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EDITORIAL

By the time this issue of the *Bulletin* is published the United Nations Conference on Science and Technology for Development (UNCSTD) in Vienna will have ended. The recommendations that will come out of the meeting are certain to have an impact on the way in which the federal government channels its aid to countries of the Third World. The minister responsible for CIDA announced for instance that the agency will contribute 12 million of its one billion dollars budget to specific S & T problems and projects.

Although invited to present their views, suggestions and recommendations on UNCSTD scientific societies and scientists in general do not seem to have had a tremendous imput or impact on Canada's position paper. The most notable Canadian effort in preparation for the Vienna meeting was the symposium organized by the International Development Research Centre (IDRC) in Toronto on May 10-13, 1979, with the collaboration of SCITEC, the AUCC, the Royal Society of Canada, and MOSST. IDRC received a lot of praise, from various quarters, at the meeting, for the way in which it has so far contributed to Third World development. It also came out that there is tremendous trust in the developing countries for Canadians and what Canada can contribute.

The sharing and utilization of knowledge and skills between developed and developing countries is far from being a simple process. Even if we, as Canadians, enjoy a good reputation in Africa, Asia or South America it does not mean that our scientific know-how and our scientists are always being put to good use there. The best contribution we have made so far has been in the training of individuals in various fields of Science and Technology. But it is difficult to envisage a more extensive, more innovative input in research related to S & T for developing countries when we are already having such a hard time keeping our domestic efforts afloat.

Following the UNCSTD recommendations will our politicians do the right thing, the wrong thing, or the right thing in the wrong way?

BIOLOGICAL SURVEY PROJECT

Northern Contract

More than 2,000 references have so far been consulted by the Secretariat entomologist, in connection with the Society's contract, Review and Synthesis of Knowledge on Northern and Arctic Insects, that was outlined in previous issues of the *Bulletin*. About 1,500 references contributed useful ideas or information for the Synthesis, and about half of these qualified for inclusion in the planned bibliography because they deal with the terrestrial arthropods of North America north of tree line. Only a few hundred potentially significant known references remain to be consulted.

Many useful references were viewed especially in the library of the Arctic Institute of North America in Calgary, and in the Boreal Institute for Northern Studies, University of Alberta, at Edmonton, which were visited in June 1979. Helpful assistance is also being received from various Ottawa libraries, notably those of the Biosystematics Research Institute, the National Research Council (Canada Institute for Scientific and Technical Information), the National Museum of Natural Sciences, the Geological Survey of Canada, and Agriculture Canada (main library).

Many of the relevant papers in foreign languages, especially in Russian, are available in translation, or informative abstracts in English are available that reduce the need for translation of some works. Nevertheless, translation of several papers has been initiated.

Compilation of a checklist of North American arctic species has been delayed because most of the relevant data are available only in generic or family revisions of groups that occur north of tree line, rather than in lists of species collected from particular localities. Such revisions continue to be identified and consulted. The checklist currently includes 1,500 species of terrestrial arthropods. A few completed parts have been sent to taxonomic specialists for review.

Some illustrations for the first part of the Synthesis (background information) have been redrawn in final form; the remainder are being prepared from the draft originals. Most sections of the manuscript of this part have been completed ready for review. Several sections of the main (entomological) part of the Synthesis have also been drafted.

This progress will be discussed at the next meeting of the Scientific Committee, arranged for 25 and 26 October 1979 in Ottawa.

Cooperative endeavours

Projects in various parts of the country (see *Bull. ent. Soc. Can.* 11 (2): 37) appear to be running smoothly. Fieldwork for the Yukon project was initiated early in June and, despite collecting difficulties engendered by the high latitude, is producing useful material through the activities of half a dozen entomologists in two teams. Fuller information on this and other projects still in progress will be available for the Biological Survey report in the next issue of the *Bulletin*.

ADDENDUM

Membership list (December 1978)

Dave D. Chadee — Dept. of Zoology, University of the West Indies, St. Augustine, Trinidad.

REPORT OF THE ELECTION COMMITTEE TO THE GOVERNING BOARD OF THE ENTOMOLOGICAL SOCIETY OF CANADA

The Election Committee of the Entomological Society of Canada met in Quebec City on July 16 to open and count those ballots received by the Chairman before midnight, July 15, 1979. A total of 376 ballots were received. The following results were recorded:

For Second Vice-President.	(no ballots spoiled)	Wiggins, G.B.
For Fellowship Committee:	(no ballots spoiled)	MacGillivray, M.E. Madsen, H.F.
For Board of Directors:	(no ballots spoiled)	Friend, W.G. Proverbs, M.D.

For Honorary Member: The great majority voted "yes" for J.H.H. Phillips.

I hereby certify that the Election Committee counted accurately all ballots received, as indicated above.

R.J. Finnegan, Chairman.

Dr. M. Ellen MacGillivray received her Award as a Fellow of the Entomological Society of Canada at the Banquet of the Acadian Entomological Society in Bangor, Maine on May 9th, 1979. Dr. MacGillivray was escorted to the podium by Dr. Geddes W. Simpson. The Certificate was presented by Drs. James B. Kring (President ESA) and R.H. Storch (President AES).

ON SOME NEW TRENDS IN TRICHOPTEROLOGY

F. Schmid

Biosystematics Research Institute
Ottawa, Ontario

Go with the times, but watch in what the times are stepping
XENOΦO

In 1958, I wrote a critical analysis of two important, recently published papers, Ross' study on the evolution and phylogeny of mountain caddis-flies (1956) and Nielsen's researches on the morphology and anatomy of the male genital segments (1957). Both of these studies appeared to me sufficiently important to justify the publication of an analysis. As could be expected, that critique did not get the same audience in America as in Europe: *Gallicum est, non legitur*. It is why I had the following pages translated from French for publication.

Recently, two other papers have caught my attention, not for their immediate importance, since they deal with rather restricted subjects, but because they represent trends in todays trichopterology, and risk leading it on various wrong paths.

The venerable *disputatio* enjoys little favour in trichopterology, but I use it here again because it had proven itself sufficiently valuable in human sciences and can always be fruitful in mutual exchanges.

THE RELATIONSHIP OF THE GENERA OF THE AMERICAN HYDROPSYCHINAE
as indicated by Phallic Structures

H.H. Ross and J.D. Unzicker

Would it be an exaggeration to say that this paper is unique of its kind in trichopterology? Such an accumulation of unfortunate initiatives, wrong viewpoints and erroneous interpretations is surprising. After the errors have been corrected, what is left is not impressive. Firstly, we shall analyse the content of this paper. Secondly, we shall try to discover the psychological conditions which have brought two excellent entomologists — the first one now unfortunately deceased, with an international stature and having produced an outstanding and universally recognized work — to stray so far from reality. The first step will be a review of the scientific content of the paper and the second step an analysis of social psychology.

The definition of the subfamily

The authors start from the rudimentary and partly inexact definition that Ross gave (1947) of the "*Hydropsyche* complex" 1). They write: "Originally defined as including *Hydropsyche, Cheumatopsyche, Potamyia* and *Plectropsyche*, this group now proves to include many Old World genera as . . . " and continue: "This group of genera, we consider to be the subfamily Hydropsychinae", as if this subfamily was a new creation on their part.

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1) The dichotomic key that follows this definition is faulty on several points. For instance: "Cul running very close to M3+4" is not the alternative of "M running very close to Cul". It is the same thing. This key has been reproduced with its errors by Gordon (1974).

By doing so, they ignore that the subfamily Hydropsychinae, as they understand it, was established three quarters of a century previously, in 1903, and its existence has always been admitted by all trichopterists and the Old World genera they quote were included in it when they were described, well before 1977.

The method of interpretation of the phallic apparatus

In order to interpret the considerable variations of the phallic apparatus, the authors start from their reconstitution of the "phallus of primeval Trichoptera". Such a beginning is regrettable. Ross himself published a phyletic tree of the order (1956), completed and improved in a second edition (1967). These trees show that the Hydropsychidae of course differentiated from the ancestor of the order, and then underwent a long evolution in multiple stages, through the ancestors 2, 3 and 4, which profoundly changed the family. But Ross and Unzicker implied that the Hydropsychidae are derived directly from the common ancestor. If each family differentiated directly from this ancestor, the phyletic tree of the order would not have the ordinance that Ross gave it, but the shape of a hand-fan, with its branches diverging regularly and independently from each other, and that would be impossible. In fact, as Ross' phyletic trees show and as Nielsen's morphological studies established, the phallic apparatus of the Hydropsychidae is deeply modified from that of the common ancestor's.

