BOOK REVIEWS

Big Systems

Choosing Big Technologies. JOHN KRIGE, Ed. Harwood, Langhorne, PA, 1993. xiv, 244 pp., illus. $69 or £36. From a symposium, Florence, Italy, Nov. 1991. Reprinted from *History and Technology*, vol. 9, nos. 1–4.

Building big technologies and technological systems is among the 20th century's most characteristic activities, and technological enthusiasts have come in all political colors. Lenin, for instance, fervently believed that the final triumph of communism over capitalism would be achieved with the aid of electrification and modern methods of large-scale production. However, the role of the nation state in fostering and developing technologies has generated growing criticism in the last few decades. To take two recent scholarly examples, in David Collingridge's *The Management of Scale* (Routledge, 1992) and Loren Graham's *The Ghost of the Executed Engineer: Technology and the Fall of the Soviet Union* (Harvard University Press, 1993), there are accounts of big technologies gone awry for a mix of reasons. In the light of Lenin's expectations for big technologies, Graham's story is particularly telling with its condemnation of Soviet industrial practices.

The former Soviet Union of course is not the only state to have had problems in managing and organizing technological systems, systems that have to be seen not just as engineering and scientific constructs but also as social, political, institutional, and economic constructs. Unraveling how choices regarding big technologies—including whether or not to opt for bigness in the first place—have been made in the past is surely of first importance for understanding the history of the 20th century.

Choosing Big Technologies is a contribution to this end in the form of a set of case studies by historians, policy analysts, and participants in particular programs. All but two of the case studies examine the roles of national governments in the construction and operation of large technologies and technological systems in the 1960s, '70s, and '80s. The emphasis is on technologies for commercial and scientific purposes, and many of the technologies that are included also fall under the rubric of big science. How big a technology has to be to qualify as "big" is not made clear, however, and in general the authors follow the practice of naming a subject and then presenting a series of examples to define it.

The European Space Agency (ESA) and the European University Institute sponsored the conference at which most of the papers in the volume were first presented, and most of the papers are devoted to space topics. The prehistory of ESA in fact forms the chief subject of three of the best papers in the collection. Taken together, they give a vivid picture of the fumblings that constituted the attempts during the 1960s at European cooperation in space projects. This cooperation was to be enacted by two fledgling organizations, ESRO (European Space Research Organization) and ELDO (European Space Vehicle Launcher Development Organization). The driving question for both organizations was how to overcome the big U.S. lead in space activities. For European scientists, the central issue was whether to try to jump straight to the design and construction of scientific spacecraft comparable to the best launched by the United States or to proceed in a more measured fashion and make up ground over the long haul. Both Arturo Russo's account of the Cos-B gamma ray satellite and John Krige's study of the Large Astronomical Satellite center on this issue. The advocates of both satellites were proposing enterprises of a type and scale with which many of them were unfamiliar, and the determination of the design and potential functions of both spacecraft was marked by intense debates and sometimes destructive bickering as new ways of doing business were thrashed out. Michelangelo De Maria and Krige's joint account of ELDO suggests that if you want a model of how not to organize and run a large international technological effort, ELDO would be a good place to start. An organization that grew out of political and industrial concerns, it was technically and managerially naïve, and, most telling for its future, it failed to forge a consensus on how to catch up with the Americans. The ELDO study depicts a starkly different situation from that described in Joan Johnson-Freese's examination of space policy in Japan, where, as she points out, consensus is the key to decision-making; it "is not just a process, it is a philosophy."

The tension between competition and consensus is played out in many of the other papers too. René Collette contends that Europe's space communications systems were fashioned in ways again shaped in considerable part by the perceived need to compete with the United States, and Pamela Mack's nuanced account of the early years of U.S. remote-sensing programs also features competition, this time interagency competition over the development of remote-sensing satellites. But, Mack argues, simple agency self-interest is not sufficient to explain the twists and turns that were involved in the policy-making. The agencies and user communities contained so many competing interests that, without effective mechanisms to produce consensus, the result was poor policy.

International competition also spurred the development of high energy physics in both the United States and Europe, as Dominique Pestre underlines in "The decision making processes for the main particle accelerators built throughout the world from the 1930s to the 1970s." For example, CERN's founders aspired to mount a challenge to U.S. dominance of the field. One obvious way to do so was to build giant particle accelerators, an ambition encouraged by U.S. foreign policy that sought to aid the rebuilding of western Europe as a bulwark against communism and viewed the strengthening of scientific institutions as one means to that end. Robert Seidel too deals with high energy physics, but he concentrates on its early years and so on an era before federal funding came to dominate sponsorship of the field. Seidel focuses adroitly on the technological choices made by Ernest Lawrence and his colleagues at Berkeley in the 1930s in building cyclotrons and on how groups outside the Radiation Laboratory learned to manufacture their own machines (the best answer by far being to import a Rad Lab veteran).

The rest of the papers deal with a range of topics. These include the choices of civil and military aircraft in the United Kingdom and the choices surrounding what to do with irradiated nuclear fuel after its discharge from nuclear reactors. John Logsdon provides a cogent overview of a number of major choices in the history of NASA.

