaeas* and in *Lycaenids*, for instance, if statistics were compiled of all possible observations of all variation of undersides year by year, aberrations of the crypton, I am confident, would be found to be much more frequent than those of phaneron on homologous undersides, and probably as in *A. selene*, *A. aglaia*, *Melitaeids*, and many others, notable variation would be found commoner in heterologous crypton than in accompanying phaneron, with extended field observations.

One firmly believes that in some reputedly rarer aberrations, *viz.*, the ab. *antico-obsoleta*, of *L. corydon* and certainly of allies, as compared with ab. *postico-obsoleta*, even the reverse may be truth, but that to determine the presence or absence of variation of the crypton of *Lycaenids* is a matter of extreme care or patience, and practically demands the use of chloroform to be exhaustive, whereas the determination of aberration in phaneron or its absence is a matter of the minimum amount of observation. And at all events some obsolescence of spots on upper wings as of basals in *Agriades* and *Polyommatus* as far commoner than obsolescence in hindwing is probably a generally recognised observation.

In the consideration of undersides, such species as show such designs as in this country do *E. hyperanthus* and *C. darus* var. *rothliebii* require special consideration beyond the particular amount due to each species, and as a suggestion is offered the following series of observations on *rothliebii*.

The protective value of the underside of *rothliebii* is especially noticeable. In the case of the most southern forms the underside renders the identification of the settled imago extremely hard, usually impossible, when it is resting as is its (?invariable) habit in clumps of heather, *Calluna*, or sometimes *E. tetralix*. The brown colouration of phaneron, as well as the ringed eye-spots, are evidently better suited to match the heather than would be the plain gray wash of the northernmost forms. That on different mosses in Westmoreland,

separated by but a few miles and a mountain range, and with little difference in elevation, the brownest and best spotted undersides occur on the most thickly heather-covered moss, may be a coincidence. It may also be a coincidence that on Scotch mosses of which I have knowledge, the grey-green lichens, which are unnoticeable on the above Westmoreland mosses, form a much more conspicuous feature where the Scotch form, with little or no spotting on the green-grey hindwing, occurs. Whereas in the same family—in *S. semele*—where great variation in narrow limits (as in most of the allies) must be conceded to the underside, the effects of such amount of melanism or albinism as occurs, or of other alterations of colour and markings (unless of exceptional rarity), is not nearly so subversive of the usual effect as the common variation of the upperside, and where the exceptional aberrations occur, the same is true still in a less degree. And the phaneron would appear to be in these cases more variable than the crypton. But these examples are greatly in the minority probably among all species, so much so as to allow it to be stated that of all aberrations subversive of the usual effects as produced by the prevailing forms, those of the crypton are far more common than those of the phaneron in butterflies generally. Probably the same statement would hold true, without qualifications as to the subversive character of the variation. Such variations as are common in phanerons, may eventually prove to be adaptive to surroundings entirely. In *S. semele* the whitest forms of phaneron have been obtained in same locality as var. *mundata* of *G. obscuraria*, and the race here of *semele* tends to a usually white underside very strongly. And in justification of this belief, which leads one to definitely suggest that undersides have suffered some fairly general neglect in the field, in the cabinet, in philosophizing, a few reasons are given :—

In the field how far do we find records of attitudes of more and of less complete rest and such, and of feeding, of sunning, of courting? And without knowing these well, is it possible to understand about the various patterns of undersides? Then their appearance from below and from above? The comparison of the highest sitting ♂ s of *L. aegon*, as compared with dark undersides of ♀ s seen from below against the late afternoon and evening sky, when they rest for sleep, is most suggestive.

In the cabinet is it libel to say that mostly (in however long a series, 10, 30, 70, etc.) it is a common thing to see two or only a few more undersides unless obvious aberrations? How many collections show the appearance of the insect in any form of rest or sleep? One would have expected at least one wholly closed.

In philosophizing, how few of us can forget the wonder caused by the first appreciation of the contrast between the upperside and underside? Have we maintained any adequately equal interest in the underside? Have many of us bestowed more than passing thought, on the really more marvellous contrast on one single wing in its two parts, *viz.*, the underside of the upper wing of, say, *P. cardui*, with the basal part even more beautifully tinted than the upperside, and the apical part with the effect of the underside of the underwing represented in a still more subdued degree, *viz.*, of the crypton and the phaneron? And as to descriptions, one offers two examples only :—

I. *Vanessa io.*—In Tutt's first volume on butterflies, *io* has no underside—not by description. In Newman's butterflies, the description runs "the underside is jet black."