An interpretation of the variations of the phallic apparatus of the American Hydropsychinae must necessarily rest on the reconstitution of the genitalia of the hydropsychid ancestor. This ancestor must be constituted by the integration of the largest possible number of characters of all or most of the genera of the family, independently of their geographic distribution. The most primitive genera, such as *Hydromanicus* and *Hydatopsyche* should especially be taken into consideration, even if they are not American. Such a reconstitution has never been done for the hydropsychids, but it is here that Ross and Unzicker should have started their analysis.

The phallic apparatus of the three ancestors of the family

Figure 1 represents the "probable phallus of primeval Trichoptera". In its main features, it is correct, but faulty in its details. The basal part is called phallobase, when it is in fact the phallotheca (phallobase = phallotheca + endotheca). It is too long. The phallotheca and the endotheca should be shorter and the aedeagus and the parameres longer. The parameres are shown in lateral superior situation, when they should be in lateral inferior position. They are membranous at their base, when they should be sclerotized throughout. But, as we have seen, it is not useful to take the primeval Trichoptera into consideration to explain the variations of the phallic apparatus of the Hydropsychinae.

Anyway, let us proceed with our analysis. Figure 2 represents the "probable first Hydropsychinae phallus" and figure 3 the "probable ancestral type from which all extant Hydropsychinae evolved". Comparing figures 1, 2 and 3 with figure 4, representing the phallic apparatus of *Hydropsyche bronta* Ross, one can see that the first three figures have been drawn by modification of figure 4. This is especially obvious for the "process e". If this part shows a membranous base and is in lateral superior position in figures 1 to 3, it is because it is so in *Hydropsyche bronta*. The phallic apparatus of the ancestor of all Trichoptera is then given as being derived from the structure of that of *H. bronta*! What is even more surprising is the fact that in reconstituting these ancestors from American species only, Ross and Unzicker unconsciously and indirectly let themselves be influenced by their nationality.

Terminology

The phallus. This term is rather often used, but it is improper, since it should be reserved for the Vertebrates, as is *penis* used by myself in the past. The term *phallicata*, also used, is acceptable, but has the inconvenience of being too precise. The most appropriate term is *phallic apparatus*, for two reasons. Firstly, it is a true apparatus, in the anatomical sense of the word: a set of organs or parts, fulfilling various steps of the same function. In Trichoptera, these parts are the phallotheca, the endotheca (phallobase = phallotheca + endotheca, as we have seen), the aedeagus, the parameres, sometimes the dorsal appendage of

the aedeagus, its ventral plate and various processes of the phallotheca. These are never all present at the same time. Secondly, this term has the advantage of being imprecise and of having a broad meaning. This allows its use for apparatus that are as widely different in component structures from each other as those of the hydropsychids, hydroptilids, rhyacophilids and hydrobiosids, for instance.

The parameres. The authors propose to replace the term *parameres* by *endothecal process*, "in the hope to avoid ambiguity", since the former is also used in various orders of Insects, with different meanings. The result is that, in the hope of avoiding an imaginary ambiguity, the authors introduce two real different causes of confusion, with cumulative effects.

It is true that in each order of Insects, specialists use their own terminology and sometimes they apply the same term to different parts, in different orders. We are still far from the day when a unified terminology of the genital parts will be used in the whole class. In the meantime, it is enough to refer to the taxonomic context to avoid ambiguities. For instance, we know that the parameres of the Hymenoptera Terebrantia are homologous to the claspers of the Trichoptera. For the same reason, the multiple meanings of the word *wing*, for instance, have never raised, as far as I know, any misunderstandings between aviators and architects, between generals and hair-dressers, neither between ornithologists, paleontologists, entomologists and mammalogists. Such examples are plentiful in all human languages and do not hinder anyone.

The first true cause of confusion introduced by Ross and Unzicker is that some trichopterists will follow their suggestion and others not. Then, we shall have two sets of terms having the same meaning and all specialists will not be aware of that. The second cause is more serious and shows Ross and Unzicker's lack of familiarity with the morphology of the Trichoptera. It is evident that the parameres are parts inserted on the endotheca, but the converse is not true: all parts inserted on the endotheca are not parameres. Their initiative introduces confusion between these two categories. The term *parameres* (the titillators of the old authors) is specific. It designates precise and true appendages, generally accompanying the aedeagus, primitively paired, inserted on the endotheca in a lateral inferior position relative to the aedeagus and never present when the aedeagus is lost. The term *endothecal processes* is not specific and applies to structures of various nature. They are not true appendages but phaneres (productions of cuticular origin) and are present only when the aedeagus and the parameres have been lost by specialization and when the phallotheca and the endotheca have been secondarily elongated to perform the copulatory functions of the missing aedeagus. Generally spiniform, they are rarely a single pair (*Ptilostomis, Eubasilissa, Hydropsyche* of the *bronta* group), generally are in quite variable numbers (most of the philopotamids and phryganeids) and even sometimes countless (*Stenopsyche marmorata* Navas, *duplex* Schmid, *sauteri* Ulmer). Sometimes, they are granulated plates (*Hagenella*).

Therefore, it is necessary to retain the term parameres for its precise designation, in order to distinguish these appendages from other parts of the endothecal armature.

It would also be necessary for trichopterists to refrain from using personal and fancy terms for the genital appendages. They should discipline themselves to adopt the same strictness of vocabulary as they have already accepted for the wing venation. It is true that a generalised system of notation for the veins is admitted through the entire class Insecta, while no comparable thing exists for the genitalia. But this should not be a problem, since a satisfactory terminology for the genital parts of the Trichoptera has been developed by Nielsen and improved and completed by myself (1970, 1979). Authors who would eventually disagree with this terminology should be motivated by constructive scientific reasons, but their terminology should not be left to chance or whims.

The interpretations of the variations of the phallic apparatus

Starting from their reconstitution of the phallic apparatus of primaeval Trichoptera, the authors try to explain the numerous and complex variations of the lobes and parts of the phallic apparatus of the Hydropsychinae. Here, once more, the reader is confronted with the

inaccuracies of their interpretations. Interpreting the variations of the genital parts, the entomologist finds himself in the same situation as the mammalogist identifying a set of teeth or the anthropologist analysing various elements of a myth, or a body of kinship elements or of marriage rules. These interpretations are not made in the way that one reads tea leaves or tarots, but in conformity with a specific style proper to the objects studied. No longer do we use a style like the first travellers' to South America, who described the seemingly marvelous cotton plant as a tree bearing sheeps as fruit and which needed only fleecing. Such a style is contrary to what is the pattern of the Universe.

Correct interpretations must be what the English language calls so appropriately *educated guesses*. The word *educated* implies the presence, in the researcher's mind, of a *corpus* of fundamental knowledge as wide as possible, from which arises an awareness of the style of the studied objects. Ross and Unzicker's interpretations appear, on the contrary, as wild guesses. It seems that the same type of results as theirs could be obtained by a non-entomologist who would amuse himself at imitating their interpretations with the help of their figures.

Assuming the ancestral Trichoptera to have parameres, the authors equate these structures with the lateral erectile lobes of the *Hydropsyche* of the *bronta* group. But, as we have seen above, they call them "endothecal processes". Here, two accumulated errors nullify their effects. These lobes are indeed true endothecal processes and not parameres. The parameres were lost early in the Annulipalpia, probably by the ancestor of the Hydropsychoidea already, since no members of this superfamily show them. Ross and Unzicker try to homologize the terminal parts of the phallic apparatus of the American hydropsychine genera with each other and with those of their primeval Trichoptera. For instance, the parts given as the parameres (but called "endothecal processes e", as we have seen) are faithfully present on all their figures, but under so highly different shapes and aspects, that clearly all these "processes e" are not homologous between themselves. If we widen our scope and take into consideration some non-American species, this becomes evident. For instance, the oriental *Hydropsyche dhusaravarna* Schmid and *vasoumittra* Schmid have so highly complicated a phallic apparatus, that clearly these structures are neoformations specific to them. If we go through the monograph Mosely wrote on *Leptonema*, we can see that the many species of this genus have a phallic apparatus showing a type of variations similar and parallel to those of *Hydropsyche*, but more extreme (probably because *Leptonema* is a tropical genus). The apex of the phallotheca and the endotheca seem to be primitively simple and become progressively complicated into structures ending in such astonishing complexities that no one would consider homologizing them with parts of the ancestral Trichoptera, since they are obviously also neoformations.

