Leaping larvae, jumping genes

Jack Cohen

Larvae and Evolution: Toward a New Zoology. By Donald L. Williamson. *Chapman and Hall: 1992. Pp. 223. £29.95, $39.95.*

WILLIAMSON has a theory about DNA transfer that he believes to be consonant with modern biology, but which I believe is contradictory to it. Marine larvae, to which his theory applies and on which he's an expert, have difficult names and affiliations, so imagine a farm parallel. What he has found is that labradors have terrier puppies, which grow up into labradors; and that horses have foals and that camels have foal-like babies. But from this he concludes that there has somehow been adoption, exchange and copying of DNA. Most biologists would disagree.

For example, the dromioid crabs (including the sponge crab *Dromia*) do not have larvae like those of other true crabs, but like those of the (anomuran) hermit crabs; the zoeae of the inachid crab *Dorhynchus* look like other inachid larvae, but have an array of points and spines exactly like those of *Homolus*, in another superfamily. The diversity of larvae among the echinoderm classes nearly parallels that of the adults, but a few species have no planktonic larval forms and very different development. If none of them had planktonic larval forms — and indeed if marine worms, molluscs and flatworms didn't — we would classify them differently and it would make a lot more sense, conforming to our so-called 'basic ideas' of protostomes versus deuterostomes, enterocoely versus schizocoely, primitive versus advanced: that is, the basic theories of 1920s zoology.

Williamson wants to 'rescue' these classical concepts, but at the enormous expense of the radical belief that different forms catch larvae from each other: he believes that the larval DNA programme probably crossed between genera, superfamilies and even phyla, mostly as a result of fertilization by foreign sperms. The receiving organism occasionally had compatible development, expressing what he calls the "paternal programme" before that of the "maternal" species and showing dissonance between larval and adult (supposed) phylogeny. His frontispiece, a respectable pluteus, purports to be from a sea squirt (*Ascidia*) egg fertilized by an *Echinus* sperm. But all other cross-fertilizations that have been described show *maternal* early development; they may show some paternal characters *after* the phylotypic stage when zygote-prescribed messenger RNA begins to be transcribed. Pluteus stage is post-phylotypic, but not by much.

My difficulty with Williamson's hypothesis is this. If the swapping of DNA programmes explains the phylogenetic puzzles of dissonance between some larvae and their adult forms, then one must assume that such programmes are descriptive rather than prescriptive. But these larvae are so similar in the critical characters. If they were a bit different, we could indeed imagine that the same DNA programme, in a different setting, prescribed them. But for us to explain their being exactly alike, DNA must describe final products, which it (mostly) doesn't: DNA is not a description of what you finally get. I would much sooner appeal to atavism and/or to stringent selection of a precise structure for explanation of similar larvae with disparate adults. Once we have to suppose that there is such fine tuning by selection, of course, all the problems can be explained as evolutionary convergence, without any DNA swapping at all. Because DNA is prescriptive, Williamson's idea itself requires such convergent fine tuning; and that renders his radical hypothesis unnecessary.

There are dissonances in lots of other assumed phylogenies. Most biologists would agree that metazoan developmental programmes are very subtle and that we don't understand much about them. So it is not surprising that there are many oddities that don't fit our prejudices. But we must not invent radical theories to hide our puzzlement or save these prejudices, particularly if it means hastily abandoning most of modern developmental biology. □

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Inordinate fondness for insects

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Treatise on Invertebrate Paleontology, Part R, Arthropoda 4, Volumes 3 and 4: Hexapoda. By F. M. Carpenter. Edited by R. L. Kaesler. *Geological Society of America/University of Kansas: 1992. Pp. 677. $87.50.*

IT is widely known that living insects greatly outnumber the rest of the animal kingdom. But their geological history is a mystery to many entomologists and palaeontologists. William Hennig pointed the way forward by promoting amber studies and by critically discussing fossil taxa that he considered particularly relevant to phylogenetic reconstruction. In the same year as his *Die Stammesgeschichte* (1969), an introduction to hexapods (insects in the broad sense) appeared in the *Treatise on Invertebrate Paleontology*, a respected monographic serial seeking to "present a comprehensive and authoritative, yet compact, statement of knowledge concerning groups of invertebrate fossils". This followed earlier discussion by Frank M. Carpenter, Raymond C. Moore and Ernst Mayr that an insect *Treatise* would be of great benefit: the promised volume (now doubled in size) has finally arrived.

It is a truly remarkable achievement that a single author has concisely diagnosed and checked the type species of more than 5,000 genera, of which nearly 1,200 are illustrated. These taxa are distributed in no fewer than 38 orders (10 extinct), spanning some 390 million years from the lower Devonian to the Holocene (the past 10,000 years). The bibliography runs to more than 2,400 references. Only at one point does the author's energy flag — in the taxonomically difficult order Blattodea (cockroaches). Stratigraphical ranges are not taken to stages of geological time, but this highlights the unresolved problems of international nonmarine correlation.

The main (acknowledged) drawback is that the literature cut-off date is the end of 1983. So only 777 families are included, although the total identified exceeded 1,080 in 1991 (*Fossil Record 2* edited by M. Benton, Chapman and Hall, in the press). Also, the phylogeny draws inevitably on Niels Kristensen's earlier (1981) review. A generic supplement is already a serious consideration; integrating the systematics of extinct and extant insects in a unified phylogeny will take a little longer, however.

Since its inception, the *Treatise* has been an essential work for all scientific institutions with an interest in the fossil record and evolutionary biology. After a long and successful career at Harvard University (his research students included E. O. Wilson), Carpenter has produced what is undoubtedly his *magnus opus* in his ninety-first year. What's more, it's a database of past insect biodiversity that will service biology and geology into the twenty-first century — when more insects than ever might join the fossil record. □

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