Letters

Macroevolution Conference

A number of speakers at the macroevolution conference held from 16 to 19 October 1980 at the Field Museum (Research News, 21 Nov. 1980, p. 883) claimed that major portions of the modern synthetic theory of evolution have been put in doubt by recent work on macromutation and punctuated equilibrium. Roger Lewin's article gives the impression that skepticism concerning these claims was expressed by a minority of the participants. In fact, many (perhaps most) of those present remained skeptical, and the proportion of doubters within evolutionary biology as a whole is almost certainly higher than that seen at the conference. Lewin admits that he risks "doing violence to the positions of some of the people at the meeting." True to his word, he presents a simplistic caricature of the modern synthesis, renders condescending judgments on its defenders, and repeatedly gives the last, longer, and stronger word to the advocates of saltationism.

The bias in Lewin's account is especially evident in his choice of quotations and in the interpretations he puts on those quotations. The saltationist view is represented by numerous quotes from Gould, Vrba, and others. But proponents of the synthetic view appear in quotes rarely, and then only as complainers. We never hear them explaining their views. For example, Lande's talk on the genetic basis of phenotypic change is dismissed in a single sentence as an unsuccessful attempt "to persuade his audience of the more traditional view. . . ." Lande's work is original and highly relevant to the questions addressed by the conference. It is traditional only in that it ties explicit, detailed models to hard data. As far as we can tell from reading Lewin's article, defenders of the modern synthesis base their views more on blind faith than on reason. Lewin writes, "Ledyard Stebbins (one of the architects of the Modern Synthesis) feels there is little to be explained at all by species selection or the Effect Hypothesis, adhering as he does to the gradualist position." Two sentences later a summary statement by Stebbins is dismissed as "a polarized view of what actually transpired." No saltationist is given such treatment.

We believe that the current debate on macroevolution is useful and healthy. We also believe that there is plenty of room in *Science* for advocacy of particular positions: there are letters, book reviews, and above all, scientific reports. The partisans can slug it out in those places, where they have the responsibility (and the opportunity) to justify their views with logic and evidence, and where it is understood that biases are being expressed. Within the scientific community and within the public at large, people look to *Science* for information on developments outside their own areas of special competence. Neither constituency is served well when advocacy is disguised as "news," thus evading the usual conventions of scientific discourse. In taking it on himself to arbitrate a scientific debate, Lewin has encouraged widespread misunderstanding of a particular set of issues and, more generally, of the way science actually works.

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Lewin portrays the Darwinian and "Modern Synthetic" view of evolution as one in which evolution "moves at a stately pace, with small changes accumulating over periods of many millions of years yielding a long heritage of steadily advancing lineages. . . ." This is a serious, but unfortunately commonplace, distortion of Darwin and the Modern Synthesis.

Darwin claimed that adaptive changes induced by natural selection are gradual, but he clearly pointed out that this does not preclude sudden transitions appearing in the fossil record due both to the effect of time scale (for example, the classic example of gradual adaptive change—industrial melanism—would appear as a sudden transition if we observed the population only at intervals of 200 years, corresponding to an extraordinarily complete "fossil" record) and due to the fact that new adaptations initially evolve in a local population and then spread rapidly throughout the rest of the species so that "they appear as if suddenly created there, and will be simply classed as new species" (*1*, p. 357).

Consequently, Darwin's meaning of the term "gradualism" was certainly compatible with the sudden appearance of forms in the fossil record—even for a very complete fossil record. Hence, the issue is not adaptive gradualism, but rather whether adaptive gradualism occurs continuously and slowly over long periods of geological time as the quote of Lewin given above implies. Darwin clearly pointed out that natural selection more often than not is a force preventing evolution and that only under relatively rare circumstances would it lead to episodes of adaptive change. He certainly did *not* embrace the view that adaptive changes are continuous over long periods of geological time. To illustrate this, juxtapose the following quotes from Darwin (*1*, pp. 357, 373) with the quote from Lewin given above:

> Many species when once formed never undergo any further change but become extinct without leaving modified descendants; and the periods, during which species have undergone modification, though long as measured by years, have probably been short in comparison with the periods during which they retain the same form.
>
> . . . A number of species, however, keeping in a body might remain for a long period unchanged, whilst within the same period several of these species by migrating into new countries and coming into competition with foreign associates, might become modified; so that we must not overrate the accuracy of organic change as a measure of time.

