

A brief but informative discussion relates these heuristics to the artificial intelligence literature. GLAUBER is observed to differ from other conceptual clustering heuristics in its use of relational input data. DALTON is compared with DENDRAL—a heuristic for identifying topological structure of organic molecules on the basis of mass spectrographic data—and it is noted that DALTON, though theory-driven, is “weaker” than DENDRAL in that it embodies less domain-specific knowledge.

Despite the substantial ingenuity exhibited in these inductive heuristics and the impressive list of successful “rediscoveries,” it remains to be shown that these successes model genuinely significant examples of scientific discovery. The authors argue for this claim in a variety of ways throughout the book, but most explicitly in chapters 2 and 9 through 11. In summary, their arguments are these. First, discovery of the type they have modeled is found to be an essential *part* of clearly significant “discovery events,” which also involve other kinds of discovery—for example, Planck’s discovery of the law of blackbody radiation was a part of the discovery of quantum mechanics. Second, some other important types of discovery—problem identification, choice of relevant data and their representation, conceptual innovation—are amenable to formulation as “solution of well-structured problems.” The limits to which the authors wish to push this second argument are not completely clear, and it is questions about these limits that will provoke controversy.

Some questions arise even about the BACON heuristics. These are capable of law discovery with a given vocabulary and of “conceptual innovation” in discovering intrinsic, theoretical concepts. But both these capacities appear somewhat limited. In the former case the heuristics apparently lack the capacity to discover the appropriate scope for the laws they discover. Given data about two-particle collisions, and having discovered inertial mass and momentum conservation, could such heuristics then discover that kinetic energy conservation as well holds for a subclass of this data? In the latter case the heuristics appear limited in two ways. First, theoretical concepts can be identified only in experimental situations where their values are uniquely determined by the data. Inertial mass cannot be identified from data about collisions involving several particles, in which the velocity data do not suffice to determine it uniquely. Second, it is not apparent how these heuristics might be extended to work in situations where more than one theoretical concept must be identified and employed in the same law. How could they identify both mass and force and

discover Newton’s second law from kinematic data? This would apparently involve first “postulating” the second law and then discovering some special force law that, together with the second law, described the kinematic data.

There is a sense in which discoveries of interrelated theoretical concepts like those of Newtonian dynamics are discoveries of “novel representations.” More obvious examples are the discovery of geocentric epicycle-deferent models for celestial motion and Dalton’s atomic hypothesis. The authors minimize the extent to which the discovery of “wholly novel representations” is associated with scientific discovery (p. 333) while also suggesting that a “problem solution” approach to discovery might be extended to discover new representations (p. 326). The examples mentioned may be just the kind of examples of “revolutionary science” that the authors exclude from the scope of their methods. Yet how and where the limits on conceptual innovation by their methods are to be drawn remains an open question.

Questions like these about the “scope and limits” of the account of scientific discovery offered in this book should be the focus of detailed and rigorous analysis among philosophers and historians of science as well as cognitive psychologists and members of the artificial intelligence community. Whatever the verdict turns out to be, the authors are to be credited with renewing the discussion of a traditional question at a higher level of precision and sophistication.

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An Effort at R&D

RCA and the VideoDisc. The Business of Research. MARGARET B. W. GRAHAM. Cambridge University Press, New York, 1986. xiv, 258 pp., illus. \$19.95. Studies in Economic History and Policy: The United States in the Twentieth Century.

In this book Graham provides a fascinating account of an unsuccessful technological innovation, the video disc developed by RCA. She describes RCA as a firm that was once the acknowledged leader in television technology, one that took advantage of opportunities, was committed to research, and, unlike most firms, maintained its employment of scientists during the Great Depression. The firm had difficulty repeating its earlier successes, however, and recognized as early as the 1960s that it needed another

major product to supplement television sets. Graham describes how RCA decided upon the video recorder: such a device would utilize both its television and its recording technologies and as a leading-edge technology would offer the possibility of cost reductions and proprietary advantages that would yield longer term profits.

