REPORT

XI INTERNATIONAL PLECOPTERA SYMPOSIUM

The XI International Plecoptera Symposium was held at the Treehaven Field Station, University of Wisconsin/Stevens Point, near Tomahawk Wisconsin USA, August 17-20, 1992. About 50 plecopterologists, spouses and students from 9 countries and 14 North American states and provinces participated. The program was opened by an address from Noel Hynes, and included 22 presented papers, 7 poster papers, a special session on stonefly photography accompanied by a photo contest, a presentation on trout stream habitat improvement in Wisconsin and a field trip for collecting and viewing of in-stream habitat improvement projects.

Organizer Stanley W. Szczytko and personnel of the University of Wisconsin, Stevens Point, were gracious hosts and the facilities and food at Treehaven were outstanding. There were the usual and stimulating exchanges of ideas and research findings during daily sessions and much fun and fellowship during the evening get-togethers, special barbecue and guitar and singing sessions led by Bill Ricker and Don Webb. One of the many highlights of the Treehaven Symposium was presentation of the first Lifetime Achievement Awards to H.B.N. Hynes and W.E. Ricker for their exemplary and continuing contributions to knowledge of the Plecoptera.

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OPENING ADDRESS BY H.B.N. HYNES

Some Thoughts on Unanswered Questions about Stoneflies

I have had an interest in the stoneflies for well over fifty years and I have wandered over much of the planet with a bug-net in my hand, so I suppose that that is why you have invited me to give the opening address to the XI International Symposium on Plecoptera at Treehaven Field Station, Wisconsin.

When I began I was very young, always the youngest among such company as "Kitten" Macan and Clifford Mortimer at the Freshwater Biological Association, and much younger than eminences like Kimmins and Mosely at the British Museum. Indeed, practically nobody else was at all interested at that time in the ecology of these insects, as opposed to their taxonomy, except Per Brinck. However, he and I were separated by World War II, and did not

learn about one another until I sent my first graduate student to work for a while in Denmark in the laboratory of my old dear friend Kaj Berg, himself a disciple of Wesenberg-Lund who was still around at Hillerod when I was there. Perhaps we should all genuflect at this point. I feel privileged to have known some of the Titans, including Thienemann, Ruttner and even Lauterborn, very early in my studies, but sad that I was just too late to meet Schoenemund, Klapálek, Despax and Uéno, and was able only to exchange letters with Frison. I must also acknowledge the welcome presence here of Bill Ricker, whose first papers on stoneflies were published during my sophomore year.

Now I am no longer a young isolated worker, but an old retired grey-head among a large crowd of very productive researchers in a growing field, and I realize that fifty years is long enough to have used up most of my energy and many of my ideas, so all that I can do today is to present some residual thoughts to throw out unanswered questions that seem to me to remain.

The order Plecoptera comprises a very ancient but plastic group of insects, which, although they are not very vagile, seem to have rather limited environmental requirements and to be able to evolve fairly rapidly at the specific and generic levels. They were thus one of the earliest insect orders to have been thought about zoogeographically (Illies 1965).

We are, however, left with several questions that seem to me to call for answers, and maybe there is not too much time remaining in which to solve them. That is because the whole order is probably the most threatened among insects by human activity. Already there is good evidence of extinctions (Zwick 1980, Hynes 1977), and I have often used *Isogenus nubecula* as an example of what is happening to our large rivers (Hynes 1984b, 1989). In all probability there were other species that survived into this century but disappeared before we knew of them, and the only hopeful note is that stoneflies seem less affected by pH, and hence one hopes by acid rain, than are some other insects that share their habitats (Petersen & van Eeckhaute 1992).

We know that stoneflies generally need well oxygenated and fairly cool water, and that this confines them to cool and/or fast streams, and greatly limits their ability to inhabit ponds or lakes. But there are exceptions, particularly among the Perlidae, and that poses the question of why that family is so restricted in distribution? Why is *Neoperla* not far more widely spread in Indonesia and beyond; and why is it not present throughout South America, to which it has had access as least as long as the cats? Is it unable to compete with *Anacroneuria* and if so why has *Anacroneuria* not spread further north in Mexico and the southern USA? Or are we here dealing with a rather recent invasion of North America by *Neoperla* from Asia, postdating the Great American Exchange when Panama became a bridge? Perhaps a close and detailed study of the relationship between Asian and North American species might answer that question.

Also, if it is true that the tropics are some sort of barrier to the spread of stoneflies, how is it that there are so many families represented in Malaya (Perlidae, Nemouridae and Peltoperlidae, Bishop 1973)? I well recall my feeling of disbelief when I collected *Amphinemura* in a steamy rubber plantation in Selangor, and my surprise at finding *Stenoperla* nymphs in Queensland under the coconut palms where a stream flows out onto a beach of coral sand. If *Neoperla* could spread into Africa, which it presumably did via Yemen (along with *Ancylastrum* and roses) why did representatives of other families now present in the Middle East not also go along? And once in Africa *Neoperla* seemed to find no barrier in the tropical climate of Kenya which it must have crossed on its way south. Even if it waited for the uplift of the East African Highlands associated with the formation of the Rift Valley, it still had to cross the hot lowlands that lie between Lake Turkana and the east coast. And then it would have had little time to evolve into the host of species that recent taxonomic methods are revealing as occurring on that continent. In short, how valid is our assumption that a warm climate is a barrier to stonefly dispersal?