II. *Argynnis selene.*—Even hints of sexual dimorphism in their undersides are non-existent, or wholly vague and by inference. There is marked sexual dimorphism in many localities—probably unfailing—only in some localities (as Ashdown, where dark brown markings may obscure it in the ♀) occasionally obscured by racial characteristics in some individuals.

If one accepts the prevailing condition of affairs, *i.e.*, until the present work of Tutt now in issue, we have to accept it as showing that the underside is considered to be much less worthy of observation than the upper—it is of subordinate importance. On the contrary, one is disposed to think that as looked on from the point of view of the furtherance of existence and of propagation of the species, to the butterfly the underside markings and colouring are more important, as a general rule, than the character of the upperside markings. That is to say that, in the upperside, departures from the normal of the markings of butterflies are less likely to result in disasters to the imagines than equally decided aberrations of the underside, or adherence to prevailing forms is more essential in the case of the underside than of the upperside.

Restriction of variation of undersides.—Any attempt to determine whether upperside aberrations are more common, more wide-reaching, more subversive of the usual general effect than the underside variations, has to be abandoned for want of time and other essentials. One's impression is certainly that on the whole it may be said:—On underside, aberrations are less common than on upperside (*e.g.*, especially *M. aurinia*). On underside, aberrations are mostly insignificant as compared with those on upperside. On underside, profound aberrations are much less common than on upperside. On underside, aberrations are much less diverse than on upperside. A further point is that aberrations of the underside would probably be found to be much more generic, *i.e.*, of closely parallel characters in allied species, than is the case of upperside, where aberrations would be found to be more occasionally parallel.

These impressions may well be useless, being based upon confessed insufficient attention generally to undersides. It is well open to discussion, and there is plenty to be said. But certain instances can be given:—Polymorphic species with great variation of uppersides show subordinate polymorphism of undersides usually, so far as one has been able to follow them out, *e.g.*, particularly *Hypolimnas bolina*. Some with less polymorphic variation—*Neptis sheppardii*, green shades and white—scarcely exhibit the variation on underside. On the underside of *sarpedon*, the effects of the polymorphism are much less noticeable than on the upperside. In our ♀ *Lycaenids* the variations of the undersides fail entirely to equal in the impressions they give the effects of the variations of the uppersides. The same is true of *Colias*, etc., etc. So far, when one has in any species appeared to find equal extent of identical variation in any particular on upper- and underside, this variation has usually resolved itself into a patho-

logical condition (bleaching, etc.) more often than not. But it is possible to go further than this with mere casual observations.

Restrictions of underside aberrations.—Casual observations give rise to impressions which lead one to examine on one or two different lines. First in the crypton. This shows extremely frequently the only variation in the specimen as regards the underside, whether in homologous (*Lycaenid*) or heterologous [*Argynnid* (*Brenthid*)] undersides.

To take a familiar group, the *Lycaenids*:—Melanotoxy and some striation (in some periods and places at any rate) are abundantly common on crypton more than phaneron. That a similar statement would be true as to the variation of crypton in *Brenthids* (*euphrosyne*, etc.) (aberrant as to melanic and obsolete uppersides) being more common than of phaneron, one is not prepared to urge too strongly, but as to the less variability of undersides in *Brenthids*, one has a few definite details to offer. Indeed, it seems quite a notable point.

In *euphrosyne* and *selene* aberration of the underside is sometimes limited to the crypton. But it may be quite as limited on other lines. Whereas with a fairly or even fully developed recurrent form of aberration one may easily obtain access in collections to, say, fifteen or twenty with fairly normal undersides for one example with any aberration of the underside at all comparable in extent with upperside—it is only in the most replete collections that the latter occur, except singly, *viz.*, aberrations of uppersides with notably aberrant undersides, and some even of the most advanced upperside aberrations are accompanied by normal undersides, just as though a marked aberration of an upperside was more readily produced than as marked an one on the underside. One specimen only has one seen or heard of, *viz.*, *euphrosyne*, with marked underside aberration and normal upperside. And further, the marked aberrations of English *Brenthids* fall into four quite distinct categories, or rather five, inasmuch as two or more of the forms may be mixed in an aberration.

