T. Patrick Culbert, Pamela C. Magers, and Maria L. Spencer. B. L. Turner II follows the same theme and shows how the evidence contradicts the swidden theory as an explanation of either the floruit or the demise of Maya civilization. The spatial, temporal, and specific expansion of Maya agriculturalists is examined by Norman Hammond, using data from excavations in Belize. Four papers concentrate on the variability of geology and vegetation in the central Maya lowlands. Don S. Rice is concerned with explaining ancient settlement patterns in the Peten as the result of pressure on various kinds of land over time. Frederick M. Wiseman looks to kinds of systems that could have been used: intensive swidden, "artificial rain forest," arboriculture, terracing, and raised fields. Through analysis by computer simulation, Wiseman concludes that no one of these could have been the sole technique practiced by the Classic Maya. Siemens analyzes the role that karst topography might have played and relates this to the overall scheme of water management and agriculture. Through paleolimnological studies in lowland lakes, Alan P. Covich demonstrates that biotic communities are cultural artifacts and not at all stable over time.

One of the most interesting contributions, containing much new material, is by Ray T. Matheny and focuses on the hydraulic engineering shown by the northern Maya. There is now evidence for ancient canals, raised fields, wells, reservoirs, and so forth from Campeche, and Matheny suggests that the purposes these waterworks (discovered in some cases from the air) served were to promote communication, encourage pisciculture, drain and irrigate low-lying fields, and provide drinking water throughout the dry season. David T. Vlcek, Sylvia Garza de González, and Edward B. Kurjack examine the problem posed by the dense settlement pattern of the agriculturally poor region of northernmost Yucatan and conclude that the salt trade stimulated this concentration of population. Puleston pulls together the data on terracing, raised fields, and tree cropping but cautions that not all linear patterns discernible from the air might actually *be* raised fields or chinampas and suggests ground confirmation. Nevertheless, the visual evidence for extensive raised fields in the *bajos* (swampy, low areas) of southern Quintana Roo presented by Harrison seems convincing.

In a comparative study of subsistence and settlement pattern in four tropical forest civilizations—Angkor (Cambodia), Anuradhapura (Sri Lanka), Prambanan (Java), and Tikal (Guatemala)—Bronson provides an Asian perspective; he argues that the Maya were economically in the least favorable position of the four, since draft animals were absent and land transport was difficult.

Summary papers are provided by David R. Harris, who downgrades swidden in the Maya case as but a marginal and pioneering type of cultivation; by Gordon R. Willey, who says that the swidden theory was becoming untenable anyway as archeological population estimates began to outstrip estimates of the population that could have been supported by the slash-and-burn technique alone; and by Turner and Harrison, who suggest future avenues of research opened up by these investigations.

In spite of its occasional repetitiveness, this volume is an admirable wedding of anthropological archeology with the sister sciences. It should be read by all concerned with the comparative study of ancient civilization. Its appearance calls for the rewriting of a lot of general texts on the Maya (including my own). Nonetheless, the iconoclastic enthusiasm displayed by the authors should not lead the nonspecialist to think that the Classic Maya never grew or ate maize, or that the lowlands were a sea of chinampas, or that ancient Maya clearings were never set to the torch. The real message of this book is the great variability and complexity of the Maya realm.

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# Max Born

**My Life.** Recollections of a Nobel Laureate. MAX BORN. Scribner, New York, 1979. xii, 308 pp., illus. $17.50. English edition of *Mein Leben: Erinnerungen des Nobelpreisträgers* (Munich, 1975).

Born once spent an evening drinking port with Rutherford and his friends. Each in turn declared what profession he should have preferred and why. Many named careers other than their own. "When the circle closed with Rutherford he hit the table with his fist and shouted: 'I shall be damned if I ever thought of being anything but what I am.' " This vigor and decisiveness, and the sure touch in physics that supported them, are the obverse of the traits Born identifies in himself in this posthumously published autobiography: timidity, sickliness, and mistrust of his own judgment. He allowed his junior colleagues to bully him. Oppenheimer sneered at his inability to compute; Jordan forced him to leave wave mechanics out of their book on quantum mechanics; Teller demanded his way "with the same stubbornness as he now [1948] insists on his political concepts."

Born traces his want of confidence to the early death of his mother, which deprived him of the natural source of encouragement and recognition. His father, an anatomist at the University of Breslau, though kindly and concerned, could not spare the time to protect his son's delicate psyche. Born grew up shy; he made few friends in school and found his social life in his large, wealthy, German-Jewish family. He wanted to be an engineer, as he enjoyed making toys on a lathe given him by his grandfather. But his father insisted that he attend the local university. There he discovered his taste for mathematics. He continued his studies at the University of Göttingen, then the stronghold of German mathematics, garrisoned by dozens of bright students commanded by Felix Klein, David Hilbert, Carl Runge, and Hermann Minkowski.

Born began well as assistant to Hilbert, but soon recovered his habitual malaise. Klein invited him to compete for a prize; he declined, irritated the master, and cut himself off, he thought, from a career in pure mathematics. He took his doctor's degree in applied mathematics under Runge. With no prospects at Göttingen, he returned to Breslau to work at his own expense in the physics laboratories of the university. Fortunately he was wealthy enough to pay for the severe damage he did. He turned to the only subject remaining, theoretical physics. He tried to work out certain problems in relativity, failed, and applied to Göttingen for guidance; Minkowski had got no farther than he, and offered him an assistantship. Two months after Born returned to Göttingen Minkowski died.