Among the many structures they studied, the authors observe that the phallotheca of *Hydropsyche* of the *simulans* group is bifid and they interpret both branches as being the parameres (their "process e"). *Cheumatopsyche* and *Plectropsyche* are provided with apical, concave and mobile valves and the authors also treat these as the parameres. These interpretations are good examples of wild guesses, since they are in contradiction with the style of morphological variations of the Trichoptera. In fact, the phallotheca of the *simulans* group is bifid simply because it is cleft. It is impossible to interpret the nature of the valves of *Cheumatopsyche* and *Plectropsyche*, because no intermediate stages exist between these two genera and the more primitive ones, which might inform us about the nature of these valves.

Ross and Unzicker's figures are clear and reliable, but their interpretations of the variations are to be rejected almost completely. The only points worth considering are the two heavily sclerotized lobes inserted on the apex of the phallobase. They are given as the phallotremal sclerites. This is not certain, but probable and in any case the best interpretation possible. Their interpretation of the endo-phallus is convincing and interesting, but not original, since it can also be found in Nielsen.

The purpose of this reclassification

Kimmins has shown that neither the venation, nor the genitalia can be of any help to separate the African species of *Cheumatopsyche* from *Hydropsychodes* (1963). McFarlane

also remarked that the NewZealand *Hydropsyche* could not be classified by the nervation only (1976). Ross and Unzicker follow their example and propose a new classification of the American Hydropsychinae, but without saying why the classification now employed is defective, which is what we would like to know. They base all their taxa, old and new, almost entirely on phallic structures. This base is far too narrow to give satisfactory results. To reject venation characters and adopt those of the phallic apparatus only is like leaving Charybdis for Scilla. It simply means changing from one referential that is insufficient because it is too narrow to another referential that is also insufficient because it also is equally narrow. A natural classification should be based on consideration of the maximum possible number of characters. The authors should have considered characters of the head, the wings and the complete genitalia of both sexes, as McFarlane did with the New Zealand species. Natural taxa are those which impose themselves on our perception, but we should not try to impose categories on a matter that does not lend itself to categorisation.

In their introduction, the authors say they tried to group the species into "monophyletic clusters". The expression is fortunate, but what is less apt is that they treated their clusters as genera, when they are mere species-groups. To divide the uniform genus *Hydropsyche* into two genera and one of these into two subgenera means a depreciation of the concept of genus. 1) In my revision (1970), I divided *Rhyacophila* into 78 species-groups. All of these are *much more* different from each other than are Ross and Unzicker's genera, but it would have been absurd to give them generic status. Can we imagine the fine genus *Rhyacophila* pulverised into 78 pieces? If the generic criteria adopted by these authors were to be applied to the whole order Trichoptera, we would witness a galloping taxonomic inflation, a true canceriform process, which would lead the classification into chaos. "Je n'ai pas l'intention de sacrifier au Moloch de l'inflation taxonomique qui dévore maintenant . . . pas mal de chapitres de l'Entomologie. Dieu soit loué, on n'en est pas encore là en Trichoptérologie (Botosaneanu 1974).

As we have seen in the preceeding pages, the general impression one obtains from Ross and Unzicker's paper is of some unreality. The reader feels that the authors have not grasped their subject. Yet, Ross had, at that time, achieved a long career as a trichopterist and the sum of his work amounts to a great deal of progress in this field. Then, we face what rhetoricians call an aporia, which means a contradictory situation. We shall try to resolve it. Let us leave aside the paper of these authors to consider the cultural environment which produced it, since the first principle of thermodynamics also applies in part to cultural phenomena.

Having inherited the spirit of the pioneers and colonizers of the North American continent, our society is pragmatic, purpose-directed and mainly concerned with productivity and efficiency, in one word, utilitarian. Productivity requires specialization, not only of the different trades, but also of the mind. Our students get from the beginning a specialized education, that is not only extremely limited in extent, but also conditions their minds to think in terms of specialization. Utilitarianism directs their attention to only that which can be useful here and now. Efficiency hinders and destroys the innate curiosity of the child.

The scientist's goal is to discover the world. This implies that he has the vocation to do so and is above all prompted by intellectual curosity; that his mind is open and avaiable; that to him happiness is to know things for themselves and as they are; that knowledge is an end in itself and is its own reward. Scientific activity is to wonder at the intelligibility of the world's beauty. It is also love for the studied object that implies an authentic and somewhat sensual communication with it. It is awareness that one does not know what one does not know and why. It requires the necessity of a critical sense on the researcher's part, in regard to his own thought and also that of other's. There must also be a capacity for indignation in regard to mistakes. Respect for the object of one's study forbids alterations and manipulations that would make it more easy to understand. Scientific activity promotes the mind's capacity to think and simultaneously curiosity enriches it with acquired knowledge. Knowledge

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1) The same reproach can be made to McFarlane. His two New Zeland genera would be best considered as species-groups or at the most as subgenera.

enlightens and fecundates itself in a whole that is both closed upon itself and open to the outside world. Such as in Valery's "Palm":

Tu n'as pas perdu ces heures
Si légère tu demeures
Après ces beaux abandons
Pareille à celui qui pense
Et dont l'âme se dépense
À s'accroître de ses dons 1)

In our society, it is not exactly so. Knowledge is not an end in itself and still less its own reward. When one studies contemporary entomological papers, one feels that the purpose of their authors is primarily to accomplish something useful with efficient means. For instance, to improve the classification of a group or to make identification of species as easy as possible by keeping to mere bare facts. It seems that the author's unconscious purpose is not so much to know the Insects and make them known by others, but to act and be useful *by the means* of the Insects. Such papers are typical products of our materialistic philosophy, whose ultimate values are action and productivity. In the end, the world is no more considered as an object of knowledge and contemplation, but is reduced to a means of action. We have here a perversion of the relations between the means and the ends, the ones having taken the place of the others and inversely.

But, productivity and efficiency cannot be ends in themselves. A man whose activity is directed exclusively towards efficiency can certainly not be poetically compared to a palm-tree in the desert that would also be a philosopher. I would rather compare him to a stomach, the function of which is to transform the matter that flows through it, but without keeping anything for itself, letting all escape beneath. Far from enriching the personality and the character of man, "common measure of all things", efficiency leads to the alienation of the mind 2) from itself and from the world. It is not surprising then, that encounter group-therapy appeared in our society and plays such an important role within it. Being prompted only by outside motivations, efficient man Marcuse's unidimensional man seems to be bound to produce only means producing other means, in an endless chain of events which reach nowhere, that is into meaninglessness.

For the scientist, it is a serious hindrance to be alienated from the subject of his study. In todays trichopterology, we have authors publishing papers on very limited subjects: specialization of a specialization within another specialization. They are interested only in the fauna of a limited region. They know nothing of the groups on which they did not publish themselves, since they are not really anxious to know the Trichoptera, but merely to work on them, that is to do something useful by the means of them. They are largely ignorant of previous literature and refer to it only to look for details they immediately need for a certain purpose. Some lack critical objectivity for each other's papers and seem to consider, with a polite impartiality, that one opinion is as good as another, once it is published. The saddening mediocrity of some recently published papers can be explained by the lack of intimate knowledge and contact with the studied objects and the absence of intellectual curiosity on the part of their authors. Among current production, quite a number of papers strike me more by their limitations and defects than by the positive knowledge they bring. Due to an extreme intellectual specialization, the object of study are taken separately and out of their context; their relationship with their homologues are ignored. Yet, we know that all things in the world, all words of a language receive their meaning from the outside only. Isolation of objects from their proper context reflects a degenerated spirit of analysis. This can only lead to an absence of their meaning and so deprives us of all means of understanding and explaining these objects. When the spirit of analysis is pushed to its extreme limit, it destroys the objects it pretends to explain, and can only involve within itself and annihilate itself in the evidence of its own barrenness.