The forms are (1) confluence of extended markings; or such transposition and alteration (2) as to form a new pattern of markings; and (3 and 4) totally opposed conditions, disappearance of many or all of markings, leaving ground colour, more or less, only on the one hand, and melanism or such extension of black markings as largely or wholly to efface the ground colour in parts or on the whole wing.

With all these varied forms of upperside variation, with only one exception, has one ever seen more than one single form of underside variation, *viz.*, more or less extension of the silvery markings over the interneural spaces, with the darker markings more and more obsolete to full degree of being represented only by dark and widened neutral lines. The one exception is a *selene* in the Webb collection, largely melanic on underside as well as on upper.* Moreover, with all the variations in aberrations of *selene* and *euphrosyne*, the underside aberration described above is common to both species.

On the undersides of *selene* and *euphrosyne*, with the exception of sexual dimorphism, but little variation (and not commonly) exists; in minor degree an amount of darker colouring, and of black outlining,

* In Tutt's butterflies is figured an ab. of *selene* showing on underside some extension and confluence with border of extra median row of dark spots on hind-wings, making two forms of underside variation, besides the general melanism.

and of extent of silver occurs, beyond what is noted above, whereas the uppersides are commonly decidedly variable. Whether or no one's experience is exceptional, it remains that three or four or more quite different forms of upperside aberrations in *euphrosyne* and *selene* are accompanied by normal undersides fairly commonly, and when accompanied by underside aberration, of any extent, the latter is almost always of one form only, in differing degree.

Mutiple adaptability of undersides.—The extraordinary contrast between frequency and variety of advanced upperside variations, and the rarity of underside variations, and their want of scope may be much more marked in *Brenthids* than in other butterflies, but I think, especially if the crypton be excepted, there is no doubt that it is true in differing degrees of all, or nearly all, butterflies; and this suggests the importance of the view that aberrations of the undersides, as well as the usual forms, need to conform to the surroundings, or at least to be in harmony. This conception having led to several years of steady collecting of especially, among many species, the undersides of "blues," has led to the conviction of the multiple adaptability of each underside to many forms of surroundings in their habitats when the imagines rest or sleep or attempt elusiveness. The *Lycaenids* of chalk, for instance, resemble grass blooms, plantain heads in bloom, chalk patches or gravelly patches, and many other natural objects; *aegon*, especially in the ♀, is protected by resemblance to heathers, last year's blooms, old "knob heads," rush blossoms and many forms of soil, and many others as *A. selene* and *M. athalia* excellently resemble bits of dead bracken, heads of plantain past their prime, dead blooms of *Floscculi*, tufts of *Melampyre*, blooms of rushes, etc. The colour of *S. semele* is adaptable to simulating many soils, and limestone, and chalky patches, burnt juniper, and dead wood or living bark. I remember once, on top of Folkestone hills, noticing the outline of a *semele* against the sky as it rested on the post of a wire fencing, and on careful searching found numbers of pairs on other posts and single specimens, all excellently protected by their underside coloration, except when seen in profile against the light of the sky of the North.

The similarity of undersides of *L. argiolus* to bright reflections from Holly, Ivy, Portugal laurel, Rhamnus, Dogwood, and Privet seems almost to suggest its association with these plants from this circumstance, and that secondarily, it uses them as larval foods; *argiolus*, moreover, is by no means too conspicuous on pale and white blooms such as bramble blossom, which it loves.

The underside of *S. malvae* resting excellently reproduces same effect as dead plantain heads or knob heads, or old fruit of solitary thistle and grass and rushes; in fact of living and dead herbage generally and varying soils.

And to take one more familiar example, the likeness of *A. cardamines* resting, to cow parsley blooms and buds, is easily extended by observation to Cardamine bloom, Alliaria, and many Cresses in bloom, Dandelion "clocks," and young shoots of Umbelliferæ. Perhaps the fact that cow parsley is so well known and often quoted in this matter, is a proof that others are a greater protection because less often "found out" in their protecting action. In fact, one wonders

whether it may not be that the best "protections" are not found out at all. Those known, however, are wonderful, and wide enough for our admiration and good testimony to the importance of "undersides."

