Book Reviews

The Semantic theory of evolution, M. Barbieri. Merwood Academic Publishers, New York. 1985. Pp. xii + 188. Price \$39.00 US ISBN 3 7186 0243 1.

Recent years have seen a growing dissatisfaction with the modern synthetic theory of evolution among many biologists, and there have been a number of publications expressing criticism and seeking to announce the arrival of a new and powerful evolutionary theory to replace or subsume the current model. Barbieri's book might be seen as a further addition to this list.

Viewed as an elaboration of the author's account of the origin and early evolution of cells (Barbieri, 1981), this book is highly recommended for anyone wishing an innovative perspective on cellular and pre-cellular evolution. Viewed in the light of claims that it attempts to present a new general theory of evolution, however, the book is disappointing.

In the Foreword, Rene Thom states that it "reads like a novel", and this is both an attraction and an irritation of the book. Considerable space is devoted to introducing basic concepts (particularly in Chapter 4, concerned with the genetic code and the Central Dogma). The reader is not required to have much previous knowledge of biology.

Barbieri reports in the Introduction that the book was originally to be called *The ribotype theory of evolution*, and the bulk of the contents are concerned with laying the ground for the exposition of that theory in the penultimate chapter. Karl Popper is credited with pointing out that the original title "was not entirely satisfactory". In my view, if most of the first chapter and all of the last are removed the original title would do nicely and the book would be considerably more coherent.

The key to the ribotype theory is the proposal that the cell should be viewed as a "trinity" of genotype, ribotype and phenotype. The term, ribotype, refers "to the system formed by all the ribosoids of a cell" (ribosoids being "all molecular systems which contain the sugar ribose"). The origin of replicating systems, Barbieri argues, lies in the origin of the ancestors of the ribotype, and in particular, of the ribosomes.

According to the ribotype theory, the early ribosoids contained "polymerizing proto-ribosomes" (composed of RNAs complexed with small peptides) which randomly polymerised amino acids into polypeptides. Among the assemblage of such polypeptides would be some, Barbieri asserts, that would fortuitously be able to combine with ribonucleic acids to form new proto-ribosomes. Barbieri supports this assertion by the argument that there are today a very considerable diversity of ribosomes, so that there must be a large number of ways of constructing them. Stumbling on an appropriate

set of sequences by random polymerisation is therefore "quite likely". The early ribosoids were, in Barbieri's terminology, quasi-replicating. They contained no genetic information, but relied on the sequences they generated containing a sufficient fraction of functionally appropriate material.

Barbieri's desire to place the ribotype prior to the genotype in evolutionary history requires that the proto-ribosomes are not too improbable to be repeatedly spontaneously generated. The argument mentioned above to support this, however, is erroneous. The fact that there are known to be a "large" number of extant sequences for, say, pyruvate kinase does not justify the conclusion that the *fraction* of all possible sequences with such a function is large.

Barbieri is concerned to keep the complexity of the early ribosoids low. The theory is rather like the mineral theory of Cairns-Smith in that regard: forget coding for the original system, get something simple off the ground, and DNA can take over later. The idea has the advantage that the relationship of the early systems to the modern ones is much clearer than in the mineral theory. The disadvantage is that the minerals are assumed to have replicated their information content, and so could be subject to natural selection, the early ribosoids could not.

Maynard Smith (1961) introduced the notion of protein sequence space to make the point that for evolution to have produced some modern effective protein sequence from some presumably less effective ancestor, the sequences must be connected by a continuous network in sequence space such that from any arbitrarily chosen point in the network, there is on average at least one adjacent point which is a better or equally effective sequence. Chance has then only to hit that network once, and *if there is replication* then the particular sequence can be improved by selection acting on its descendants. Selection for increasing the function will eventually drive the selected population onto a peak in the space where there are no superior neighbours. The fact that there are many distinct sequences found in nature performing the same function just means that there are a number of such peaks for that function (no conclusion can be drawn about the number of independent networks there are, nor about how large a chunk of sequence space a network occupies).

In terms of the demands being made on blind chance, the natural selection of replicating sequences is far less demanding than Barbieri's ribosoids. Indeed, as has been argued that many of the properties of one good protein, are likely to be good for other proteins too, so that a good pyruvate kinase might be expected to have useful features for other kinase activities, and perhaps also for dehydrogenases, phosphorylases and so on. Similar arguments may also apply to the ribosomal poly-

peptides of early systems. There may be many sequences in a network therefore that have some usefulness for several functions (*i.e.*, sequences that occur in many networks concerned with different functions), so that hitting a sequence once allows (if duplication of sequences is possible) many activities to be perfected from the same starting point. It may be that, in a random collection of sequences, the chances of having a single member of a network present is very small, yet if there ever were populations of polypeptides to select from, with some sequence-replication mechanism, a single or few such events may not stretch credulity too far.