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1) You have not lost these hours. So light you remain After these beautiful abandons, Like he who thinks And whose soul exerts itself So it might grow with its own gifts.

2) And of other things as well that are not relevant here.

CONTRIBUTION TO THE SYSTEMATICS OF THE CADDIS-FLY FAMILY LIMNEPHILIDAE

III: The genus Goereilla

G.B. Wiggins

For the past few years, Dr. G.B. Wiggins has strongly promoted the study of immature stages of the Trichoptera. It is a fact that the study of imagines has for a long time been too exclusively favoured by trichopterists and that young stages have been neglected. In a number of fine studies of the life history of several dicosmoecine and goerid genera and more recently in the brillant compilation: "Larvae of the North-American Caddis-fly Genera", Wiggins repeatedly insists on the necessity to consider immatures as well as imaginal characters in order to obtain truly sound phylogenetic reconstitutions. By doing so, Wiggins has opened ways for trichopterology, even if not completely new. He strongly contributed to its progress along these lines and has emerged as one of the very best actual trichopterists.

Nevertheless, his initiatives call for commentaries, since they are not entirely objective. In "Larvae of the North-American Caddis-fly Genera" we read: "It is well established that data from larval morphology are essential in assessing systematic relationships of Trichoptera and for advancing hypothesis concerning their phylogeny . . . ". This affirmation is wrong taxonomically, which does not bother me too much, but it is also misleading and that is why I shall object to it.

We are here facing a problem that obviously is more psychological than scientific. In the past, immature stages were considered as almost unimportant and suddenly one discovers that they are essential and one can no longer do without them. Other such cases of sudden reversals of values can be found throughout the history of sciences and they actually occured under the same similar historical conditions. Though they usually were necessary, those sudden changes were more reactions against a previous state of things, than objective in themselves. They were almost invariably accompanied by the strong belief that they are the most important element of the problem or of the situation, simply because they occured last. Similar psychological determinisms can easily be observed in daily life. In election campaigns, many electors tend to vote for the last candidate they have heard. He seems to be the most impressive, because he was heard last. The case we are here dealing with is somewhat of the same nature. It is important we should be realistic and keep our sense of values and nuances.

The actual state of the classification of the Trichoptera shows that the study of the immature stages is not essential. The many authors who progressively, over more than one century, built this classification did it mostly on the basis of imaginal characters. If they missed something really essential, the classification now accepted would not be as satisfactory as it is, but instead rather chaotic.

A second reason can be found in the whole of my work. Over the years, I made many important changes in the classification of Trichoptera and these were made exclusively on the basis of imaginal characters. None of these changes has since been contradicted by the study of immature stages. I certainly made mistakes, such as placing *Lepania* in the Moropsychini, *Homophylax* among the Pseudostenophylacinae, *Rhyacophila rickeri* in the *sibirica* group and certainly others I have not yet discovered. But these were not due to my real and almost complete ignorance of the immature stages, but to a lack of judgement on my part. *Errare humanum est*. I simply failed to see the imaginal characters indicating that the true place of *Lepania* is in the family Goeridae, *Homophylax* in the Chilostigmini and *Rh. sibirica* in the *verrula* group. The only exception is a partial case. Imaginal characters of *Austrotinodes* led me to place this genus in the Psychomyiidae and I only suspected (1958b) that it might be an Ecnomidae. It is Flint's study of the immatures (1973) which changed these suspicions into certitudes and definitively assigned *Austrotinodes* to the Ecnomidae.

Generally speaking, in the whole Living World, mature organisms tend to be more different from each other than younger ones are. In the Animal Kingdom, later stages are usually more differentiated than earlier stages. In the Vegetal Kingdom, parts that appear later in the development of the plant are generally also more different between themselves than parts which appear earlier. There are quite a few well known exceptions to this general rule, but Trichoptera are not one among them. Characters are much more numerous in caddis-flies than in their immatures, because adults have a more complex and differentiated morphology than immatures (two wings, genitalia, two sexes). Only the male genitalia of some families show a seemingly unlimited number of characters. Furthermore, imaginal characters show a much wider range of variation. To take a few extreme examples only, a comparison of the nervations of *Pseudosericostoma simplississimum* Schmid and *Trichomacronema shanorum* Schmid; or of the genitalia of *Tolhuaca cupulifera* Schmid or *Potamyia flava* Hagen on one side with those of *Rhyacophila kyungpa* Schmid or *Hydrobiosis umbripennis* McLachlan on the other side, shows an amazing amplitude of variations, not only of the details, but also of the general architecture of those apparatus. In many genera of Trichoptera, it is still impossible to distinguish from each other larval stages of species that have been described on adult characters. Nobody would hesitate to describe a new species on imaginal characters only. But, experience has shown that the very rare existing descriptions of new forms based on larval characters only (Iwata, Lepneva) are rather useless.

Except for a few special cases, knowledge of the immature stages is not essential, not even necessary for obtaining a sound classification and phylogeny. It is merely useful and to be desired as a supplementary and welcome source of characters. It is clear that the study of the immatures should be encouraged, not only since it is taxonomically and phyletically useful, but also as a purpose in itself, since it represents an advancement in knowledge. But, it would be dangerous if it supplanted the study of the imagines as it actually tends to do, as we shall see below.

Wiggins' affirmation quoted above is also misleading. Some young trichopterists now feel inhibited in their researches when they do not know the immature stages of the species they are working on, because they have been told that they are missing something "essential". I shall quote two examples only. Peck and Smith (1978) refuse to take into consideration my affirmation that the *glareosa* group of *Rhyacophila* is the closest relative of the *hyalinata* group, because they do not know the young stages of the former. Ignoring the imaginal characters, they adopt the solution of facility which consists in postponing the resolution of their problem until the undetermined time when they will know these stages. Since it is easier to capture imagines than to find their immatures, it is to be feared that such cases will occur again.

Flint, who is far to be a beginner, does something similar on a larger scale. He questions (1971) the status of the South American Psychomyiidae (*sensu Flint*): "The classification of this "family" is not at all satisfactory. The difficulty starts in whether one should recognize its division into one, two or more families, and extends to the definition of the genera. Until the life history is known for species in all genera, I do not believe that we can develop a stable, widely accepted classification". It seems that Flint reduces the imagines to abstracted supports of the taxonomic terms only and considers the immature stages alone as the real taxonomic substance of the family. In fact, the three families that are included under the name Psychomyiidae (*sensu Flint*) are the Polycentropodidae, the Psychomyiidae (*sensu omnium Auctorum*) and the Xiphocentronidae. These families, like all the others, have been first and quite well defined on the basis of their imaginal characters, but Flint chooses to ignore this. His wide knowledge of the imagines of the Neotropical species gave him all the necessary clues to solve the problem he was exposing, but he did not use them. He also adopts the easy solution which consists in postponing the resolution of his problem to better days: "Until the life history is known . . . ". In fact, Flint's question has little meaning. It is clear that if these three families were distinguishable by the immature characters only, they should not be kept separated. If these families are really distinct, it is primarily because of their imaginal characters.

Experience has shown that too exclusive attention to immature characters can be dangerous, since it leads to wrong results. Ross and Gibbs (1973) displaced the genus *Phylocentropus* in the Dipseudopsidae on the basis of larval characters only. They ignored the imaginal characters that show this is blattantly wrong. Ross (1956) classified the Rhyacophiloidea in the Integripalpia, overlooking the fact that Martynov placed them in the Annulipalpia when he created the two suborders. Ross did so on the basis of a faulty interpretation of the variations of the anal legs of the larvae. But, the female genitalia show without any equivocation that the true place of the Rhyacophiloidea is in the Annulipalpia, as Martynov rightly pointed originally. In the following pages, we shall see that if Wiggins has also taken adult characters into consideration (and not larval ones only), his opinion on *Goereilla* would have been different.

In his paper on the genus *Goereilla*, Wiggins presents us with a detailed study of a genus that is extremely interesting by its phyletic position. This paper first appears to be a model of what any study on Trichoptera should ideally be: it is based on all three stages, larva, pupa and imago. It contains excellent descriptions (though quite incomplete for the imago), abundant illustrations of each of the three stages and ends with a discussion of the phyletic position of *Goereilla*: "*G. baumanni* is . . . a plesiomorph goerine, and a representative of the most primitive line yet known".