Contrast development of uppersides.—This may seem a mere one-sided panegyric on the underside unduly separated from its association with the upperside, but a growing appreciation has rendered one more than suspicious that the main use of undersides—protection—may partly owe its capabilities or success to the uppersides having developed on lines of contrast, in many cases. This is strongly suggested by such forms as *Thecla rubi*, *Lycaena bellargus* and allies, *P. ulysses*, the *Delias* and other *Pierid* groups. The bright colour having its display on the upperside, but often the upperside having a duller contrasting colour, are opposing arrangements which strongly emphasize this. If the underside has been determined by adaptation to dull, or bright coloured surroundings respectively, or with bright warning colours, the chances of escape may easily be enhanced by a contrast forming on the upperside in two ways at least.

(1) *In elusion on settling.*—Thus the blue *L. bellargus* ♂, by a sudden closing of the wings, becomes not recognisable as the same insect, if as an insect at all, and therefore not liable to be identified as the object of the chase—without a process of education, which may never be completed in the foes. The greater the contrast, the less is there any preparation to foreshadow the unexpected and detract from the "quick change" process. The protective undersides, if this is true, have an enhanced value for resting insects, through the development of a brilliant upperside. And the same result is obtained by the opposite process of a nearly black upperside contrasting with the protective bright underside of *T. rubi*.

(2) *Co-operation of upperside and underside.*—A process of true co-operation is suggested. Such is shown by such a butterfly as *Delias nysa* with a black underside and white upperside, which, when flying, represents a third brilliant silvery butterfly—a "fusion" effect. These contrast butterflies, *bellargus*, *rubi*, etc., are much less conspicuous when flying, and much more difficult, on account of variations in appearance, to follow in their flight than the others, which one may call repetition-surface butterflies, *e.g.*, *G. rhamni*, English *Pierids*, *E. cardamines* ♂, *P. machaon*, etc. The comparative ease with which the sight can follow the latter is familiar to all. Even an almost black butterfly, with repetition-surfaces, in flying suddenly into shade, is much more easy "to keep one's eye on" (let alone a *Pierid* or other repetition-surface) than is a contrast-surface butterfly—*T. rubi*, *D. nigridius*, etc.

In this co-operation process many species may more or less be included, perhaps, but one tends to overlook the details of one's experience often in one's anxiety, and success or disappointment in attempting to avoid co-operating oneself in the insect's escape. But we can easily recall one instance in *Ino statices*, and recollect how comparativeiy inconspicuous it is rendered in flight, by the co-operation of the gray hindwings and duller underside, than when we depended for recognition solely on its metallic upper wings' colour undiluted, *i.e.*, "unfused."

I am afraid this is only making the matter worse, and subordinating the upperside to the underside with a vengeance. However, no doubt coming to further consideration of uppersides, they can easily be re-established in at least a position of equality.

Sexual dimorphism.—Now, further, in the development of upperside while supporting its claims to one element, the purposes of recognition (similarity with its own species and distinction from other species—more or, often, less successful), still another possible element must be mentioned as having claims, *viz.*, protection of still further extent. Protection and recognition may be in combination as may loosely be said to be the case in mimetic arrangements, or they may be in apparent opposition, as would probably be considered usual, as that the brilliant blue of ♂ *L. bellargus* might be said to be an upperside danger. But this action may be less usual than we might think, though at the outset, where we have to grapple with the phenomena of sexual dimorphism (a striking, widespread, apparently nearly inevitable necessity), it might seem that, with the exception of warning colours (so lately understood) as at present limited, any sort of display could not be an immediate protection as opposed to the co-operative or mediate action before mentioned. But we must concede to the ♀s the advantage (necessity?) of being able to be inconspicuous when "on the lay," or "on the sit," or copulated (birds, beasts, reptiles, fishes, etc., as well as lepidoptera). Obviously, a long-tailed Japanese cock, a ♂ pheasant, or a peacock is ill-suited to sit in seclusion. A ♀ *bellargus* has to be attending to business which carries her from place to place for distribution of ova. Inconspicuousness may, in different branches of the animal kingdom, be due to differing details of procreation. Conspicuous male parents may absolutely aid by diverting attention from the ♀.

The ♂ *bellargus* may divert attention from the ♀ in the act of separation when disturbed or at the end of copulation, and even in copulated *bellargus* in occasional involuntary flight, if the male colour shows, it may divert attention by its virtue as a warning colour, as will be suggested later, whereas in frogs and toads, with prolonged periods of exposure when copulated, sexual dimorphism is at a minimum.