Then there is the matter of life histories. Many, perhaps most, groups of ancient aquatic insects (the primary ones as I have called them elsewhere, Hynes 1984a) have fairly flexible life histories, and can adjust the number of generations to fit the seasons. Stoneflies can do this to the extent that long-lived species seem able take longer under cool conditions (Pteronarcyidae and Perlidae for example can take from one to several years to complete development); but in great contrast to Nematocera, Ephemeroptera and Trichoptera there seem to be no truly bivoltine stoneflies in temperate regions.

One thing we lack is knowledge of life histories in fairly evenly warm climates. Do, for instance, tropical Perlidae produce more than one generation per climatic (annual) cycle? One might expect that to be true in areas where there is little seasonal change, as in some parts of northern South America. Such little indication as we have is that *Neoperla* grows fairly fast on Mount Elgon in Uganda (Hynes and Williams 1962). However, we should recall that we have little indication of multivoltinism among the many species that inhabit springs in the temperate zones. Situations like those on the East African mountains, where streams flow down long valleys from the cold high altitudes, could be exploited to try to answer such questions for Perlidae, or Malaya for the other families occurring in tropical rain forest. Also there are many species in several families in tropical Queensland.

We know that temperate species can be fooled in the laboratory to complete their life cycles at the wrong season. Khoo Soo Ghee produced adults of the late summer species *Leuctra fusca* during April in my laboratory in Liverpool as long ago as the 1960s. It seems to me that Malayan Nemouridae or Peltoperlidae, which are probably easy to rear in the lab, might

answer some of these questions; as might *Nemoura variegata* in Europe or *Dinotoperla bassae* in Australia, both of which can live in still water and thus could make good laboratory animals. *D. bassae* even lives in temporary pools, and so seems like a special gift to the researcher - a sort of plecopteran white rat.

Then there is also the problem of taxonomy, that has increasingly worried me, as I am sure it does all biologists who are trying to work on ecological topics. When I started to study stoneflies it was supposed that one could look at them and then name the species. Over the 50 years since the detail of that looking has become ever more intense, and in some groups it is becoming almost impossible to be sure. I need only cite the use of the aedeagal armature or the sculpture on eggs to make my point. In some of the Australian genera (eg. *Dinotoperla*) one relies on the very detailed shape of the epiproct, and is lucky to make even a guess at females. And even though it was I who worked out the details of the complexity of the *Cardioperla* species of Tasmania I would be very reluctant to try to identify an isolated specimen (Hynes 1987). In recent years we have been getting into such details as the drumming signals, which as in bird song are very specific. We even have examples of local dialects (Rupprecht 1972, Stewart et al. 1982).

All this means that working out ecological details is becoming very difficult, which makes some of the questions I pose above almost impossible to answer.

Are we moving towards a strictly molecular definition of species? Are we following the lead of other workers on primary aquatic insects into the extraordinary complexity of say the *Simulium damnosum* complex that has been unearthed by the Onchocerciasis Control Programme (Walsh et al. 1979), or of the genus *Helicopsyche* recently demonstrated by Jackson and Resh (1992)? Already electrophoresis has been applied to the taxonomy of a difficult group of stoneflies (Funk et al. 1990). I fear that we are, and that maybe we shall not really solve these problems for the stoneflies before they become so fragmented by human abuse that all we shall have left are isolated populations, which might be expected to evolve differences simply because they are islands. It seems that ancient islands can evolve very different stonefly faunas even when they lie quite close to the mainland and on the same tectonic plate (Hynes and Hynes 1980).

The ability of stoneflies to evolve rapidly is well displayed by existing island populations (Britain, Hynes 1947, Campbell Island, Illies 1973, Iceland Hynes 1955, Lillehammer et al. 1986), so the complexity may get away from us before we grasp it. However, it seems pretty certain that the stonefly worker with the bug-net is going to have to be replaced by the indoor scientist with the molecular biological laboratory. And that is a point that makes me grateful for having worked on the group before that happens. I like messing

about in good stonefly habitats. It is much better for the spirit than is a laboratory!

SUMMARY

Stoneflies are probably the insectan order that is most threatened by human activity, so we would be wise to find out as much as possible about them, and what they can tell us about the wider biological picture, before many species become too endangered to withstand the inevitable slaughter of entomological research.

We have established that the order is ancient, with fairly clear requirements for cool and well-oxygenated water for larval development, that the adults are not very vagile, that small barriers inhibit them, but that inside those barriers these insects seem to evolve rather rapidly at the specific and generic levels. Our ideas on their zoogeography are based on these assumptions, and that the tropics must be a major barrier to dispersal of the order. The last assumption is questioned here.

We also seem to have established that among the insect groups that invaded fresh water long ago the Plecoptera are unique in apparently never having more than one generation per year. It is suggested that study in the tropical zone and the laboratory could determine why that should be so.

Finally it seems to me that the complexity of taxonomy is getting out of hand. In many groups we have come to rely on such sophisticated morphological differences between species that identification is often impossible at the specific level. Clearly that must inhibit ecological study, and something must be done about it. Are we heading towards something like DNA finger-printing?

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