Unfortunately, Wiggins bases his conclusion on four larval characters, ignoring all the so many imaginal characters, except two. The reader is somewhat disappointed, because he is looking for a study taking into consideration the characters of all three stages, integrating them with each other and drawing a conclusion from their synthesis. Wiggins' conclusions are convincing: *Goereilla* appears indeed as the most primitive goerid, but only if one considers the four characters he quotes. If we widen our scope to include also the imaginal characters, the vision gets blurred and then the picture reappears clear again, but under a different form: *Goereilla* is not the most primitive goerid.

The best possible way to evaluate the degree of primitivity of *Goereilla* is to compare its characters with those of the theoretical ancestor of the family 1). This ancestor has never been constructed and this paper is not the right place to do it. But, we are in possession of some clues. We know that the Goeridae differentiated from the Limnephilidae. Then, which are the limnephilid genera and the goerid genera that are the most closely related to each other? As far as the adult characters are concerned, they are *Apatania* on one side and *Goera, Lithax* and *Silo* on the other side.

Let us briefly review some of the imaginal characters of *Goereilla*. The presence of ocelli and the rather little differentiated ♂ maxillary palpi make it indeed the most primitive genus of the family. In the ♂ genitalia, the IXth segment lost its dorsal apical lobes (or lobe), but these are still present in the four above mentioned genera. The praeanal appendages of *Goereilla* are broad, bifid and concave, specialized characters, since they are small and ovoïd in the four genera quoted above. The Xth segment is composed of two large branches of a rather complicated shape, whose identity is no more discernable in *Goereilla*. In the other genera, the branches of the Xth segment are multiple, identifiable and in long and thin rods, more primitive characters. The phallic apparatus of *Goereilla* lost the aedeagus and the parameres and consequently the phallotheca and the endotheca became somewhat elongated; the endotheca is armed with a spinose armature. All these characters are important specializations. It would take too long to go on and consider also the inferior appendages, the ♀ genitalia, the shape of the wings and the nervation, but we would obtain the same kind of results. Let us mention however the ecological characters. Most species of the four genera quoted above live in cold running waters, when *Goereilla* lives in the wet, black muck of spring seepage areas.

Then *Goereilla* is indeed the most primitive genus of its family by four larval and two imaginal characters. But quite a long list of adult characters points in the opposite direction.

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1) I already explained somewhere else (1979) why we must retain familial rank for the Goeridae, but this is not relevant for the present discussion.

Meanwhile, one of Wiggins' remarks opens a door on what could be the right solution: "These two species", *Lepania cascada* and *Goereilla baumanni*, "may rightly be considered phylogenetic relicts". The Lepaninae (Wiggins' Lepanini) seem indeed to be a relict branch that separated early from the base of the goerid stem, shortly after it differentiated from the limnephilids. Due in part to its specialized habitat, the Lepaninae evolved in their own direction, diverging from the other goerids and went further in their own direction than some goerines did in theirs. *Goereilla* is indeed the earliest derivative Lepaninae known up to date 1) but this subfamily does not seem to be the least specialized, even if it differentiated the first.

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1) Soon Dr. I. Levanidova shall let us know about a still more primitive lepanine genus from Siberia.

HELP NEEDED FOR CUBAN INSTITUTE OF TROPICAL MEDICINE

The Institute of Tropical Medicine in Cuba is being expanded with the objective of becoming a major treatment and research center. They now have a staff of 184 people. Their current 5 year plan calls for the construction of a 120-bed hospital, numerous research laboratories, and the establishment of a centralized, computer-based, screening system that will monitor patients infected with tropical diseases. The major advantage to researchers would be the excellent facilities and the patient control which would prevent re-infection.

CUSO (Canadian Universities Services Overseas) is now considering a co-operative project with the Cuban Government and Dr. J. Keystone of the Tropical Disease Unit, Toronto General Hospital, Toronto M5G 1L7 is co-ordinating these activities. Dr. Keystone is interested in contacting Clinicians, Parasitologists, Entomologists, Malacologists, Mycologists, Epidemiologists, and specialists in arborviral diseases who might wish to do research at the Institute in Cuba. If you are interested, please contact Dr. Keystone directly and please pass this information on to colleagues in other fields.

The Institute needs to increase its library and consequently would welcome reprints from members of our Society dealing with such subjects as biting fly systematics, physiology, behavior, or control. Please send any relevant papers to:

Prof. C. Dr. Gustavo Kouri, Director
Instituto de Medicina Tropical "Pedro Kouri"
Ministerio de Salud Publica
Ave. 15 y Calle 200
Reparto Siboney
Havana
Cuba

W.G. Friend for the Science Policy,
Public Education Committee.

GET IN THE NEWS

The Science Policy, Public Education Committee of the Entomological Society of Canada believe that the publication of popular articles concerning Entomology would increase public awareness of our branch of science and, hopefully, this would benefit all of us. This matter has been discussed with Lydia Dotto, Chairman of the Science Writers Association of Canada, who advises that Entomologists with newsworthy items should contact the Managing Editor of their local newspaper and discuss the proposed article. Often our concept of what is newsworthy differs from that of a journalist and few scientists seem to have the knack of writing popular articles. The contacts developed with the local newspapers often lead to requests for other stories when items of seasonal interest such as outbreaks of tent caterpillars, armyworms or mosquitoes occur.

The Committee feels that the more the public learns about Entomological research and insect control, the more support there should be for Entomology.

W.G. Friend for the Committee.

MEMOIRS OF THE ESC

No. 108. "Canada and Its Insect Fauna" *edited by* H.V. Danks. 573pp. Issued 12 April 1979.

No. 107. "The Nepticulidae (Lepidoptera) of Canada" by Christopher Wilkinson and M.J. Scoble. 129pp. Issued 4 June 1979.

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CANADA BITING FLY CENTRE

The Canada Biting Fly Centre is being established in Winnipeg under the recommendation of the Expert Committee on Insect Pests of Animals (CASCC). Funding for the Centre is provided through a two-year contract from the Department of Supplies and Services, and Agriculture Canada and the Department of National Defense. The Centre will function primarily as a co-ordinating centre for information on biting flies in Canada. It will be responsible for the collection and dissemination of information and, when requested, will provide expertise in biting fly control technology. The need for a centre of this type has long been recognized by biting fly workers across Canada.

Immediate objectives of the Centre are to compile an inventory of biting fly specialists in Canada and their research interest, as well as an inventory of facilities available for providing services related to control programs. The Centre will survey all levels of government, and industrial and commercial concerns to determine the demand for a biting fly information service which can be sustained on a user-fee basis. It will also assess the need for a nationally recognized extension and technology training centre for Canadian workers involved in control programs.

Winnipeg was selected as the site of the Centre both on the basis of the scientific personnel and resources in Winnipeg, and its central situation. The Centre is located at the Department of Entomology of the University of Manitoba. Further information is available from Dr. M.M. Chance (Manager), and Dr. R.A. Brust.

WANT A GRANT?

The Xerces Society, an international non-profit scientific organization, offers modest grants to support scientific research related to conservation of terrestrial arthropods. Proposals explicitly related to potential endangered species or management of terrestrial arthropod populations will be given preference. Grants will usually be several hundred dollars U.S. Young investigators and those without formal professional affiliation are encouraged to apply. Deadline for 1980 proposals is 15 January 1980. For further information, write after 15 September 1979 to Dr. Francie Chew, Xerces Grants Committee, c/o Department of Biology, Tufts University, Medford, Massachusetts 02155, USA.

RECENT DEATHS

CARRUTH, L.A., Tucson, Ariz. On 26 December 1978, age 71. Emeritus member ESC. Retired head, Department of Entomology, University of Arizona.

SCHEDL, Karl, Lienz, Austria. On 18 May 1979, age 82. World authority on Scolytidae (bark beetles). Worked for Canada Department of Agriculture 1926–32.

MUNROE, Douglas D., Auckland, New Zealand. On 12 June 1979, age 32. Member, ESC. Researcher, Biological Control, Department of Scientific and Industrial Research.

INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

c/o British Museum (Natural History)
Cromwell Road
London, S.W. 7 5BD
United Kingdom

ITZN 59

June 1979

The following *Opinions* have been published recently by the International Commission on Zoological Nomenclature in the Bulletin of Zoological Nomenclature, Volume 35 part 4, 31 May, 1979.