Conspicuous ♂ parents in birds may readily divert attention from the ♀ and the nest, and are as we know, in some species, replaceable by batchelors, more readily than are the eggs or young.

The sexual dimorphism in butterflies means, of course, added safety to active hunting ♂ in ready recognition of sex of widespread unimpregnated ♀ waiting. And also added safety, in times of possible danger, to the ♀ in ready recognition of passing ♂, so that neither need unnecessarily expose themselves, in doubtful times, for want of sex-recognition. The inverted sexual dimorphism of *janira* may be a source of safety to the ♂s, which are much hunted at times by swallows. But, though of course the usually less brilliantly coloured ♀ is generally considered to be in a condition of greater safety from this fact, and from her retiring, slinking, quieter habits, are we justified in assuming the amount of danger which is usually attributed to the brilliant colouring of a male—such as *L. bellargus*, *i.e.*, in assuming it to be an unmitigated danger.

In the co-operative action, one has suggested it as one source of safety. But further, if one is correct in thinking that one undoubted fact may be possibly extended by further observation, the bright colour may be a further source of safety.

This fact is that some enemies of insects, *e.g.*, wasps, have been observed to select as their prey the ♀ s, to the exclusion of the ♂, except at times when copulated; and they, with this exception, with any sort of supply of ♀ s, pass over all ♂ s. If this preference tends at all to hold good for other insects, and this, presumably, preference for eggs extends at all to sexually dimorphic insects, the brilliance of the ♂ s may definitely aid in the saving of them from foes in search of eggs for breakfast, and one more than suspects that this is the case with some such foes as crickets, grasshoppers, dragonflies, at any rate, at some times.

This supposition is not contradicted by such things as the marked cases like *E. cardamines*, where, except in breeding, ♀ s are as compared with ♂ s immeasurably "rarer," *i.e.*, are less often recognized in a wild state.

This scheme, of the ♀ tending to the obscure, and to hiding, and of the ♂ ever tending to obtrude its non-femaleness, falls into line with much that is known of mimicry and polymorphism. Where mimetic phases are adopted by the imagines, if the ♀ and ♂ do not alike mimic, which is very commonly the case, the ♀ s are generally those that mimic one, two, or more other species for protection. Thus in the case of *Hypolimnas bolina* the ♂ is fairly constant, while through Australia to India, and many other countries to Mauritius, the ♀ s have many forms in one country even, as Australia, and various forms in different countries.

The mimicry of *Danaids* by *Hypolimnas misippus* ♀, in a single form, so entirely different from any ♀ of *H. bolina*, while ♂ s are not so greatly unlike, points to special development in the ♀, the ♂ merely needing to be quite different from the ♀ s. The ♀ of both in North Queensland needed especial attention to secure series, whereas the ♂ s flaunted commonly. These facts do not seem strongly in opposition to the idea of ♀ -hunting enemies.

This idea, that the bright colours of such ♂ s as of *L. bellargus*, *E. cardamines*, or the duller colours, as in *E. ianira* of the ♂ s may be to some extent a warning colour to tell the foes that they are not ♀ s, does meet one supposed difficulty, which really includes two questions. This difficulty concerns the apparent enormous increase of dangers accruing from brighter colours of ♂ s which are not distasteful to the surrounding enemies. This difficulty is, however, perhaps considerably reduced by its separation into the two questions which one's personal beliefs answer in the negative: (i.) Is the prevailing idea of greater prevalence of ♂ s as compared with ♀ s correct? (ii.) Is it correct to consider, as is largely done, that the life of the ♂ is considerably of less importance than that of the ♀ in the matter of *continual* preservation from enemies?

The converse of both these ideas is supported by some evidence. Thus a proposition is advanced that the number of ♀ s, on the whole, is not greatly, if *at all*, generally inferior to that of ♂ s., Indeed, in some cases, in whole families the ♀ s have been known to be

greatly in excess. Even in some few species the repeated excess of ♂ s over ♀ s, *if proved*, is no index, for other species, to be assumed as evidence.

