Most papers are, however, primarily serious but with lighter touches, such as the Molecular Trojan Horse that illustrates the article by Dimitriadis on the transformation of eukaryotic cells, and perhaps most of the contributions would not seem particularly out of place in a more conventional book. (Each contribution has been photographically reproduced from the original typescript, but none is difficult to read.) There are some 40 articles in all, from which much can be learned about liposome lore. Anyone who has been personally involved with liposomes, or who knows workers in the field, will also be interested by the anecdotes. Those who have not might find them slightly tedious. The unusual format has, however, encouraged authors to be more forthright than usual, and this is a valuable feature in itself. For example, to quote finally from George Poste:

> I was highly skeptical (and remain so) about the feasibility of targeting liposomes to tumor cells in solid neoplasms and their metastases *in vivo*. . . . My criticisms are directed only to those proponents of targeting who chose to ignore the substantial problems created by anatomic and physiologic factors *in vivo*. . . . Overselling always carries the danger that real opportunities may be lost because of skepticism elicited by earlier extravagant claims which have failed to come to fruition.

Although *Liposomes* edited by Ostro is of comparable size to *Lipsome Letters*, and was published at about the same time, it appears to have taken longer to produce perhaps because it is not based on camera-ready copy. There are eight contributions, five of which are from the pens of authors who also appear in Bangham's volume. In Ostro's book, however, their contributions are basically more serious (influenced by Bangham, I nearly wrote more prosaic), or rather all of them except Bangham's own short historically-orientated chapter. The editor has attempted to assemble reviews of various areas of liposome research in such a way that the volume represents a compilation of the major work in the field since 1965 (up to, as far as I can see, fairly early in 1982).

In the preface Ostro (whose own affiliation is The Liposome Company) comments that 30% of those attending a recent meeting on liposomes were affiliated to commercial laboratories. This is a useful laboratory reference book and it similarly reflects current interest in the practical and commercial applications of the targeting of liposomes as delivery systems. Thus the three chapters (liposomes as a tool in molecular biology; immunologic aspects of liposomes; therapeutic applications of liposomes), which are most concerned with various aspects of this topic, occupy considerably more than half of the volume. □

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Catastrophe, chaos and delay

Athel Cornish-Bowden

Mathematical Models in Molecular and Cellular Biology.
Edited by Lee A. Segel.
Cambridge University Press: 1983. Pp.768. Pbk £15, $29.95.

WHY is the story of the mathematician who opened a lecture with the words "Consider a spherical elephant" not funny? Biologists need to understand this before they can appreciate the value of mathematics and overcome their suspicion of it. Some caution is justified, as there are many who seem to regard numbers and equations as a convenient shield for the poverty of their ideas, but mathematics has made considerable contributions to biology and deserves to be taken seriously.

To give one example: a valid theory of metabolic control cannot be developed without a mathematical framework that extends beyond the elementary kinetics of isolated enzymes; in consequence, biochemists who reject a mathematical approach continue to be misled by oversimplified ideas, such as "bottleneck enzymes", "controlling steps" and so on. In some areas of biology it is easy to check by experiment whether a mathematical theory makes correct predictions, even if one does not understand the mathematics. Metabolic control, however, is difficult to study by experiment (other than by computer simulation), and so one cannot easily convince the sceptic that metabolic fluxes must be largely unaffected by moderate changes in the concentration of almost any enzyme. In such cases scientists who deny themselves all but the most elementary mathematics are also denying themselves understanding.

Anyone genuinely interested in learning how mathematics is being applied to research problems in biology can hardly do better than begin with *Mathematical Models in Molecular and Cellular Biology*, a book first published in 1980 and now available in paperback. Dr Segel has taken his responsibilities as editor seriously and has produced a real book, not just a haphazard collection of papers. Indeed, the individual authors are given so little prominence that their names do not appear at the beginnings of their chapters.

I particularly liked the obvious efforts taken to support the mathematics with a readable text and to make it believable. For example, the information that the equation $x_{t+1} = rx_t(1-x_t)$ can generate chaotic (aperiodic) behaviour is accompanied by the instructive suggestion that "the reader simulate the difference equation on a hand calculator to see this". After doing just that, I found it much easier to accept the important biological message that "phenomena that look chaotic may come from simple underlying laws". As the author of that chapter remarks, "the implications . . . are most unsettling"!

I was disappointed to see that population genetics is mentioned only in passing, but this was the only major omission that I noted. Some other topics, such as elementary thermodynamics and enzyme kinetics, are also dealt with briefly or not at all, but there would be little point in filling a book of this kind with the staples of general biochemistry texts.

Catastrophe, chaos, delay. . . . It will be evident by now that the title of this review is not a comment on the book: it is inspired by the editor's list of some of the mathematical concepts to be discussed in a biological context, but there is much else to interest anyone with a lively mind. □

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Sex in society

Stuart Sutherland

Men and Women: How Different Are They?
By John Nicholson.
Oxford University Press: 1984. Pp.193. Hbk £9.95, $19.95; pbk £2.50.

ONE OF the more curious aspects of social research is that it so rarely produces definitive results. This is especially true in the area of sexual differences and their origins: *Men and Women* is a rather cursory survery of the evidence on this topic. It is intended as a popular book and rarely cites primary sources or gives enough details of experiments and surveys to allow the reader to judge for himself — or, one should perhaps add in this context, herself. John Nicholson appears to be bent on showing first that sex differences are either non-existent or small, and second that where they exist they are caused by socialization not by genetic effects. His arguments are often cunning, but that does not prevent many of them from being fallacious. Here are some examples.

In attempting to disprove the existence of an innate maternal drive, he uses as a counter-argument baby-battering by mothers. This is like arguing that hunger is not largely innate because some people are anorexic. Again, he argues that even in animals the maternal drive is a weak one, because it often disappears if the mother is separated from her offspring for a short time. But the development and maintenance of many innate drives are known to depend on the presence of the appropriate stimulus. Given the importance of maternal behaviour for the survival of the species, it would be remarkable if no genetic