Similarly, the Modern Synthesis is often treated as if it were a single, unified view of evolution, yet as is evident to anyone who has read and contrasted the works of Fisher, Haldane, and Wright (three of the principal contributors to the Modern Synthesis from the population genetics viewpoint), there never was a single evolutionary theory. Moreover, Fisher (*2*) discussed why many speciation events and morphological transitions follow a pattern of stasis punctuated with "sudden spurts of change"; Haldane (*3*) explicitly stated that the gradual, continuous changes in population genetics would occur "on a geological time scale, almost explosively"; and Wright (*4*), in his shifting balance theory of the 1920's and 1930's, explicitly stated that natural selection was an insufficient

*The order of names is alphabetical.

explanation for adaptive evolution and predicted that adaptive evolution would be characterized by periods of stasis interspersed with episodes of rapid adaptive transition. He also explicitly discussed the macroevolutionary implications of his theories, which are quite incompatible with the stereotyped "Modern Synthesis" presented by Lewin and others.

In summary, the macroevolution meeting at Chicago was not so much an historic challenge to evolutionary theory as it was a challenge to the history of evolutionary theory.

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References

1. C. Darwin, *The Origin of the Species* (Modern Library-Random House, New York, 1936).
2. R. A. Fisher, *The Genetical Theory of Natural Selection* (Dover, New York, 1958), p. 153.
3. J. B. S. Haldane, *Am. Nat.* **71**, 337 (1937).
4. S. Wright, *Proc. Am. Philos. Soc.* **93**, 471 (1949).

Reporters at the Chicago macroevolution conference, including Lewin, apparently missed what was really happening there. The fossil record says eloquently that profuse evolution has indeed occurred over millions of years, but the data just aren't sensitive enough to analyze evolutionary kinetics. This is the province of the evolutionary geneticist who works with descent and change in populations of present-day organisms. Very simply, we have abundantly demonstrated that evolution can be *either* jerky *or* gradual depending on the circumstances and the genes concerned. So what is all the fuss about? Forty years ago, the modern followers of Darwin (Fisher, Haldane, Wright, Dobzhansky, and Mayr) stole the evolutionary spotlight from the paleontologists. This conference saw an attempt by a few fossil zealots who are able to charm reporters to regain attention. Most unfortunately, the ideas they used have neither data base nor innovation.

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Lewin summarizes a complex series of formal papers and discussions. As he notes, my particular views on the fossil record as a source for data on certain aspects of macroevolution were not to the liking of some of my paleontological colleagues. In the brief comments on this point, however, my position seems to come through as an intonater of the "ancient lament" on the incompleteness of the fossil record. The level of incompleteness of which I spoke can be improperly inferred from the following quoted sentence: "I take a dim view of the fossil record as a source of data" and the reply by John Sepkoski: "I'm tired of hearing about the imperfections of the fossil record." Although tiredness may hardly be a basis for rejection, I believe that both of these remarks, appearing out of context, fail to carry the sense of what was meant and I should like to clarify this.

My presentation was an affirmation of my conviction that evolution must proceed with continuity, involving the derivation of new species from antecedent species. Three hypotheses of the models of the derivation of new species were presented:

1) Phyletic gradualism, by gradual accumulation of small changes (Darwinian);

2) Punctuated or stepped speciation, with moderate morphological disjunction between antecedent species (as punctuated equilibria); and

3) Speciation with major disjunctions between antecedent and descendant species (mechanism unspecified).