The "messiness" and ad hoc nature of the policy-making involved in choosing big technologies is a striking theme of many of the papers, and such are the obstacles described that it almost comes as a surprise when anything gets built and works as promised. As some recent studies of the space station suggest, for instance, "muddling through" is often the best that can be hoped for with very big technologies developed by nation states. Muddling through is nevertheless in clear contrast to the simplicity of some models of policy-making

Vignettes: Foundations of Economics

Taking a risk is not always the result of a calculation—far from it. Already in economic affairs, as Keynes pointed out, "If human nature felt no temptation to take a chance, no satisfaction (profit apart) in constructing a factory, railway, a mine or a farm, there might not be much investment merely as a result of cold calculation." A little further back he declared that "when enterprises were mainly owned by those who undertook them or by their friends and associates, investment depended on a sufficient supply of individuals of sanguine temperament and constructive impulses who embarked on business as a way of life, not really relying on a precise calculation of prospective profit."

—*Ivar Ekeland*, in *The Broken Dice and Other Mathematical Tales of Chance* (University of Chicago Press)

The moral and ethical content of economics instruction was . . . once much more explicit than now. It has not been absent in the mainstream economics of the past quarter century. Rather, it has been largely unacknowledged, as many modern economists contend that the prevailing paradigm in their field is or could be made to be value-free. . . . Economics, as practiced by an overwhelming majority of economists, is in fact fundamentally based on the value judgment that individual preferences should count in the allocation of society's scarce resources.

—*Malcolm Gillis*, in *Ecology, Economics, Ethics: The Broken Circle* (F. Herbert Bormann and Stephen R. Kellert, Eds.; Yale University Press)

that have been promulgated. One such, for example, is the so-called rational model. Here a particular technology is conceived of as emerging from a process in which the historical actors are aware of a choice of goals and in which the consequences of choosing a certain option can be judged accurately. Making a choice then leads to the technology that optimizes the trade-off between costs and benefits. But the papers in *Choosing Big Technologies* support a large body of other writings that suggest strongly that the rational model is hopelessly inadequate. In his introduction to the volume John Krige eschews general models and suggests instead that the choice of a technology or technological program is the "negotiated outcome of a sequence of mini-decisions taken against an ever changing background of scientific and technological knowledge and of social, political, and financial constraints." When, however, the intended outcome of the negotiations is ambiguous, as in the cases of ELDO and U.S. remote-sensing satellites, the results can be problematic at best.

A few papers in *Choosing Big Technologies* are overburdened by detail and are not lightened by the posing of general questions, the raising of comparative issues, or linkages to broader literatures. In a short introduction and longer afterword, Krige and Roger Williams respectively try gallantly to pull together the various strands, but in the end the collection, as such collections usually do, stands as something of a hodge-podge. *Choosing Big Technologies* nevertheless presents a rich series of case studies and lots of food for thought for engineers, scientists, and policy-makers, as well as those seeking to understand the ways some sorts of big technologies have helped to shape the history of the late 20th century.

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Thoughtplay

Games of Life. Explorations in Ecology, Evolution, and Behaviour. KARL SIGMUND. Oxford University Press, New York, 1993. viii, 244 pp., illus. $49.95 or £30; paper, $17.95 or £9.95.

There is a style of popular scientific writing that draws its narrative energy from the personalities of a few prominent scientists and the drama that flows from their obsessions. The best of this genre are well worth the attention of students and practitioners of science, but these readers are also well served by something a little meatier, in the manner of George Gamow or Erwin Schrödinger in their "popular" mode. Karl Sigmund's *Games of Life* is firmly in this latter tradition, though it does contain a few (quite entertaining) biographical asides.

The book is a semipopular account of theoretical evolutionary biology, with an emphasis on behavioral phenomena and on game-theoretical methods. The tone is genial and playful. Although the book is about mathematical ideas, Sigmund has opted to avoid explicit mathematics (equations). Presumably this is meant to make the book more palatable to a readership of biologists, but there are a few spots in the book where an equation or two would make the argument a lot more transparent.

Sigmund introduces his book with a spirited defense of the use of mathematical thinking in the context of biological problems. He reminds us, for example, that Mendel was a student far less of biology than of mathematics, and later in the book he goes so far as to suggest that Mendel's mathematical training accounts for the otherwise enigmatic circumstance that it was he and not his contemporary Darwin who laid the genetic foundation that was to support Darwin's own ideas. As one is carried along by Sigmund's persuasive account here, nothing seems more natural than to apply mathematical thinking in biology—one can almost imagine the day when a semipopular book on mathematical biology will contain a few equations.

For Sigmund, mathematics is the essential tool of the thought experiment, the exploration of the explanatory power of some *what if?* proposition. Of course, this is the stuff of deducing phenomena from hypotheses of mechanism and process—of theoretical science itself. But as Sigmund points out, this activity is very close in spirit to play, to games of *let's pretend*. The book has chapters on the mathematical theory of games, but it explores many other games as well, such as a penny-matching game between parasite and immune system as an explanation for sex and dice and card games to describe molecular evolution. Throughout the book, Sigmund plays with the word *play*, exploring the surprisingly many ways in which it is used.

The overall scheme of the book has some inspired touches. Sigmund begins with a chapter on artificial life, to my mind an appropriate acknowledgment of the fundamental role in evolutionary biology of this relatively new discipline. The object of research in artificial life is "to build models that are so life-like that they would cease to be models of life and become examples of life themselves," in the words of artificial life pioneer Chris Langton. The roots of such work lie in John von Neumann's concept of self-replicating automata. Sigmund's explication of this concept, one of many expository gems in the book, is the most lucid and forthright I have seen in print. His sketch of