Changing concepts of chance

I.J. Good

The Probabilistic Revolution. Vol.1 Ideas in History. Edited by Lorenz Krüger, Lorraine J. Daston and Michael Heidelberger. **Vol.2 Ideas in the Sciences.** Edited by Lorenz Krüger, Gerd Gigerenzer and Mary S. Morgan. *MIT Press:1987. Vol.1 pp.449, \$32.50, £29.25. Vol.2 pp.459, \$32.50, £29.25. Set price \$60, £53.95.*

IN 1982–1983 an international group of scholars assembled for about a year in Germany, augmented by visitors, to discuss the events that “made probability a part of philosophy, scientific theories and practice, social policy, and daily life between circa 1800 and 1950” (but mostly before 1930). These meetings led to four first-class books, one by Stephen Stigler (*The History of Statistics: The Measurement of Uncertainty Before 1900*), one by Theodore Porter (*The Rise of Statistical Thinking, 1820–1900*) and the two books now under review. The nationalities of the authors are American 12, German 7, French 4, British 3, Canadian 3, Hungarian 1 and Finnish 1, but all the chapters are written in English.

There are two kinds of history, depending on whether the historian looks at the events through modern eyes or tries to adopt the point of view common at the period discussed. Both approaches are used in these volumes. Volume 1 concentrates mainly on the nineteenth century, and especially on historical, philosophical and social aspects of probability and statistics, while Vol. 2 deals more with the period 1870–1930 and with the great influence of probabilistic thinking on modern psychology, sociology, physiology, evolutionary biology and physics. Probabilistic thinking entered economic thinking in a big way mainly after 1940.

It is difficult to give a succinct picture of a work of some half-a-million words, 35 chapters and 31 authors. One way is to mention statistics *about* rather than *in* the books, beginning with a Hall of Fame which will perhaps convey some impression of the contents.

The following list gives the number of pages on which each of 18 stars is mentioned: Laplace 90, Quetelet 81, R.A. Fisher 59, Darwin 51, Fechner 43, Dobzhansky 38, Sewall Wright 37, Boltzmann 31, Poisson 30, Egon Brunswik 24, Maxwell 24, Wilhelm Wundt 24, Richard von Mises 23, Galton 22, Niels Bohr 21, Cournot 20, Einstein 20 and Gustav Radicke 20. The numbers of people mentioned respectively on r pages ($r = 1, 2, \dots, 19$) are 391, 113, 65, 40, 29, 18, 15, 15, 10, 11, 4, 2, 2, 5, 5, 5, 3, 4 and 2, fitting

well to the familiar formula $S/[r(r+1)]$ where $S = 757$ is the total number of people mentioned. (This formula often fits biological samples and samples of vocabulary, apart from large values of r . For most applications of the formula only small values of r matter.)

The total number of mentions is about 2,788. It follows that the approximate coverage of relevant personalities in the work (that is, in further work on the same topics, the probability that the next name to be mentioned will already have been mentioned) is about $1 - 391/2,788 = 84$ per cent. The expected number of new names in another work of the same size is $S \log_e(4/e) = 292$. So there is still scope for further historical research, but diminishing returns have set in, and it would now be more useful to concentrate on the period 1930 to 1980.

Let me now turn to a non-random sample of specifics. Some of the chapters are directly related to the title of the work and tend to justify it. There has been a philosophical swing from Laplacian determinism to quantum mechanical indeterminism, and a revolution from the epistemic interpretation of probability (as depending on our ignorance) to the interpretation of probability in terms of long-run relative frequency; but the latter revolution has revolved past 180° and is now at about 270° . The epistemic interpretation, thought by some to be dead, refuses to lie down.

Even if the world is physically deterministic, in a somewhat metaphysical sense, from a practical point of view it is indeterministic and we don't need quantum mechanics to teach us that. The analogy with pseudorandom numbers is pertinent in this connection and this analogy is useful when discussing irreversibility in classical statistical mechanics. A single ‘chance’ switch in the dealing out of genes leads to the birth of a distinct person. We are all the products of blind chance. Anyone over 60 can reasonably claim, with seeming illusions of grandeur, that if his or her actions had been different when young none of the people now under 20 would ever have been born.

Hacking says

The marvels of modern medicine produce modest increases in life expectancy ... compared to that ... provided by the sanitary movement and its band of ... statisticians. Most people like myself, whose ancestors lived in an urban slum sometime in the nineteenth century, would simply not *exist*, had there not been the sanitary movement. Our ancestors would have died before breeding.