In the case of *Hypolimnas misippus*, extraordinary and incomparably greater prevalence of ♂ over ♀ was observed on N. Johnstone river, Queensland, an undoubtedly protected female resembling an extremely distasteful *Danaid* (? *Petilius*), also uncommon. On the contrary, the ♀ s of *H. bolina* for six months were almost as strikingly observed more often than ♂ s, which suggests a possibility merely of ♀ s being more restricted to localities suitable to breeding grounds. In the cases of comparative rarity of *A. cardamines* ♀ s established as a repeated observation through many years, breeding, which, on the whole, nearly or quite equalises the numbers of the sexes, is reducing the number of conformances to any possible rule of ♂ s exceeding the ♀ s, and reducing them continually, and the same is true of *L. argiolus*.

It would be quite as fair to represent the apparent commonness of the ♂ as an index of its greater exposure to a possibility of danger.

1st.—Because of its greater brilliance or noticeability.

2nd.—Because of its extended activity.

3rd.—Often because of its greater pugnacity.

Then the conclusion one arives at is that the ♂ s are greatly more in evidence, but not greatly commoner than the ♀ s, is a rule generally for all species, or almost all. The probability is, as is here held, that the ♂ seems more common because of its conspicuous activity, etc., exceeding that of the ♀, and that if more exposed to danger, it is just as able to escape from danger, also because of its greater conspicuousness and activity, through the "quick-change" process being so marked. And a second proposition is advanced, *viz.*:—That the life of the ♂ requires protection and preservation as much as (or almost so) that of a ♀.

(1) Because ♂ s are on the wing (it may be many) days before the ♀ s are more than quite scanty.

(2) Though it is argued that, the functions of the ♂ once over, he is no longer wanted, undoubtedly, with a single copulation, many eggs laid are infertile (the later) in captivity, and in some species, *e.g.*, *L. bellargus* and *L. corydon*, even as it is, sometimes their function never is finished, the last excluded ♀ s are never fertilized in some seasons, there being no ♂ left to impregnate. More observation may show this to be more nearly general than can be here stated, observations referring only to *L. icarus*, *L. bellargus*, *L. corydon*, *H. comma*, and *A. aglaia*.

(3) The similarity between the undersides of ♀ s and ♂ s in each species does *not* point to any lack of necessity for protection of ♂ s as compared with ♀ s. The sleep aspects of ♀ and ♂ *A. cardamines* are precisely similar.

The numbers of each sex being at all nearly equal, inadequate protection and survival of males must lead to an insufficiency of a considerable extent.

(4) The observed copulation of ♀ s with battered ♂ s shows value of preserved ♂ s. The reflection naturally occurs that we may be holding exaggerated views of the dangers to tasteful imagines of their brilliant colours and contrasting designs.

But observations reimpress on one's mind the rapidity, on any disturbance, with which the cessation of flight (to escape) is accompanied by the closing up of all bright colour in most, and where the habit is not to close, other details show themselves; as, emphatically, in *Casiapa porphyrops*. In every case in North Queensland where observation succeeded in tracking this *Hesperid*, in its flight and settling, when it whisked itself into the bushes, it was observed spread out flat (and it measures 2½ inches in expanse) on some low leafly herbage about three feet from the ground, and its strong contrast of indigo, with old gold bands, was protected from our view, from its situation on the undersurface of drooping large leaves whose backs looked downwards and towards the shade of the jungle, and away from the roadway. This is true of the hours of sunshine (11 a.m., 2 p.m., 3 p.m., 4 p.m.) on separate occasions.

It is not, of course, suggested that no other agency works for development of brighter colours. Mimetic adaptation is scarcely touched in these remarks. Nor is inheritance in sexually dimorphic species, of colour of one sex, by opposite sex in next generation. Sexual selection by ♀ s of brighter ♂ s is another possibility. But this last is, in some species, so easily proved to be an extremely remote "possibility," an often evidently non-existent fact (except in so far as in some competitive cases, the successful ♂ is an old roué, and may have been, by virtue of his superiority, at one time incomparably brilliant) that other factors must eventually be found to have been masked by this imaginative role.

But it is here suggested that whatever agencies have contributed to commencements of appearance of bright colours, their perfection is being influenced by:—(1) Recognition between sexes. And the possibility of this by colour was proved by Briggs' experiments. in which ♀ s even were proved to be attracted by colours of ♂ s artificially represented. (2) In the case of elusion by the increasing difficulty of identification (in the quick change process) with increasing contrast by brighter upperside colours. (3) Protection of ♂ from ♀-hunting enemies.