Once again Born stood unprotected before Klein. He was now rescued by Waldemar Voigt, the professor of theoretical physics, under whom he habilitated. His inaugural lecture brought something new to Göttingen: a sympathetic account of J. J. Thomson's speculations about atomic structure, which Born had come to admire during a brief stay in Cambridge. He reverted to the formalism of relativity until 1912, when he and Theodor von Kármán adapted Einstein's quantum theory of specific heats to crystals. They were partly anticipated by Peter Debye. Born regarded their work as the more profound and, characteristically, complained that it did not receive proper recognition. Also, and again characteristically, he chastised himself for having missed the discovery of x-ray diffraction, to which, he thought, his examination of crystals should have led him.

During the First World War, Born served as a technical expert for the artillery. Between business he and Alfred Landé computed the compressibility of crystals made up of arrays of flat Bohr atoms. Born says that their discovery that pancake atoms could not account for compressibility induced him to try to falsify Bohr's theory. After the war, as professor of theoretical physics at Göttingen, he settled down to this Popperian task, hunting for the simplest cases in which the theory broke down. By 1922 he knew that it failed for the helium atom and the hydrogen-molecule ion. By 1924 he was pointing the way to a new mechanics of the atom.

Max Born in the uniform of the Leibkürassiere, with relatives, Breslau, 1907. [From *My Life: Recollection of a Nobel Laureate*]

Born's approach was to translate the dynamical relations appearing in the Bohr theory into equations between quantum mechanical entities, such as frequencies and transition probabilities. Following this procedure Heisenberg invented a "kinematic reinterpretation" of the standard theory. Born recognized Heisenberg's procedures as matrix algebra and extracted the peculiar relation $PQ - QP = h/2\pi i$; then, in collaboration with Heisenberg and Jordan, he set up the formalism of matrix mechanics. In the winter of 1925–26 he lectured on the new theory in Cambridge, Massachusetts, and worked with Norbert Wiener to reformulate it in operator language. Meanwhile wave mechanics was invented. Back in Göttingen, Born used it to work out the scattering of charged particles; while doing so he developed the probabilistic interpretation of the wave function for which he won the Nobel prize.

These grand accomplishments did not bring full satisfaction. Heisenberg got most of the credit for matrix mechanics; no one remembered that Born had been the first to write an equation in noncommuting $P$'s and $Q$'s. The work with Wiener just failed to reach wave mechanics; one more substitution and Schrödinger's equation would have been Born's. As for the probabilistic interpretation, it paled before the principles of uncertainty and complementarity; instead of being applauded as a fine piece of natural philosophy, the paper on scattering was cited for a routine mathematical method called the "Born approximation." Even the Nobel prize had not its full luster to Born's eyes. It came too late, in 1954, 28 years after the work it honored.

Perhaps Born's greatest contribution was the establishment of a world center for theoretical physics at Göttingen. It drew its strength not only from him but also from the close collaboration between his institute and those run by James Franck (experimental physics) and Richard Courant (mathematics). The Nazis destroyed Born's establishment in their first "cleansing of the civil service." He emigrated to England and thence to Scotland ("We considered other alternatives, even emigration to America"), where he succeeded C. G. Darwin as professor of natural philosophy at the University of Edinburgh. The students did not resemble Pauli and Heisenberg. Still they made teaching trying. Born had trouble with those tricky problems in mechanics on which Anglo-Saxon physicists used to be bred. "It took me more time to solve them than was available to the students on the examination."

One wonders at the continuous self-depreciation of this Nobelist. His autobiography leaves the impression of a decent, hard-working, humorless man who could never quite become a friend to himself.

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## Nuclear Physics in France

**Scientists in Power.** SPENCER R. WEART. Harvard University Press, Cambridge, Mass., 1979. xiv, 344 pp. + plates. $17.50.

As historians of physics and technology broaden their studies beyond the mere evolution of individual theories or techniques, they increasingly grapple with how scientific, political, and social factors combine to direct scientists toward specific fields of research. In *Scientists in Power*, Spencer R. Weart provides an account of some of the forces that brought it to pass that nuclear bombs and fission reactors were the innovations that emerged from the broad subject of nuclear physics. The book is a case history of the French experience that has the vividness, pace, and depth of characterization of a good adventure novel. Weart then generalizes upon this history to rough out a model of how inventions can emerge from the interaction of scientist, nature, and society.

Weart's earliest chapters set the scene with a history of Pierre and Marie Curie and their circle of scientist-friends. These men and women, many trained in the intense and intimate surroundings of the Ecole Normale, formed the liberal wing of French science in the first decades of this century. They saw research as a powerful instrument for the creation of a just society; human problems would disappear in the face of the material resources and deeper understanding that science can create. Indeed, in World War I, French science did prove to make direct and obvious contributions to its country's safety.

After the war, a new note was faintly sounded. The more politically conscious of the scientists, among them Georges Urbain and Paul Langevin, noted the "amoral side of science" and the importance of placing its direction in the hands of progressive forces (p. 19). Most mem-