He omits to add that we are all products of chance anyway.

There are three chapters on scientific revolutions and paradigms. For example, Hacking says “*Don't look for a big revolution until you find new kinds of institution that epitomize the new directions created*

by the revolution” (his italics). Another sufficient criterion for a revolution is when an early article in a field is originally rejected and is later regarded as pioneering.

None of the authors states the historical hypothesis that paradigms form a rough hierarchy, with those at the top tending to last longer than the sub-paradigms lower down. (For the interpretations of ‘paradigm’ see Margaret Masterman's article in *Criticism and the Growth of Knowledge* Vol.II.) This hypothesis, even if vague or false, suggests a programme of historical and philosophical research.

I was especially struck by Stephen Stigler's chapter in Vol.1, partly because of its brevity and clarity, and because it contains a joke (the only other suspicion of a joke that I noticed was Ian Hacking's use of the lovely word ‘probabilification’). Stigler says

I ... can count on the fingers of one hand (indeed, almost upon the thumbs of one hand) the application of probability assessments of accuracy in the social sciences before 1880. ... Together, they [Francis Galton, Francis Edgeworth, and Karl Pearson] helped launch a statistical revolution in social science akin to that which Laplace had launched in astronomy.

Swijtink mentions R.A. Fisher's argument that distinguishes between deductive and inductive thinking. In deductive thinking a conclusion can be reached by using only part of the evidence, but this (in principle) is unjustifiable in inductive thinking. Paradoxically, the precision of the results obtained by the ‘anti-Bayesians’ can be obtained only by ignoring some of the evidence, especially in distribution-free techniques in statistics.

In Vol.2, Mary Morgan says “... probability ideas have been used successfully to plug one gap in the system of economic theory only to find that uncertainty [in Frank Knight's sense, 1921] remains as considerable as ever”. Another way to express this, that some of us would prefer, is to say that subjective probabilities do not typically have sharp values, they are interval-valued. This important aspect of epistemic probability is, I think, not mentioned anywhere in these two volumes.

M.J.S. Hodge discusses explanation in natural selection and mentions that Darwin “conformed his argument to the *vera causa* ideal for a scientific theory”. A *vera causa* is an explanation “known to exist ... and not merely conjectured to exist”. My own concept of ‘explicativity’ is more general because it allows for the probability of an explanatory hypothesis (for example, see a chapter in my book *Good Thinking*).

I found the last chapter, by Nancy Cartwright, especially interesting. She says that the basic attitude of American physicists in the 1920s and 1930s, as contrasted with that of some of their European counterparts, was that “the task of

physics is not to explain but to describe" and she quotes Kemble (1929): "... we may address ourselves to the task of describing what we observed in the most compact manner possible". The question arises why compactness of description is desirable. Is it because it saves expense, or is it because a shorter description is, after all, more likely to lead to a good explanation and to get us closer to an understanding of the world? It would be a great blow to natural selection if its explanatory power were to be disregarded, so it seems that the views of most biologists about the purposes of theorizing must differ very much from those of most American physicists, those in the 1920s and 1930s at least. Cartwright says that her aim is "to document the lack of philosophical perplexity [among physicists]; ... [and] to argue that it was entirely rational". It is similarly entirely rational for bookbinders to ignore the chemistry of gum and to be more concerned, for example, in making books that lie flat. Unfortunately the binders of these books have failed in this respect.

Cartwright says "To ask what happens when observations cannot be made is a meaningless question, ...". I don't know her attitude to virtual particles and to quarks but I have not yet read her book on laws of nature.

The true history of probability, or of science in general, will never be written because so much depends on unrecorded oral communication, and also because writers often do not cite their sources. So all history is to some extent misleading but these two volumes come close to the best that can be achieved on the topics and the period covered. They contain a mine of historical and philosophical material. You can dip in almost anywhere, and be interested, whether you agree or disagree with what is said. Learning about the historical roots of your discipline has much the same kind of interest as learning about your own ancestral roots. □

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Spring books

Nature's next book review supplement, Spring Books, appears in the issue of 21 April.

Among the titles to be reviewed are *Infinite in all Directions* by Freeman Dyson, two on science in the Antarctic, and two others on the history of the Lick Observatory (which celebrates its centenary this year), Virginia White's *Handbook of Research Laboratory Management*, *Smallpox and its Eradication* edited by F. Fenner and colleagues, *H.A. Kramers* by Max Dresden and *Eye, Brain, and Vision* by David H. Hubel.

