that would suit him: that of assistant secretary of the Smithsonian, serving under secretary Joseph Henry. This was largely a desk job, requiring Baird to carry out Henry's requests, to write literally thousands of letters each year, and to contribute publications in the Smithsonian's name based on the growing collections. The job did not require Baird to undertake the strenuous or life-threatening expeditions he evidently did not like. Nor did it require him to produce any original theoretical scientific contributions in order to secure his reputation; he could continue the cataloging and describing that he preferred. As Rivinius and Youssef argue, Baird remained self-assured in his work, but he stayed on familiar ground and avoided risky scientific ventures or new inquiries. He would often do more and more of the familiar rather than try the truly innovative.

When Louis Agassiz sought to exclude Baird from the new National Academy of Sciences in part because his work was too straightforwardly descriptive and Baconian, Baird had enough influential friends to block the move and to become a member. Baird made other enemies as well, though probably fewer than most of us would have made in similar roles. Moreover, there were problems of his wife's health, which Rivinius and Youssef have had retrodiagnosed by Allen Greenlee. Greenlee suggests that she may have had endometriosis complicated by loneliness and depression, aggravated by Baird's obsessive work and neglect of her needs. She controlled her partner emotionally in what the authors see as a warm, companionable, but not passionate marriage. Their single daughter Lucy remained devoted to her parents—and remained single.

While Baird was at his invalid wife's beck and call at home, he evidently needed to retain control elsewhere. Thus when he succeeded Henry to the secretaryship of the Smithsonian, he ran everything himself and let his assistants do little decision-making, certainly less than he had done as assistant secretary. This naturally led to conflicts with his associates and made life at the museum more stressful.

Rivinius and Youssef offer relatively little detail about Baird's scientific work, but they do not pretend to have written a comprehensive scientific biography. Rather they have given us a good first look at Baird as a person and as an important organizer of American science, particularly through his work at the Smithsonian. In introducing us to Baird, they entice historians to probe further and to ask new questions about Baird, about the Smithsonian's role in American science, and about the nature of natural history itself. This work does not replace Allard's study of Baird at the Fish Commission, nor does it undercut the need for further work on Baird. Indeed, this delightfully well-written volume whets the appetite for more.

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Unpopularized Genius

John von Neumann. NORMAN MACRAE. Pantheon, New York, 1992. x, 406 pp., illus. $25.

Norman Macrae, recently retired as the principal editor of *The Economist*, says he wrote this book after discovering that very little biographical material about "Johnny" von Neumann (1903–1957) was available to the general reader, even though among those scientists and mathematicians who were familiar with von Neumann's work all agreed that his genius was as great as that of any 20th-century scientist, including Einstein and a host of others about whom much has been written at all levels. Von Neumann's contributions to mathematics and to digital computing do not lend themselves easily to a lay interpretation, nor do they evoke the kinds of metaphorical allusions to society that, say, Einstein's theory of relativity does, and this may explain why his name is not a household word. (On the other hand, given the ways the popular press has distorted and sensationalized the work of more famous 20th-century scientists, perhaps von Neumann's obscurity is not such a bad thing after all.) In any case, Macrae bravely wades in not only to the Hungarian mathematician's works on computers and game theory, which do lend themselves to a lay interpretation, but also to his other, more abstract mathematical works that until now few outside the specialized fields have ever seen explained, if even noted. Macrae does not entirely succeed, but his effort is commendable. His experience as a chronicler of the science of economics—itself no easy subject to explain—serves him well here.