Opinion No.

1117 (p.209) Refusal of request for two rulings concerning the names of species of *Sphaerodactylus* (Reptilia, Lacertilia).

1118 (p.212) Conservation of *Tribolbina carnegiei* Latham, 1932 (Arachnida).

1119 (p.216) *Amaurobius* C.L. Koch, 1837, and *Coelotes* Blackwell, 1841 (Araneae): conserved under the plenary powers.

1120 (p.221) *Noctua armigera* Hübner, 1808 (Lepidoptera) conserved.

1121 (p.223) *Chanda nama* Hamilton-Buchanan, 1822, designated under the plenary powers as type species of *Chanda* Hamilton-Buchanan, 1822 (Pisces).

1122 (p.227) *Loligo opalescens* Berry, 1911, given nomenclatural precedence over *Loligo stearnsii* Hemphill, 1892 (Cephalopoda).

1123 (p.229) PLESIADAPIDAE Trouessart, 1897, given precedence over PLATYCHOEROPIDAE Lydekker, 1887, (Mammalia).

1124 (p.233) *Lichia* Cuvier, 1817 (Pisces) conserved.

The Commission regrets that it cannot supply separates of Opinions.

AN(S) 109

The Commission hereby gives six months notice of the possible use of its plenary powers in the following cases, published in *Bull. zool. Nom.* vol. 35, part 4, on 31st May 1979, and would welcome comments and advice on them from interested zoologists. Correspondence should be addressed to the Secretary at the above address.

2161 *Lethocerus* Mayr, 1853 (Insecta, Hemiptera, Belostomatidae); proposed conservation in place of *Iliastus* Gistel 1847.

2215 *Toxostoma crissale* "Henry" (= Baird), 1858 (Aves: MIMIDAE); proposed conservation in place of *Toxostoma dorsale*; with a proposed addition to Article 32 of the International Code of Zoological Nomenclature.

2234 *Lespesia* Robineau-Desvoidy, 1863; proposed designation of a type species under the plenary powers (Diptera, TACHINIDAE).

2235 *Cancer vocans major* Herbst, 1782 (Crustacea, Decapoda): request for the use of the plenary powers to validate a neotype.

2253 *Chromodoris californiensis* Bergh, 1879 (May): proposed conservation over *Chromodoris glauca* Bergh, 1879 (March) (Mollusca: Gastropoda).

BOOK REVIEWS

INSECTS OF HAWAII. VOLUME 9. MICROLEPIDOPTERA. PARTS I AND II. Elwood C. Zimmerman. The University Press of Hawaii, Honolulu. xxiv + 1903 pp., 1355 line or monochrome cuts, 8 color plates. 1978. $U.S. 60.00.

No short review can do justice to this many-sided account of the remarkable Microlepidoptera of Hawaii. Though there are only eight families and perhaps as few as 22 original colonist stocks in the native fauna, these have radiated enormously. Over 600 species are considered in this volume, and, as these are only the already named species and those needed to clarify certain taxonomic problems, they are only a fraction of the total. The genus *Hyposmocoma* alone has over 350 described species. There are 76 known adventive species, adding another eight families.

As in previous volumes, Dr. Zimmerman has revised the accepted classification from the ground up. He has figured almost all species, mostly from types, and has given numerous structural illustrations. The text is condensed but informative, and — how rare in a taxonomic work — it is *interesting*! Almost every page has a challenging or exciting comment. There are the expected taxonomic contributions: much rearrangement, with new synonymy and combinations, some new genera and a few new species. Most of the unnamed species, however, are referred to by number, and names are not proposed. There is extensive material on early stages, an innovation and improvement in this series. Some of it relates to earlier volumes on Macrolepidoptera and pyraloids. Some of the illustrations of larvae are by Margaret MacKay, former BRI scientist and sometime editor of *The Canadian Entomologist*.

Pains have been taken to make the book clear for the reader to the best of the author's ability. Keys are related to text and to illustrations, and there is an abundance of explanatory and cautionary notes. There are sensitive comments on the endemic fauna and its rapid destruction by civilization. The production is good, though the less glossy paper is not as attractive as that of earlier volumes. I found one misprint and one omitted citation; then, reviewer's honour satisfied, I gave up looking for trivia.

At about three cents a page, this book is a bargain. Of more than local interest, it will be of broad importance to lepidopterists, to biogeographers and to evolutionists, and in particular it is an example of how a regional fauna ought to be written. Dr. Zimmerman complains that obstacles prevented him from doing more. We can only be astonished that one man could do so much.

Eugene Munroe

A SURVEY OF THE LEPIDOPTERA, BIOGEOGRAPHY AND ECOLOGY OF NEW CALEDONIA. J.D. Holloway, W. Junk B.V., The Hague, Boston, London. xii + 588 pp. 1979. Dutch guilders 175,00 / $U.S. 85.35.

This new addition to Jeremy Holloway's penetrating series of faunistic-biogeographic works on lepidopterous faunas of tropical Asia and neighbouring islands deals with a particularly interesting island. Tropical in climate, of mid-Jurassic age, measuring 390x50 km, and reaching elevations of over 1600 m, New Caledonia is warm enough, moist enough, old enough, large enough and rugged enough to support a varied endemic and immigrant biota. Its position south of the Solomons, east of Australia, west of the New Hebrides and north of New Zealand places it at a crossroads for very diverse colonists, yet sufficiently off the track to permit development or preservation of a rich array of archaic and derived forms. The book has chapters on geological history, phytogeography, vegetation, sampling programme, ecology of the Macroheterocera, geography of Lepidoptera and of other animal groups, biogeographic discussion, and a systematic account of the 13 families of Macroneterocera. The latter deals with over 200 genera and over 400 species; some of the species are extralimital, but were discussed to clarify New Caledonian forms. Ten genera, 138 species and 27 subspecies are described as new. The illustrations of habitats, moths and genital structures are plentiful and excellent. The discussion of ecology and biogeography is rich in information, sophisticated in analysis, and thoughtful in interpretation. Paper, typesetting,

reproduction, design and binding are satisfactory. Though the book is expensive, it presents the results of a large and well-integrated investigation, whose importance extends far beyond the geographical confines of its primary subject. Author and publishers are to be congratulated on a good job well done.

Eugene Munroe

BIOCHEMISTRY OF INSECTS. Edited by Morris Rockstein. Published by Academic Press, New York, San Francisco and London, 1978. pp. xiv + 649. Price $U.S. 29.50.

Since the publication of *The Biochemistry of Insects* (Gilmour, 1961), the field has broadened considerably. M. Rockstein recognizes this in his preface to *Biochemistry of Insects* and states his goal as an "in-depth updating" of Gilmour's book. In a multi-authored work such as this one, skillful editing is required to ensure that all the material is readily accessible and is presented in the same form and at the same level; ultimately, it succeeds or fails as a comprehensive work on the basis of its component chapters.

Rockstein claims the book is to "serve as an important reference source for the advanced student, the research scientist, and the professional entomologist seeking authoritative details". The lack of an author index limits the reader's ability to gain access to information, and thus hinders the book's use as a reference source. Occasional omissions in the subject index aggravate this problem. Also cross-referencing is almost non-existent. No cross-references link the separate discussions of cytochromes found in sections on hemoproteins, biochromes and detoxication mechanisms. Other examples are too numerous to mention.

More than half of the chapters lack citations within the text. For the average undergraduate student, to whom Rockstein also addresses this book, citations may be a distraction. But for the researcher or the lecturer, access to experimental details is an absolute necessity. And in almost half the chapters inadequate reference lists, comprised mainly of books and review articles, compound the problem.

Inconsistency in referencing is part of a general inconsistency in the approach and goals of the various chapters. Some authors seem to be writing for undergraduates, others for more advanced readers. Thus we have the situation where, in "Chemical Genetics and Evolution", F.J. Ayala explains that "The nucleotides may be considered as the letters of the genetic alphabet" while, in an earlier chapter, "Protein Synthesis in Relation to Cellular Activation and Deactivation", P.S. Chen assumes familiarity with the rare nucleotide Q, the structure of which was not determined until 1976. N. Weaver, in two otherwise well-written chapters on "Chemical Control of Behavior . . . ", rarely discusses the limitations of experimental results, while A.G. Richards provides a very critical, albeit somewhat sarcastic, discussion of experimental procedures in "The Chemistry of Insect Cuticle".