One argues from the views expressed in these remarks that the usual view of comparative significance of upper- and undersides is erroneous, that it might be fair to hold that, in such measure as the markings and colour of the uppersides of our butterflies determine the success in life of each individual, in exactly such do also their undersides, and determine consequently the continued existence of the species.

But if one surface has to be sunk to a lower level, it is contended that the upperside is the one to which the subordinate position should be assigned, instead of the usual much less regarded underside.

At a Council Meeting held on February 1st, 1910, it was decided (with a view to ensuring uniformity of nomenclature) to adopt South's List as the basis of the Society's Minutes, etc., etc., pending the publication and general adoption of a similar standard list embodying the many more recent, and more or less piecemeal, revisions of nomenclature.

In the case of the above paper, and also of that following it, however, it has been found impossible to bring the nomenclature entirely into line with South's List without obscuring the author's meaning; in order therefore to avoid this, certain names, indicative of the modern subdivisions of such large groups as South's Vanessa, Lycæna, etc, have been allowed to stand.—Ed.

DISCUSSION re. THECLA (CALLOPHRYS) RUBI.

(Opening remarks by Dr. T. A. CHAPMAN, F.E.S., November 16th, 1909.)

As the few remarks I propose to make are by no means intended as a paper, but merely as indications of points for discussion, either to obtain the information upon them that members may already possess, or to obtain suggestions as to the further observations that appear to be necessary to increase our knowledge of the species, I shall throw the few items that I lay before you into the form of questions wherever possible, rather than into statements that would imply that the subject was already fully elucidated.

Our palæarctic Lycænids divide themselves very readily and naturally into the three divisions of coppers, blues, and hairstreaks. Some exotic forms, however, leave these divisions less crisp, and suggest some further ones as necessary. *Thecla* (*Callophrys*) *rubi* is clearly a hairstreak; this may seem so obvious that it is like gilding refined gold to elaborate it. Yet it is the case that it has certain affinities with the blues, not sufficiently marked, perhaps, to make us suspect it as posing as one of the connecting links I have alluded to, but enough to be of some interest. The most definite of these is, perhaps, the egg. One might place this almost in the same genus with *Polyommatus* or *Agriades*. On comparing it with these, one would not suspect it of belonging to a different division of the Lycænids. Another point of affinity is that the larva possesses a honey-gland apparently well-developed, probably functional. At one time I was inclined to believe that the possession of a honey gland by the larva was a character that might be depended on to separate the blues from the other Lycænids. This is clearly not so, and there is, indeed, at least one blue that does not possess this gland. *Thestor ballus* and *Callophrys avis* also both possess this gland.

Mr. Tutt is inclined to regard the habit of passing the winter as a pupa as a Lycænid character; this may be so, but Lycænids pass the winter as eggs, as larvæ, and as pupæ. Our other British Theclids are so closely related to each other that their all passing the winter as ova cannot be fairly generalised as a dominant fact amongst the Theclids. Indeed, of the six genera into which Scudder distributes the Theclids of the eastern United States, two pass the winter as eggs, but four as pupæ.

There are several points in which the Thestorids are so unmistakably Theclid, that they could easily afford to have more characters in common with the blues without endangering their claim to be "Hair-streaks."

One of the most curious of these is a peculiarity of the pupa. Lepidopterous larvæ possess spiracles on each of the first eight abdominal segments. Practically all lepidopterous pupæ have the first of these hidden away beneath the wings. The next six are present and functional, whilst the last, that on the 8th abdominal segment, is obsolete, though usually represented by a scar.

In the Theclids, and though there are in all probability other instances, I have not yet met with one, the spiracle of the 7th abdominal segment is obsolete, precisely as is that of the 8th.

This is the case in *Callophrys* and *Thestor*. Another peculiarity of the Theclids is in the ancillary appendages. The *Edeagus* is very long, and this is related with a very long chitinous tube in the 8th abdominal segment of the female to receive it.

Another Callophryid character not uncommon in the Theclids, absent in the blues, is the existence of an androconial brand on the male forewings. This is far from universal in the Theclids, but I cannot call to mind an instance of its presence in any "blue."

I ought, perhaps, to have alluded to the habit as to foodplants of *T. rubi* (and *Thestor ballus*), as affording indications of affinity with the blues, that is their preference for papilionaceous plants, and in the case of *T. rubi*, especially for the flowers.