John von Neumann. [Photo Researchers, Inc.; Los Alamos National Laboratory]

Among the few popular books that do discuss von Neumann's life, several emphasize the fact that in the 1950s he had moved from a role as a mathematician to that of a scientific adviser to the Pentagon on matters of nuclear deterrence and warfare. Books such as Steve J. Heims's *John von Neumann and Norbert Wiener* (MIT Press, 1980) lament this falling away from the purity of his earlier life and work. In the introduction to the present book, Macrae warns the reader that he sees nothing wrong with von Neumann's shift into politics and his alliance with the more hawkish faction of the U.S. military establishment. Anticipating criticism for this unconventional apology, Macrae warns the reader beforehand and takes pains whenever he brings up this subject to argue that perhaps von Neumann's advice, repugnant as it might seem to us, may indeed have successfully prevented an all-out exchange of nuclear weapons between the United States and the Soviet Union during the past four decades. He has a point.

But while worrying about building up a case for defending von Neumann's role as a Cold Warrior, Macrae falls down elsewhere. The worst passages in the book are where he looks at von Neumann's role in bringing the stored-program, digital computer first into the consciousness of electrical engineers, mathematicians, and scientists, then into the commercial world. Drawing on the events of the early 1950s, when von Neumann was at the Institute for Advanced Study and loosely connected with an early stored-program computer there, Macrae concludes that the one-time theoretical mathematician had an excellent sense of the commercial and, what is more, that his model for bringing a technology out of the laboratory is one that the Japanese have learned to copy better than the Americans and is responsible for Japan's current technological lead in a variety of modern science-based technologies. There is nothing wrong with raising the issue of Japanese competition here, and Macrae is well qualified to discuss it. But using von Neumann's experience at the Institute for Advanced Study seems all wrong. It is true that the IAS team freely disseminated the design details of their creation as it was taking place, but the IAS computer was hardly an elegant piece of engineering. (An example: when certain tubes in the computer burned out it was necessary for a technician to cut through several wires to get at them, unplug them, and replace them with fresh ones. The wires then had to be soldered back again.) Most of the labs and universities that made copies of the IAS computer had to modify the design extensively, and even then few were able to construct a computer that was very reliable. And what of the fact that the Institute for Advanced Study itself did not enjoy having the computer on its campus and tolerated it only because having it was seen as the only way that von Neumann would agree to reside there? This case hardly represents a harmonious and exemplary transfer of knowledge from the academic to the commercial world.

Still, this book has much to recommend it—the author has a sense for what is important about von Neumann's life and work, and he convinces us that we, too, should concern ourselves with it. And he does that with a clear and forceful style of writing that few popular science books have. No doubt as historians take a more dispassionate look at the Cold War and as we begin to understand better the impact of the electronic digital computer in our lives, "Johnny" will get the attention and credit he deserves.

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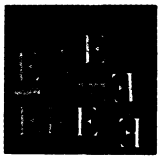


Redefinitions in Physics

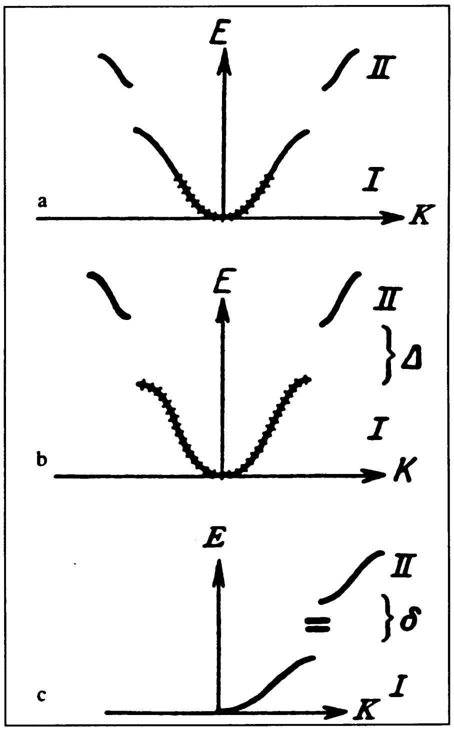
Out of the Crystal Maze. Chapters from the History of Solid-State Physics. LILLIAN HODDESON, ERNEST BRAUN, JÜRGEN TEICHMANN, and SPENCER WEART, Eds. Oxford University Press, New York, 1992. xxiv, 697 pp., illus. $75.