RCA decided to work on the video disc in preference to magnetic tape, which was favored by the Consumer Electronics Division early in the project, for what were then valid reasons, including a desire to keep the cost and price of its video recorder low to attract a mass market. Accordingly, RCA pursued the strategy of perfecting a relatively inexpensive machine with few features. The video disc also seemed attractive because the sale of discs would yield continuing profits as with razor blades for razor manufacturers. Attractive programming then would be especially important in selling the system, a factor that eventually caused difficulties because, whereas magnetic tape technology permits the user to make recordings from broadcasts, the video disc is limited to playing previously recorded programs.

Graham details carefully and well the scientific and commercial development of the video disc. She provides examples of the role that competitive factors and internal firm dynamics—especially relations between the laboratory and the operating division—played in the process. The bureaucracy of a large firm, where repeated demonstrations and reports and evaluations were necessary, seemed to slow down the innovative process. Graham also describes in detail how RCA’s writeoff of its computer operation caused problems in the development of the disc. By the time RCA finally introduced the video disc player in 1981 the video cassette recorder had caught on and was viewed as superior to video disc recording. Graham suggests that if RCA had been quicker the video disc might have been more successful. She also gives an interesting account of RCA’s consideration of adopting a “fast-second” role.

The development of the video disc confirms that the process of innovation can be long, expensive, and uncertain. This reviewer would have appreciated more analysis of why RCA neglected the possibility of Japanese competition and of why it did not anticipate the possibility of substantial cost reduction in magnetic tape technology or the rental of tapes. One also wonders how close to cost RCA priced its machines and whether it could have priced them lower and made more on the discs.

In any case, Graham has given us an excellent and provocative analysis of the process of innovation within a large firm. It

is a fascinating and worthwhile book, both as a case study of innovation and as a source of clues to the reasons for the later demise of RCA as an independent entity. Graham's work allows one to speculate on the impact that the recent acquisition of RCA by General Electric is likely to have on the process of innovation in general and in the electronics industry.

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Evaluating Nuclear Strategy

Managing Nuclear Operations. ASHTON B. CARTER, JOHN D. STEINBRUNER, and CHARLES A. ZRAKET, Eds. Brookings Institution, Washington, DC, 1987. xxiv, 752 pp., illus. Paper, \$18.95.

Nuclear Crisis Management. A Dangerous Illusion. RICHARD NED LEBOW. Cornell University Press, Ithaca, NY, 1987. 227 pp. \$24.95. Cornell Studies in Security Affairs.

For many years the main focus of research concerning nuclear operations was on the weapons systems themselves and the strategic doctrine that was intended to guide their use in the event of war. More recently, attention has centered on the technical and political problems connected with the use or threatened use of weapons systems and with the implementation of strategic doctrine. Both *Managing Nuclear Operations* and *Nuclear Crisis Management* address such problems.

At first glance *Managing Nuclear Operations* is an impressive book. Jointly sponsored by the Brookings Institution and the Harvard University Center for Science and International Affairs, the volume contains 21 essays (many of great length) on various aspects of managing peacetime, crisis, and wartime operations. The contributors include academic and research-institute specialists in addition to government officials and contractors. The chapters focus on the mechanics of nuclear operations as well as on policy questions. Some of the topics addressed are safety and security, communications technologies and vulnerabilities, NATO nuclear operations, assessment of command system vulnerability, the implications of strategic defense, antisatellites, and arms control for nuclear operations.