Even the format and terminology used by the authors varies. In "The Functions of Carbohydrates in Insect Life Processes", G.M. Chippendale represents biochemical pathways as lists of separate chemical equations, using equal signs and compound names; most authors use the more familiar scheme of linking a series of structures by arrows. Rockstein warns the reader, in his preface, of "the occasional use of different names for the same insect" but this warning makes the problem no less inconvenient when it occurs. Chippendale uses both *Phaenicia sericata* and *Lucilia sericata* to refer to the same insect. N. Weaver is the only contributor who includes the species' describer in insect names.

As with any first edition, there are typographical errors. Most of these are readily identifiable as such, and hence are relatively innocuous. But some are misleading, especially to the reader unfamiliar with a particular area. For example, the structures given for 7-dehydrocholesterol, 22-deoxy-alpha-ecdysone and alpha-ecdysone (p.316) are missing a methyl group. In contrast, erroneous pyrethroid structures (p.532) are easily identified by their trivalent oxygens and pentavalent carbons. Other serious errors include the use of "cystine" for "cysteine" (p.108), "Coleoptera" for "Diptera" (p.136) and "hybrid stability" for "hybrid sterility" (p.603). Some errors provide comic relief. A novel electron transport chain (p.135) would create both electrons and energy from nothing. And those with

an interest in advanced organic chemistry will want to read the discussion of the "synthetic boll weevil" (p.451).

Most chapters provide a fair amount of useful information, but are deficient in one or more respects. "Biochemical Defenses of Insects" by M.S. Blum lacks references to the literature, but is nonetheless aimed at the more advanced student. This chapter consists mostly of a catalogue of defense compounds and their distribution. The information is useful, but there could be a more complete discussion of biochemistry. Also the verbose writing style makes reading difficult.

Some chapters are particularly poor. The topic dealt with in "Functional Role of Proteins" by M. Agosin should have been integrated throughout the book, since it is much too broad for a single chapter. In addition, this chapter suffers from poor grammar, resulting in ambiguities, as in "the removal of ATP or CA^{2+} with chelating agents such as EDTA." (p.97). The results are not always this trivial. The section on chironomid hemoglobins (p.122-124) is so confusing that the reader must search the original literature for a proper account. Agosin also tends to make unsubstantiated statements. The most extreme example occurs when he concludes that molecular studies on triose phosphate dehydrogenases "may suggest when in evolutionary time a particular behavioral pattern develops in an invertebrate line." (p.130). The reasoning leading to this conclusion is not apparent, and no citation is given. But a paper by C.W. Carlson and R.W. Broseurer (1971, Biochem. **10**: 2113-2114) draws the same conclusion: "This is the first case in which molecular studies may have suggested when in evolutionary time a particular behavioral pattern developed in an invertebrate line." This paper, so vital to understanding the omitted reasoning, should have appeared in Agosin's list of references.

R.D. O'Brien's chapter "The Biochemistry of Toxic Action of Insecticides" should perhaps be criticized mostly for what it omits. There is no discussion of the standard methodology for determining modes of toxic action. Also, his treatment of the coupling of electron transport to ATP generation (p.537) is based on the dated concept of a cycle of unknown high energy intermediates; the chemiosmotic hypothesis, which won Peter Mitchell a Nobel Prize, is not mentioned. Moreover, O'Brien ignores the Gibbs-Donnan effect: "When a membrane is impermeable to an ion, that ion cannot influence the polarization of the membrane." (p.525).

Those familiar with *Evolution* by T. Dobzhansky *et al.* will no doubt experience a sense of *déjà vu* upon reading "Chemical Genetics and Evolution" by F.J. Ayala. Much of the chapter comes verbatim from, or closely paraphrases, Ayala's four chapters in *Evolution*, where his treatment of the topic is more complete. Nor does he stop with his own chapters. Dobzhansky writes: "In 1947, Sacca was the first to report that a population of the housefly, *Musca domestica*, had become resistant to DDT." (*Evolution*, p.121) and Ayala writes: "Insect resistance to a pesticide was first reported in 1947 for the housefly, *Musca domestica*, with respect to DDT." (p.594). This simple rephrasing results in an error, since resistance has been recognized since 1914. (A.L. Melander, J. Econ. Ent. 7: 167-173).

Although some chapters are poor, other chapters are notably good. "Functional Role of Lipids in Insects" by R.G.H. Downer, "The Chemistry of Insect Cuticle" by A.G. Richards, and "Biochemistry of Insect Hormones and Insect Growth Regulators" by L.M. Riddiford and J.W. Truman have common characteristics which make them outstanding. Without losing too much detail, they are organized clearly enough to be understood by a freshman. They cite basic research and discuss critically both procedures and conclusions. And they define the present limits of knowledge, suggesting possibilities for future research. These chapters come closest to fulfilling the purpose stated by the editor.

In *Biochemistry of Insects*, Rockstein sets out to provide a "summary volume" of current knowledge valuable to readers ranging from the freshman to the researcher. However, the needs of freshmen and of researchers are, to a degree, mutually exclusive — the detail required by the researcher can confuse the freshman. The result is a book which cannot be used in its entirety by any one reader. It is also marred by occasional inaccuracies and omissions. Nonetheless, it does summarize much of the present knowledge.

BOOK NOTICES

"The Biochemistry of Plant Phenolics" ed. T. Swain, J.B. Harborne and C.F. Van Sumere, in the "Recent Advances in Phytochemistry" Series Volume 12. Plenum Press, New York and London. 1979. $U.S. 59.40.

This volume represents the proceedings of the First Joint Symposium of the Phytochemical Society of Europe and the Phytochemical Society of North America (Ghent, Belgium, 1977), and as such is probably of more use to plant biochemists and plant physiologists than entomologists. The volume has 19 chapters moving logically from the chemistry of plant phenolics, the biochemistry, the physiology and finally three chapters on the role of phenolics in plant disease, phenolics of pharmacological interest and phenolics and the environment. These three chapters will however be of considerable interest to many zoologists and entomologists. For those intending to carry out research on phenolics the initial chapters on the separation and identification of these substances would be invaluable. Van Sumere *et al.*'s discussion on the most up-to-date methods of phenolic chromatography is very well written and extremely useful.

It is a pity that the final chapter in the book (by one of the editors, T. Swain) is not longer as it presents a number of thought-provoking ideas in regard to the ecological role of plant phenolics.

D.M. Reid
University of Calgary

Spruce Budworms Bibliography, with Author and Key Word Indices by Daniel T. Jennings, Fred B. Knight, Susanne C. Hacker, and M.E. McKnight. April 1979, Misc. Rpt. 213, Sch. of Forest Resources, University of Maine, Orono. 687 pp.

Available at $19.00 paper copy, Accession No. PB297124/AS, U.S. Department of Commerce, National Technical Information Service, Springfield, Virginia, 22161.

Prepared by the School of Forest Resources, University of Maine at Orono, in cooperation with the Canada/United States Spruce Budworms Program (CANUSA) this bibliography contains over 1500 references to literature on coniferophagous budworms. Emphasis is on the spruce budworm, *Choristoneura fumiferana* (Clemens), and the western spruce budworm, *C. occidentalis* Freeman. Also included, but to a lesser extent, are references to literature on the jackpine budworm, *C.pinus pinus* Freeman, the 2-year-cycle budworm, *C. biennis* Freeman, and other spruce- and fir-feeding *Choristoneura*. A brief abstract is included with most references.

David G. Grimble

Larsson, S.G. 1978, Baltic Amber — a Palaeobiological Study. Scandinavian Science Press Ltd., DK-2930 Klampenborg. Denmark. 192 pp. 120 Dan. Kr. ($U.S. 19.70). Hard-cover.

This is the first volume in a new series called Entomonograph designed to enable entomologists to publish studies in systematics, taxonomy, morphology and other subjects which are often precluded from conventional publications because of their length. Five other volumes are in preparation and others will follow. This volume begins with a re-creation of the amber forest environment and goes on to consider the fauna, of which insects are by far the most abundant component, under the headings of Plant Sucking Insects, Leaf and Seed Eaters, Gall Producers, Nectar Suckers, Insects Trapped While Resting, The Fauna of Moss and Bark, and The Hidden Fauna of Tree Trunks. Finally the author examines the fate of the Baltic Amber forest in the face of climatic deterioration.