The blues, as a group, are certainly associated by all of us with papilionaceous plants, whilst the Theclids are not, but it must be admitted that the number of species on which we found the generalisation is possibly inadequate. A great many exotic blues, however, patronise this natural order, and I don't recollect any other Thecla that does so.

It is, perhaps, appropriate for one to introduce the subject of *Thecla rubi*, since I have lately been considering it in several aspects in connection with *T. avis*, the new species of the genus that I was lucky enough to capture and recognise. Still there is some difficulty in finding anything novel about it or requiring discussion, since it has been so recently presented to us with so much detail, to which I have added my share, in Tutt's 9th Vol. of the *British Lepidoptera*.

Still, there are a few points in which there is reason to suppose that more light might be thrown, and in any case, it is always a most welcome little butterfly, as being one of our early spring species.

We probably know nearly all that is to be known about its variation, regarded simply as variation, but its variation in relation to its distribution is a subject on which our knowledge is, to say the least, very crude. Beyond a general idea that the blacker forms are more northern, the redder more southern -forms, little or nothing has been formulated. Last spring I took at Amelie-les-Bains, a number of specimens, all of which belonged more or less to the reddish var. *ferrida*, but a few weeks later at Vernet-les-Bains, at 1000ft. higher, but only some twenty miles off as the crow flies, all the specimens I saw were of dark, that is northern, facies.

Then as to the spotting of the underside, what advantage accrues to the insect from having these spots fully developed and conspicuous, or under what circumstances is it more useful to the insect to have the spots reduced to one only, or even have them entirely absent. We know that one of these two extremes often predominate in particular localities, that sometimes the local race is fairly uniform, in other places no two specimens are quite alike. Col. Swinhoe showed me a series taken at Arcachur, in which the white spots were well, even strongly, developed, and in nearly all the black scales that accompany the white spots were especially conspicuous.

I may note before passing on that these dark scales are always present in *rubi* (except, of course, when the spots are absent), yet I have seen them referred to in none of the published descriptions of the insect that I have looked up.

The question of this variation in the row of spots, how far it is vague and accidental, how far it characterises certain geographical races, and what are the causes that determine the presence or absence of the spots in any particular race, is, of course, intimately associated with another point that we are equally ignorant of. That is, what is the use of these spots. Whether these problems must be dealt with together, or whether one can be first understood and then the other more easily investigated, we know not.

As regards the use of the spots, we may begin its study by picturing the evolutions of Mr. Prout's tame specimen, which so kindly allowed him to observe its persistence in orienting itself in its special manner. The butterfly does not so frequently settle on flowers as on green leaves, and when it does so moves at once into a position with the sun shining vertically on one side; certainly for basking, but whether also for hiding, is not quite clear. It is not very obvious that it would be more conspicuous or less so, exposed in this way to the sun, than if it sat at any other angle.

At any rate it is in this position that the row of spots will be most obvious, and, therefore, probably that they are then most useful, whatever that use may be. The settling amongst leaves certainly points to the green colour being for concealment. We must suppose, therefore, that the white spots in some way assist this object. It is well known that a large area of one colour is more conspicuous than a similar surface broken up by varied colours and marking, and the white spots will no doubt have some such effect. It seems also possible that they may give much the same effect in bright sunshine, as the sparkle and shade of the serrated margin of a leaf, such as that of blackthorn, on which the butterfly often settles, the serrations being often convex or bent over, and at a different angle to the main surface of the leaf, and therefore, when they reflect the sunlight, the general leaf surface will not.

Assuming this to be so, the greater or less development of the spots, or rather *per contra*, their more or less complete obsolescence, might be correlated with the plants the butterfly has, in any particular locality, as resting places, whether, that is, they have leaves in which this effect occurs or not. This is entirely speculative, but suggests items for field observations, though perchance such observations have already been made by some member of the Society.

Another peculiarity of *T. rubi*, in which it is alone amongst British butterflies, is the possession of tails, which take a special attitude when the insect is at rest. There are many exotic species, of which Mr. Kaye, I think, once exhibited a very pronounced example, in which these tails take the appearance of an animals head with eyes—to the probable discomfiture of enemies. Are these tails in *T. rubi* merely the degenerate remains of the more perfect display that belonged to some tropical ancestor, or are they still useful in this or some other way? One would suppose that before such tails could become so elaborately mimetic, as in these exotic forms, they must have been fairly well-developed, and with some other use. Is *T. rubi* still at this stage of development, and what is the other use?