Among the essays that stand out are those by Bruce Blair and Stephen Meyer. Blair examines the alert system in both crisis and conventional war situations. Alert states are known as Defense Conditions (DEFCONs) and range from DEFCON 5 (normal peacetime) to DEFCON 1 (most serious). Blair

examines the disposition of forces, states of readiness, and procedures associated with DEFCONs 1, 2, and 3. Meyer's contribution on Soviet nuclear operations is based on an extensive survey of Soviet writings and treats the Soviet conception of command and control and how it would be implemented in peace, crisis, and war. Of particular interest are the connections Meyer makes between issues such as the predelegation of nuclear release authority to any Politburo member other than the General Secretary and the political succession process, and between the Moscow antiballistic missile (ABM) system and the succession process in the event of a nuclear war. In the first case Meyer suggests that the Soviets may have avoided such predelegation because to do so could be interpreted as designating the General Secretary's peacetime successor. In the second case he argues that the Moscow ABM system may be of such great importance to the Soviets because the succession process will depend on somebody in Moscow surviving a U.S. attack, since "one can be certain that no candidate for the General Secretary's commander-in-chief role is going to wait in Irkutsk on a daily basis just to provide continuity of nuclear control in the unlikely event of a surprise attack."

Perhaps the most striking feature of *Managing Nuclear Operations* is what has been omitted or inadequately covered. There is nothing about the nuclear testing process, a crucial aspect of nuclear operations. The role such testing plays or doesn't play in the maintenance of a reliable arsenal is a key question with implications for the proposed comprehensive test ban. Similarly, no attention is given to the role of intelligence in nuclear operations—either in selecting peacetime targets or in contributing to wartime damage assessment.

Several very important topics are discussed only sketchily. How would nuclear targeting be conducted with respect to the Soviet Union? Instead of an answer, the editors give us Theodore Postol's essay on the targeting of Washington, D.C. Although some general principles can be gleaned from this chapter it hardly represents a serious attempt to explain the complexities of trying to implement U.S. strategic doctrine, which calls for attacks on Soviet military, political, and economic targets with the full range of U.S. strategic forces. Likewise, the discussion of continuity of operations is inadequate. Surviving communications nets are of little value unless the government can continue to operate. No treatment is given to the plans of crucial government agencies for continued operations and the feasibility of those plans. Albert Babbitt's chapter on command centers

contains no details on continuity of operations with regard to wartime command centers.

The reason for the omissions is clear: many of the contributors possess security clearances, as is evident from their positions, and as a result feel constrained to avoid detailed discussions of many topics—even topics that have been covered in depth in the open literature. Thus, in his essay on warning and assessment sensors John Toomay ignores the wealth of published material concerning the nature of the Defense Support Program satellites, ground stations, and communications links and instead writes merely of the application of infrared technology to early warning satellites.

The result is a book that is far less than it could be. Rather than provide the reader with a solid set of essays on the mechanics of nuclear operations followed by discussion and recommendations concerning policy options, the editors have assembled a jumble of essays, leaving the reader to fill in the gaps and sort the wheat from the chaff.

Nuclear Crisis Management is a valuable companion volume to *Managing Nuclear Operations* or any works written under the assumption that nuclear crises can be managed. The author says that such an assumption is dangerous under present circumstances. In further contrast to the viewpoint of *Managing Nuclear Operations*, Lebow sees good crisis management as requiring "fundamental changes in the force structures, doctrines and the target sets that define contemporary nuclear strategy."

Lebow suggests that there were three fundamental reasons for the outbreak of World War I: preemption, loss of control, and miscalculated escalation, possibilities that still exist in the nuclear world of today. Preemption could be a problem because, according to Lebow, the United States and the Soviet Union share many of the flawed assumptions that were responsible for German preemption; both give evidence of believing that striking the first blow can make a significant difference to the outcome of the war. Factors that could lead to loss of control over the crisis include the information overload produced by a massive technical intelligence network and the enormous Defense Department communications system, institutional compartmentalization, the compression of decision time due to the speed at which intercontinental ballistic missiles and submarine-launched ballistic missiles can reach their targets, and informal decision and implementation procedures that undermine safeguards.

Lebow believes miscalculated escalation to be a risk because of a variety of factors—including the complexity of nuclear war.