G.P.

M.D. Atkins. 1978. Insects in Perspective. Macmillan, 866 Third Avenue, N.Y. 10022. 513 pp.

The publisher's blurb on the jacket of a book sometimes performs a useful service and perhaps even helps to sell the book. Sometimes, as in Atkins' book, it backfires. Superlatives abound: the sections on pest control are said to be "up-to-the minute" and "comprehensive"; beneficial insects receive "an unusually complete examination"; the view given of the insect world is "truly balanced"; the discussions of behavior, structure, and physiology give the student "a sound understanding"; the treatment of behavior and ecology are "extensive"; the appendix is "excellent"; the photographs and line drawings are "outstanding"; and many topics receive "complete coverage". And if this is not enough, "Here is the first volume to fully integrate general areas of applied entomology with pure entomology." If all this were true, this would indeed be the book we have all been waiting for. But it does not come off. The "pure" and "applied" sections are each long enough to be books in themselves, there is little gained by putting them together in one book, and there are better treatments of both already on the market. And for a former Canadian to use a book on the insects of a continent on which grylloblattids do not occur as the source of a drawing of *Grylloblatta campodeiformis* is sacrilege!

G.P.

Blum, M.S. and N.A. Blum. (Editors). 1979. Sexual selection and Reproductive Competition in Insects. Academic Press, 111 Fifth Avenue, N.Y. 10003. 463pp. US $23.00. Hard cover.

This book had its origins in a symposium on reproductive behavior held during the 15th International Congress of Entomology in Washington. Some of the contributions, however, were written especially for the book, and the title has been changed to better reflect the underlying theme, which is the exploration of mating systems in evolutionary terms. There are 13 papers, 4 of which are general (Otte; Borgia; Parker; Alexander and Borgia), while the others concentrate more on particular groups of insects: Scorpion flies (Thornhill); fig wasps (Hamilton); Platystomatid flies (McAlpine); lamellicorn beetles (Eberhard; Otte and Stayman); luminescent beetles (Lloyd); Crickets (Cade); bees and wasps (Alcock); and *Nasonia* (Barrass). Each contribution is a self-contained paper but there are both author and subject indexes for the book as a whole. The theme of the book can be seen from a perusal of the author index: Lorenz is cited on 3 pages in 3 articles and Tinbergen on 3 pages in 2 articles, while Trivers appears on 44 pages in 8 articles and Maynard Smith on 24 pages in 7 articles!

G.P.

Hermann, H.R. (Editor). 1979. Social Insects, Vol. 1. Academic Press, 111 Fifth Avenue, N.Y. 10003. 437pp. Hard cover.

This three-volume, multi-author treatise on social insects is an attempt, in the words of the editor, "to collate the works of modern researchers working in the field of insect sociobiology". Volume 1 is intended to be the most general of the three and includes a mixed bag of topics. After Hermann's introductory chapter, theories on the origin of insect sociality are reviewed by Starr, and Carpenter and Hermann give a short account of the fossil record for social insects. Territoriality is next reviewed by Urbani and is followed by a long chapter on caste and division of labor by Brian. Crozier deals with the genetics of sociality and the book finishes with a chapter on the structure and bionomics of larvae of the social Hymenoptera by G.C. and J. Wheeler, and one on the social and evolutionary significance of social insect symbionts by Kistner. A subject index completes the book. The editor's preface states that "Volume 2 examines further behavioral phenomena demonstrated by social and subsocial insects and social noninsectan arthropods; Volume 3 reveals the social nature of each group of eusocial insects".

Gordon Pritchard

PERSONALIA

Professor and Mrs. F.A. Urquhart have received the Burr Award from the National Geographic Society, Washington, D.C.

Congratulations.

EMPLOYMENT — EMPLOIS

Simon Fraser University
Burnaby, British Columbia, Canada

DEPARTMENT OF BIOLOGICAL SCIENCES

DIRECTOR OF MASTER OF PEST MANAGEMENT PROGRAM

Applications are invited for the vacant position of Director of the Master of Pest Management (M.P.M.) program and related activities at Simon Fraser University.

The appointment will be at the rank of Associate Professor or Professor, with level and salary commensurate with training and experience. The successful candidate should have an established professional and academic reputation in pest management and a current research program in at least one aspect of pestology. Responsibilities will include the development and organisation of the M.P.M. program as well as research and teaching in the individual's area of expertise. The position is available as of 1 September 1979, subject to budgetary approval. The current salary range for the Associate Professor and Professor rank, is respectively, $25,636 — $41,018 and $31,552 — $55,216 per annum.

Applications, including an up-to-date curriculum vitae, copies of representative recent publications and the names of at least three referees, should be sent to:

Dr. M. Mackauer, Chairman, Department of Biological Sciences,
Simon Fraser University, Burnaby, B.C., Canada, V5A 1S6

from whom further information may be obtained. Applications will be received until 31 October 1979, or until the position is filled.

ENTOMOLOGIST

Applications are invited for a tenure-track position as an Assistant Professor in entomology. The successful applicant will be expected to develop a strong research program and to participate in the teaching program at both the undergraduate and graduate level including the Master of Pest Management Program. Candidates must have a Ph.D. degree in entomology or a related subject. The position is available immediately, subject to budgetary approval. The initial appointment will be for a period of 2 years, renewable. The salary scale is under revision; the current salary base for the Assistant Professor rank is $18,604 per annum.

Applications should include an up-to-date curriculum vitae, a brief statement of research interests and goals, and reprints of published research. Applicants should request a confidential assessment of their research and teaching ability from three referees, to be forwarded directly to:

Dr. M. Mackauer, Chairman, Dept. of Biological Sciences,
Simon Fraser University, Burnaby, B.C., Canada, V5A 1S6.

Deadline for receipt of applications is 15 September 1979, or when position is filled.

ENTOMOLOGISTS AVAILABLE — ENTOMOLOGISTES DISPONIBLES

The Employment Committee of the Entomological Society of Canada has published a booklet containing the resumés of members who are looking for employment. A copy of this booklet has been sent to all present employers of entomologists in Canada, including Agriculture Canada and Environment Canada research stations, as well as the chairman of all university biology departments. If you do not have access to this publication, a copy may be obtained from:

The Chairman
Employment Committee
Department of Environmental Biology
University of Guelph
Guelph, Ontario
N1G 2W1

Le Comité de l'Emploi de la S.E.C. a publié un livret contenant les C.V. des membres à la recherche d'un emploi. Une copie de cette publication a été envoyée à tous les employeurs d'entomologistes au Canada, y inclus Agriculture Canada at Environnement Canada, ainsi qu'aux directeurs des départements de Biologie. Si vous n'avez pas accès à cette publication, vous pouvez en obtenir une copie à l'adresse ci-dessus.

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INSECT REARING CONFERENCE

March 4, 5 and 6, 1980
Atlanta, Georgia

The conference is USDA-sponsored but with a scope consisting of federal, state, university and commercial interest. International participants are welcomed. The conference objectives are:

a. Assemble the scientific principles of insect rearing that have been established in recent years. These would include guidelines for establishing and maintaining colonies of insects for specific purposes.

b. Identify problem areas in insect rearing programs and develop scientific recommendations for problem solving. These would include research, development, and implementation protocols.

c. Establish the scientific complexity and integrity of insect rearing as a field of scientific research.

d. Through publication of the conference proceedings, document the state of the art of insect rearing and establish a reference for direction of the science.

The planned format for the conference is two and one-half days of presentations with two nights of summary and discussion. The afternoon of the third day will be used to summarize and identify areas of need and make recommendations for future research.

The program is organized into 6 areas of emphasis:

1. Colony establishment and maintenance
2. Insect diets
3. Production, utilization, and quality testing
4. Engineering
5. Management
6. Pathology

Papers presented will be compiled and published as a book.

For more information regarding the conference, please contact Dr. R.F. Moore, Conference Coordinator, USDA-SEA, Agricultural Research, Cotton Insects Research, Post Office Box 271, Florence, South Carolina 29503, USA.