Among the reviewers are John Stachel, Desmond King-Hele, Nicholas Kemmer, Warren A. Sinclair and R.W. Guillery.

Chaos illumined

Graham Dunn

Chaos in Biological Systems. Edited by H. Degn, A.V. Holden and L.F. Olsen. Plenum:1987. Pp.323. \$62.50 (North America, \$75 (elsewhere)).

MANY biologists are deeply sceptical about the merits of applying advanced mathematical reasoning to biological problems. Until recently, this attitude was largely justified. A leading mathematician and cell biologist once told me that, of four possible categories in the application of mathematics to biology, only one is missing: good mathematics applied to good biology. Even so, mathematical methods have proved essential for the development of biometry as an objective science, and many other applications of mathematics to biology have been far from trivial; the morphogenetic theories of Turing and Thom, for example, may yet become respectable candidates for the missing category. But sophisticated mathematical reasoning has not had a widespread effect, so far, on our understanding of biological processes.

Chaos in Biological Systems may be among the heralds of a dramatic change. Here chaos is illumined, not by flashes of lightning as in the words of Oscar Wilde, but by mathematics. Chaos theory, a topic in the forefront of research into non-linear dynamic systems, is now promising to have profound implications throughout biology and medicine as well as in the physical, chemical and socio-economic sciences. The name of the theory is little more than a decade old although, together with Turing's and Thom's ideas, the roots of chaos theory in mathematics go back at least as far as Poincaré's theory of bifurcations in the late nineteenth century. Turing's pioneering computer programs of 1951 were non-linear models of biological morphogenesis, and it is the recent development of fast and cheap computing that has now enabled mathematicians to begin experimenting freely with non-linear systems of equations.

The hope for biology is that fairly simple non-linear systems may be able to represent the regulatory feedback loops that lie at the core of practically all biological dynamics from molecular interactions to predator-prey relations. Many such systems of equations can be found in the book, and the behaviour of these models is often quite lifelike. Striking spatial patterns can emerge from homogeneous starting conditions and complex periodic behaviour, or even random fluctuations can develop in simple systems — hence the name 'chaos'.

The book is a collection of papers presented at a NATO Advanced Research

Workshop held in Cardiff in December 1986. The first impression on scanning the list of titles is of the staggering range of biological systems that are now thought to show chaotic dynamics. Among the topics represented are pathological heart rhythms, plant transpiration, platelet regulation, receptor binding, enzyme dynamics, slime mould aggregation, morphogenesis, ecology, disease epidemics and population growth, and a large section is devoted to neuronal dynamics. Not all the papers conclude that the dynamics of the system under study are chaotic, but it seems that no level of biological organization can be excluded from chaotic behaviour.

Even so, the book does not exhaustively cover the areas of current interest, and the more involved but more exciting topics, such as control of gene expression, embryogenesis and evolution, are scarcely mentioned. Nor is the book intended as a primer for biologists. Some of the mathematics is heavy going even for enthusiasts. Nevertheless, many biologists will find browsing through the book to be a rewarding experience, though it is a pity that no general introduction or glossary of the whimsical jargon of chaos theory is included; strange attractors, renormalization group algorithms and subharmonic bifurcations all appear on page 1.

Many, if not most, of the main problems in contemporary biology are to do with the dynamics of interactions. As with the famous chemical reaction discovered by Belousov and Zhabotinsky, the rules of interactive behaviour are sometimes fairly well understood but the behaviour that emerges is often unexpectedly complex. What this book shows is that chaotic dynamics are probably widespread in biological systems. If this is true, chaos theory denies the hope that the behaviour of such systems will ever be deducible or predictable from a knowledge of the rules and so the cherished stratagem of biologists, molecular reductionism, may prove ineffective in unravelling these problems. But chaos theory offers the new hope, hardly tenable before, that the underlying dynamics of complex biological behaviour may often be relatively simple and hence understandable.

To show that a biological system can behave chaotically may not seem to say a lot about it — at least to outsiders — but these are early results that might be comparable to the early results in biochemistry of the nineteenth century. As I write, sitting in front of a chart of biochemical pathways, I wonder whether biomathematicians of the next century will emulate the magnificent achievements of twentieth-century molecular reductionists. □

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