Now familiar to every physicist, the term "solid state physics"—probably coined after its German equivalent, *Festkörperphysik*, used occasionally since the 1930s—was not in use before the 1940s, and for some physicists the term even then sounded "kind of funny," as one of them put it in 1944. The many properties of solid matter now regarded as part of that specialty—crystal structures, magnetism, electrical conductivity of metals, mechanical properties of solids—were originally studied by chemists, engineers, and physicists having little if any contact with each other. As the authors of the first chapter of *Out of the Crystal Maze* note, the field was a mosaic "in which each tile seems scarcely related to the next." The conceptual unification of these phenomena and their integration into the discipline of physics would come only after the emergence of new experimental techniques and methods—the most important being low temperature and x-ray crystallography—and the development of quantum mechanics (as detailed in the book's second chapter). It would also be greatly stimulated by the applications developed during the Second World War and pursued afterward in many industrial laboratories hiring research physicists.

The emergence of solid state physics as a specialty can be understood only by taking into account both intellectual and social developments. On the one hand, publications by Heisenberg, Pauli, and Bloch applying the new quantum mechanics to electrons in metals showed that it was now possible to solve the problems raised by the classical theory of Drude and Lorentz and its semiclassical version developed by Sommerfeld, thus opening the possibility of making detailed calculations of the different properties of solids. On the other hand, the formation of the specialty was also strongly fashioned by the demographic growth of the discipline of physics starting in the 1930s and accelerating after the war. Whereas physicists like Pauli and Heisenberg were interested in the solid state only as a test case for quantum theory, the younger generation was much less mobile and settled for life to study a particular subset of physical phenomena. This specialization led to a redefinition of their social identity as they came to perceive themselves more and more as "solid-state physicists" rather than simply as physicists moving from subject to subject like their predecessors. This process was also greatly influenced by the fact that more and more of them were working in industry rather than in academe. In the United States, for example, the difference in outlook between industrial and academic physicists brought about, after years of discussion and in spite of fears of "balkanization" of the society, the creation within the American Physical Society of a solid state division as a means of discouraging industrial physicists from leaving the society to create their own organization.

The history of the scientific efforts that went to make up the field is detailed in *Out of the Crystal Maze* in separate, variously authored chapters devoted to band theory, point defects and color centers, mechanical properties of solids, magnetic materials, semiconductors, and superconductivity and critical phenomena. These chapters can be read independently of each other, and I suspect that scientist readers will go directly to the chapters most closely related to their own research interests, for the book is "written chiefly for people with some education in physics" and many of the chapters are quite specialized and cannot really be understood without a fair amount of technical knowledge. Overall, the account is written from the point of view of the theoretician. The evolution of the theoretical understanding of phenomena is at the center of most of the chapters, and experimental and technological advances receive much less attention.

Notwithstanding the common factors that went into the evolution of the field, I am not convinced by the treatment of the subject in *Out of the Crystal Maze* that by the 1950s these physicists were part of a "solid community," as is suggested by the title of the chapter by Spencer Weart that concludes the volume. The paucity of interconnections among the accounts in the book would suggest that the distance separating some of these subspecialties is large, comparable perhaps to that separating optics from nuclear physics. The authors of the chapter on magnetism even talk of a "magnetics community," and the development of big magnets led some scientists such as Aimé Cotton to become interested more in the instruments themselves than in using them to study solids. If for some time



"Alan Wilson's picture of electron energy *E* versus wave number *K*, with a band gap of width Δ. (a) A metal, with all the filled electron states well below the gap; (b) an insulator, with filled states reaching up to the gap, which blocks the electron motion; (c) (after Bloch) a semiconductor with impurity states in the gap. (From F. Bloch, 'Wellenmechanische Diskussion der Leitungs und Photoeffekte,' *Phys. Zs.* 32 [1931]: 883, 886)" [From *Out of the Crystal Maze*]