I maintained that by and large the fossil record does not provide data necessary to establish an equivalency between "fossil" species and "living" species. If this is the case, it is difficult and misleading to infer microevolutionary changes from the temporal or geographic sequences in the record at both the infra- or interspecies level. Although both the first and second hypotheses may, and in fact likely do, express modes that exist, the fossil record itself is insufficient to falsify either one. This is the level of incompleteness with which I was concerned, far from denying the value of the fossil record in other aspects of evolutionary investigation. The third hypothesis is supportable in general if data are less than critically analyzed, but can be falsified by many particular instances as long as morphospecies are accepted as a sufficient basis for interpretation, which I consider to be the case at this level. The hypothesis cannot, however, be totally falsified by the contradictory cases alone, both because the record is insufficient for such a generalization and because it is by no means clear that only a single mode of change exists. Biological investigation of existing species can, I believe, provide a more adequate basis for support or falsification of this hypothesis.

Finally, the matter of "species" stasis, a subject of the conference, is sup-

ported by a great many well-documented cases in the fossil record, if what is meant is the stability of morphospecies over long periods of time. Rarely, however, are the data of the record sufficient for interpretation of microevolutionary changes within these lineages or determination of such consequences as physiological, reproductive, or mechanical changes and similar modifications which may affect functional, populational, and ecological aspects of the morphospecies of concern. It was an exhortation to reasonable caution in these directions that was the thrust of my remarks. I do not, in fact, think that a great gulf exists between me and my colleagues on this matter, nor have I been, as might be inferred from Lewin's article, a diehard proponent of evolutionary gradualism or sufficiency of explanation by synthetic theory.

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Thank you for Lewin's "Evolutionary theory under fire," a fine article that vividly describes the self-correcting manner by which scientific knowledge progresses. The choice of title, while obviously designed to draw attention to the proceedings of an important symposium, is unfortunate because it suggests that evolution is being challenged instead of pointing to the reevaluation of the mechanisms by which organic evolution proceeds. As a result, this article is undoubtedly destined to enter the out-of-context arsenal that has become a mainstay of recent creationist literature.

We are sure the creationists will be delighted to have an opportunity to cite *Science* in apparent support of their cause.

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Lasers in Space

In Nicholas Wade's briefing (News and Comment, 9 Jan., p. 148) about my recent study (with Kosta Tsipis) of laser weapons (*1*), I was identified as an "MIT physicist." Although I was at the Massachusetts Institute of Technology when the study began, since September 1979, I have been at Carnegie-Mellon University.

Although Wade's review accurately summarizes selected points of our report, his comment that a carbon dioxide laser is the Pentagon's leading candidate for a space-based laser weapon deserves clarification. We did not make such a statement. In fact, we did not discuss Department of Defense plans at all; instead, we discussed general constraints applicable to all laser weapons, and we postulated and criticized several weapons and scenarios of our own invention.

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References

1. M. Callaham and K. Tsipis, *High Energy Laser Weapons—a Technical Assessment* (Department of Physics, Massachusetts Institute of Technology, Cambridge, 1980).

Perhaps I am prejudiced because I was employed at Avco Everett Research Laboratory in Massachusetts where a breakthrough to truly high-power lasers was made, but I must take exception to Wade's caustic "A cooler look at laser weapons."

While it is true that atmospheric propagation problems may hinder the use of laser weapons on the battlefield or at sea, their use in space is another matter entirely. Laser or particle beam weapons may be effective defenses against ballistic missiles and may provide a defense against the hydrogen bomb terror that we have faced since such missiles became operational.

There are severe technical difficulties in fielding a network of operational laser-armed antiballistic missile satellites, to be sure. But if current trends are followed, such weapons systems may be undergoing orbital tests before the decade is out. Certainly they will be tested by the Soviet Union, which has shown a continuing interest in developing space-based weaponry, such as their antisatellite systems, and in bringing such weaponry into use. . . .

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Erratum: In the article by C. E. Land (12 Sept. 1980, p. 1197), the labels for the curves P(negative estimate) and Power in the left-hand panel of Fig. 1 (p. 1199) should be reversed; the data sources for table 1 (p. 1200) should have been given as (*19, 25*).

Erratum: Photomicrographs of normal and sickled erythrocytes that were published in the 30 January issue (Research News, p. 469) should have been credited to the laboratory of Patricia Farnsworth, Department of Physiology, New Jersey Medical School, Newark. The photos were taken by graduate student Patricia A. Burke.