The requirement in traditional arguments for functional degeneracy in protein sequences is therefore not as high as Barbieri requires: a functional space need be entered only once; heredity will help ensure that it is not lost and corresponding continuity of form and function will allow selection to improve the sequence one has initially found. This is by no means a lethal objection to Barbieri's scheme in the absence of any estimate of what the actual degeneracy is, particularly for the relatively short sequences that may be involved, but the lower the requirement, the better.

Barbieri criticises Woese's concept of "statistical proteins" (low fidelity of translation in early systems resulting in a number of related protein products for any given coding sequence), on the grounds that they would lead to an "error catastrophe" in the cell. This seems to be asking to have the cake and eat it. If the degeneracy of protein sequence space is as high as Barbieri thinks, some of the variant proteins produced under Woese's model will have adequate activity/specificity and the cell should muddle along. Furthermore, as long as replication has better than zero fidelity, natural selection can improve Woese's cells. This is not so for Barbieri's ribosoids. This is the critical point for the scheme, for unless a primitive ribosome with low fidelity of translation is very much more complicated than one which just randomly strings amino acids together, a major part of the case for having a non-coding ribotype first disappears. Such a ribosome, after all, has a chance of replicating itself, unlike the "polymerizing protoribosomes" of the model.

I have given considerable attention to the Ribotype Theory model of the earliest systems. It is only fair to say that the subsequent unravelling of the fate of the ribosoids makes enjoyable reading. The arguments on the evolution of both eukaryotes and prokaryotes from what Barbieri calls heterosoids (systems with DNA and 80S or larger ribosomes with associated nucleoli) deserve careful thought. The Ribotype Theory should be taken seriously.

On the question of the book's broader aims, however, there are more grounds for complaint. The uneven depth of presentation in the first and last chapters is a source of irritation which detracts from the otherwise agreeable style. As a minor illustration, we are told in the first chapter of the book (on pages 22–23) that there is an "old analogy" between individuals (cells in a population of microbes, say), cell division and death on the one

hand and species, speciation and extinction on the other and some of the attractions of such an analogy are given. The reader is informed that there is, however, a "price to pay" for this analogy, as it requires the admission "that speciation is as real as cell division is", which seems, to Barbieri, to be a problem. This ontological problem seems to have something to do with our total ignorance "of the genetics and biochemistry of speciation"; it is reiterated that "speciation is still a biochemical mystery". It does not seem to have anything to do with our problems over the ontological status of species for we are assured on page 19 that "it is universally accepted that the phylum is a biological category as fundamental as the species. The anatomical barrier between different phyla is *as real* and as natural as the reproductive barrier between different species" (*my italics*).

The impression is given from this section that nothing is known about speciation at all. For example, no citation is made of any of the major post-war texts which discuss speciation. While on the question of the analogy between individuals and species, I would have liked mention of the work and arguments of those who hold that there is more than an analogy here and that species *really are* individuals. The idea that a general theory of speciation concerned, if I read Barbieri correctly, with the biochemistry (and genetics) of isolating mechanisms is needed before the *reality* of speciation becomes unproblematical is difficult to follow.

By far the major problem with the book is the last chapter. This bears the same title as the book, and is Barbieri's attempt to redress the deficiencies of the synthetic theory. The chapter begins with three short sections devoted to adaptation. The requirement that the concepts of fitness and survival be independent is explained, and, after considering, in the third of these sections, the difficulties of defining a niche without reference to the organisms which occupy it (again with no discussion of any of the literature) it is asserted that such independence is impossible and "Darwinian adaptation" is a tautology. In view of the very extensive discussions of the tautology question in literature available when the book was being written the treatment here is a little brusque.

Like many others seeking to replace the modern synthesis, Barbieri wishes to identify non-local causal agents in biological evolution, and naturally turns to thermodynamics for what might be described as macroscopic evolutionary laws. The "semantic theory" derives its name from an extended analogy with linguistic evolution. The information creation principle of Gatlin (1977) is proposed here as a "third law" of thermodynamics which states that thermodynamically meaningful information may be both created and destroyed. (I take it that this involves renumbering the current third law concerned with the entropy of perfect crystalline substances at absolute zero rather than replacing it!)

