is very short and makes little reference to archaeology) and in some cases the authors seem more accustomed to writing about states than about their collapse. The attention to detail exposes some weaknesses of theory: thus Millon's discussion of the end of the great city of Teotihuacan, in the valley of Mexico, is very speculative, while Bronson's treatment of the role of 'barbarians' in the fall of states would have benefited from an analysis of proximate as opposed to ultimate causality.

Like Tainter, the authors in the edited volume concentrate on collapse as a process of political fragmentation. There is a comparable dissatisfaction with the empirically untestable 'models' of Spengler and Toynbee. Further similarities can be seen in the recognition that state societies are atypical in human evolution, being a very recent phenomenon, and that we have overestimated the degree to which they were self-regulated. Although none of the contributors proposes the same model for collapse as Tainter, it is clear that the relationship between the costs and benefits of complexity is central to the thinking of at least two of them. Thus Yoffee writes that "stability in historic states and civilizations is maintained when the periphery considers that the resources it provides the center also return benefits to itself", while Cowgill considers that one of the main problems faced by large states is how they managed to "balance their costs and expenses when they could not simply capture resources from others".

Differences of emphasis are also clear between the two books. There is a stronger condemnation of biological analogies for cultural evolution in Yoffee and Cowgill (see Yoffee's introduction), and McC.Adams argues that there is no equivalent of extinction in human cultures. Indeed the same author goes further to argue strongly that collapse of states and civilizations is neither absolute, nor inevitable and determinate. We should not treat states as 'stable', but rather consider, as a problem worthy of study, how they cope with the problems of short-term instability. Once again, the equilibrium model of complex cultures is seen as inappropriate.

Perhaps the clearest comparison between the two books can be made by reading Cowgill's excellent discussion of the ways in which, in the social sciences, we conceive of problems such as collapse, and how we attempt to explain these problems. Cowgill contrasts general, highly abstract theories with particular, detailed analyses of individual case-studies. He prefers 'scientific' explanation, but argues that we need to develop "models that achieve high outputs of usefully accurate predictions and postdictions of significant phenomena in return for relatively economical inputs of relevant data and parsimonious theory". What would be fascinating would be to hear his views on Tainter's 'explanation' of collapse, which, I suspect, he would characterize as being too abstract to account for variation in particular observations. How do states vary in rates of collapse, how far do other institutions fall apart along with political fragmentation, how far does collapse have a deleterious effect on central and more peripheral populations, why was collapse beneficial to some peripheral populations and not to others, and so on? These are the kinds of questions with which Tainter is less concerned, but any theoretical approach which cannot accommodate them will be seen as less than successful.

Are there any lessons to be learnt from these two books? Clearly the ways in which we conceive of early states and their collapse have changed, at least in part because of the input of new data from archaeology. Nineteenth-century views of collapse were over-dramatic — they overestimated the stability of early states, they employed what we would now see as rather simplistic, biological analogies for the rise and fall of civilizations, and the data on which they were based were restricted essentially to the main political centres. Archaeology, in alliance with other social sciences, is now working with an infinitely larger database and using different theoretical approaches, enabling, for example, a comparative analysis of change in political and other cultural institutions. Our very conception of collapse, let alone its explanation, is sufficiently different as to nail, once and for all, gloom and doom analogies drawn from the past to predict the future. □

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Clarifying chaos

Michael Berry

Mathematics and the Unexpected. By Ivar Ekeland. *University of Chicago Press: 1988. Pp.146. $19.95, £15.95.*

THIS century has seen three revolutions in the science of dynamics. The first two were relativity and quantum mechanics, which changed the laws of physics under conditions of speed and size far removed from our direct experience. The third is chaology, which left the laws of motion (Newton's) unaffected but has radically altered our understanding of the behaviour they describe. It is this revolution that is one of the two themes of this book.

Ekeland's treatment is both philosophical and historical. In the first part of the book he describes the old newtonian–laplacian view of mechanics, in which the reversibility and uniqueness of solutions of the equations of motion were taken to imply that events (abstracted as orbits of bodies under forces) are predictable, and "Past and future are seen as equivalent, since both can be read from the present. Mathematics travels back in time as easily as a wanderer walks up a frozen river".

The shattering of this interpretation by Poincaré's discoveries and their recent extensions and widespread applications is discussed in the second and central part of the book. By carefully chosen examples, in which the mathematics is explained lucidly and with the minimum of technicality, the main message is reached: " . . . a purely deterministic law may materialize in a totally random series of observations if part of the information is withheld, as it must be in any practical situation. . . . Like the queen of England, determinism reigns but does not govern".

There are some errors. It is not the case that "Dissipative systems cannot have complicated trajectories": when forced, they can, and the trajectories explore the 'strange attractors' whose most familiar manifestation is the unpredictability of the weather. It is not true that the appearance of the Feigenbaum constant "in many different circumstances has been one of the great scientific puzzles of past years": Feigenbaum's discovery of the constant was accompanied by his mathematical explanation of its universality. And Arnold's cat is confused with the baker's transformation, which is a different chaotic mapping. These mistakes mar but do not ruin the exposition. They are more than compensated for by an elegance and directness of expression.

My enthusiasm does not extend to Ekeland's treatment of catastrophe theory, which is his second theme. Thom's grand vision of a library of nature's forms is explained clearly enough apart from minor technical errors, but the discussion is a decade out of date. It is no longer true that "there has not been a single undisputed success of catastrophe theory in the field of experimental science". A flourishing new branch of optics has been created, which has furnished quantitative explanations of many natural phenomena and stimulated a variety of experiments. The author seems unaware of this, and of Arnold's enormous extension of Thom's classification (far beyond the seven dimensions at which Ekeland asserts that the theory fails).

This is a Jekyll and Hyde of a book, on which the verdict has to be that the chaology deserves applause but the catastrophe theory is inadequate. □

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