and by Côté, but most of the chapters summarize work which has all now been published in journals.

The advantage of this book is that it gathers together contributions from all of the leading authorities in the field of genomic imprinting, and as such it gives a comprehensive coverage of almost all aspects of imprinting. So it is good to have on your bookshelf if your laboratory or institution has an interest in imprinting, as a quick reference to a rapidly expanding field. And that is the problem with this book and all similar conference proceedings; in a rapidly moving field any collection such as this is outdated before publication. So publishers, why not give these proceedings a rest?

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Evolutionary Change and Heterochrony. Kenneth J. McNamara (ed.). John Wiley and Sons, Chichester. 1995. Pp. 286. Price £50.00, hardback. ISBN 0 471 95837 9.

Evolutionary genetics is missing something. From information about the selective landscape and the available variation it can do a pretty good job at understanding what will (and did) occur. But what variation is expected? This is the theory that is missing. A science of the creation of variation, a theory of mutation, is needed. Without it we have little hope of a full understanding of the shapes of things to come or of those past.

McNamara identifies this problem from the outset. Discussing the much-considered Darwin's finches he sums up the issue asking 'what intrinsic factors operate to provide a wide variety of bill shapes on which natural selection can work?' So it was with considerable excitement that I read on. Was I finally to be told how to think about the creation of variation? Regretfully not.

For much of the book the authors show only that heterochrony (changes in the relative time of appearance and rate of development for characters already present in ancestors) is important for this feature or that, in this clade or that (usually vertebrates but invertebrates get a look in, as do plants). Like proud parents disturbed that their child is not getting the attention it deserves, they push it onto centre stage and are concerned that we (the reader) should be impressed by its achievements: isn't it big, hasn't it grown, isn't it important, doesn't it explain so much! So proud are they that some of the authors descend to nonsensical hyperbole: phenotypes rather than genes, we are told, are what evolve.

The insistence on employing the method of proof by listing becomes all the more tiresome when the lists are split and split again into forms of heterochrony each given their own pigeon hole. The difficult language for the categories, rejected as unimportant by the better authors, only obscures the scene.

Hyperbole and semantics aside, the listing of examples impresses one that heterochrony is important to some degree. But so what? How does this help us approach a theory of mutation? The most important function of a theory of mutation is to tell us about what did not happen, rather than filling in the mechanism of what did. If all variation was available then we do not much need a theory of mutation. We can then understand the pattern of evolution independent of processes generating variation. Similarly, telling me how an optimal state (e.g. the loss of Trilobite eyes in dark water - discussed by Raymond Feist) was achieved is then a bit like telling me that Darwin wrote the *Origin* in pen rather than pencil - the end result would be the same and the means only a curious detail.

The importance of understanding variation is being able to know what conceivable mutants might not actually be possible. Evolution may not be able to go down some routes even though from selectionist theory we might have thought that it would. More subtly, variation in the rate at which viable variants are produced may explain why certain characters change faster than others.

It is in this context that evolutionary developmental biology becomes more than a list of examples. It tells us, for example, that genomic imprinting in mammals puts a block on the evolution of asexuality (paternal inheritance of organelles in gymnosperms may well do the same). The question then, that I hoped this book would ask, was whether there is something about development that makes heterochronic changes harder or easier. That such changes occur is all but inevitable, identifying instances is a first step, but understanding why they occur at high frequency (assuming they do) gets to the heart of the issue.

Unfortunately few of the authors devote much time to this question. Volker Mosbrugger is unusual in devoting two paragraphs to asking why heterochrony was important in his clade (land plants). McNamara notes that heterochrony underlies the evolution of sexual dimorphism and asserts that this is probably due to changes in expression of only a few genes (his case is far from proven). This 'few genes' hypothesis is one that a few others mention briefly. Most of these,

however, just suppose it to be true with no proof (in the case of changes in trilobites it is hard to see how the assertion could be proven!). Moya Smith is an exception in going into some of the details of the molecular genetics of her system (tooth development).

Here then perhaps is the greatest irony. As the authors seem to concede, for the most part, to understand why some developmental processes are flexible and others not, we will need detailed molecular genetics. The palaeontologists and comparative anatomists, who for the most part are the authors of the 13 chapters, cannot hence provide the answers. Whilst it was they who first asked whether heterochrony may explain the shape of things past, this book seems more a testimony to their dated approach, than the shape of things to come.

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