The core of the semantic theory is that "natural conventions" (a term borrowed from linguistics) exist, and are processes which result in such information being

created or destroyed. These processes provide a mechanism, according to Barbieri, whereby organisms can adapt to the great "natural cycles" of the organic world. This is very difficult to follow and an example to illustrate the mechanism might have clarified much. The semantic theory retains adaptation to the environment by means of natural selection (though how this is reconciled with its previous dismissal as tautologous is not clear to me) but places the major evolutionary force as these natural conventions whereby adaptation to natural cycles is brought about. Barbieri argues that this is now truly explanatory, as the definition of this kind of adaptation is not circular.

Provocative and readable though the chapter is, it should either have been omitted from the book altogether or expanded very considerably indeed. The reader is warned in the preface "to expect from this book only the bare essentials of a new theoretical framework". These essentials are simply not there. All of the sections need to be much more closely argued than they are. That the author has achieved this to a considerable degree in the bulk of the book suggests that a more careful statement of this position need not have prejudiced the readability of the book unduly (though it would have made it longer and delayed its publication). Although a connection is made to the earlier part of the book (*viz.* that the new theory is justified now that the "genotype-phenotype" model of the cell has been replaced with the "genotype-ribotype-phenotype" model) it is tenuous at best and the chapter sits uncomfortably with its predecessors.

A final word on the price: a slim volume of less than 200 pages should not be so expensive, particularly when the book is not intended as a reference work, but to be read as a "novel". A budget paperback edition, like that which appeared for Crick's *Life itself*, would increase the potential readership greatly.

REFERENCES

- BARBIERI, M. 1981. The ribotype theory on the origin of life. *Journal of Theoretical Biology*, 91, 545-601.
- GATLIN, L. L. 1977. Meaningful information creation: an alternative interpretation of the Psi phenomenon. *Journal of the American Society for Psychical Research*, 71, 1-18.
- MAYNARD SMITH, J. 1961. The limitations of molecular evolution. In Good, I. J. (ed.) *The Scientist Speculates*. Heinemann, London. pp. 252-256.

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The correspondence of Charles Darwin. Volume 2: 1837-1843. Frederick Burkhardt and Sydney Smith (eds). Cambridge University Press, Cambridge. 1986. Pp. xxxiii+603. Price £30.00. ISBN 0 521 25588 0.

This is, quite simply, a magnificent book that will give endless hours of pleasure to anyone interested in the

life, work and times of Charles Darwin. The letters in this volume were written during the 7 years following Darwin's return to England from the voyage of the *Beagle* and, thus, cover what is probably the most important and intellectually fertile period of his life. The 400 letters, more than three-quarters of which appear in print for the first time, record the full range of Darwin's activities and interests during this period, including his engagement and marriage to Emma Wedgwood; house hunting in London and Kent; his election to Fellowships of the Geological, Royal and Zoological Societies; the publication of his *Journal of researches, the structure and distribution of coral reefs* and the *Zoology of the voyage of H.M.S. Beagle*; and, though less prominently, the beginning of his work on species and their origin.

This is the second volume of an edition which is intended to provide complete and authoritative texts of Darwin's correspondence. Because the editors wish to provide a sound basis for future scholarly work on Darwin and his correspondents, each letter is accompanied by footnotes which are intended to make the contents of the letter clear, to relate it to other letters and documents on the same subject, and to provide the reader, who wishes to pursue the subject of a letter, with references to other information to allow him to do so. The volume also includes a biographical register to enable the reader to identify each of the many people mentioned in the letters, as well as a comprehensive bibliography and index.

There is no doubt that the editors have succeeded in meeting these objectives or that this volume achieves the highest level of scholarship. At the same time, the book can be read at many levels and is, therefore, as accessible to the general reader as it is to the specialist. Those interested in Darwin's scientific work will find much to interest them in this volume. Thus, there are letters to and from Lyell and other geologists on coral reefs; the origin of erratic boulders, which at that time, Darwin and Lyell supposed had been transported by floating ice from their original location, rather than by glacial sheet ice; and the origin of the parallel terraces of Glen Roy, which, again, at that time, Darwin believed were a succession of beaches formed as the land rose from the sea. There are a couple of letters from James Sullivan, one of Darwin's shipmates on the *Beagle*, giving accounts of the geology and fauna of the Falkland Islands. Many of the letters in the earlier part of the volume concern Darwin's activities in organising the description of the zoological material collected during the voyage of the *Beagle* and the publication of this work in 19 quarto issues. With the help of a number of members of the scientific establishment, including Henslow and Whewell, he obtained a Treasury grant of £1000 to defray the cost of the engravings which illustrated these publications. Those who feel that too much of their time is taken up with writing and refereeing research grant applications, will, no doubt, be interested to learn that Darwin obtained this grant by writing to and being interviewed by the Chancellor of the Exchequer of the day, Thomas Spring Rice!