retical 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 members of the Curie circle, however, took from the war only a heightened appreciation of the efficacy of science and bent themselves to a successful attempt to increase research budgets. One outcome of the funding they secured was a job for Frederic Joliot, talented protégé of Marie Curie and Langevin.

The scant 60 pages Weart devotes to this background is somewhat strained by the richness of the events and the large number of personae. The story occasionally becomes thin or fragmented, and as a result these chapters are less successful than those that follow.

Scientists in Power hits its stride in chapter 4, at the point when Joliot, in January 1939, received the article by Otto Hahn and Fritz Strassmann suggesting that the uranium atom could be split. By then, Nobel Laureate Joliot was a professor at the Collège de France and, with an eye toward practical applications as well as pure research, had built up impressive laboratory facilities. Joliot enlisted two young colleagues to work on fission. One was Lew Kowarski, who, after a childhood disordered by the Russian revolution, was struggling to attain the security of a scientific career. The other was Hans von Halban, Jr., the urbane and well-to-do scion of an Austrian academic family. Weart's narrative proceeds as a group biography of the scientific and political fortunes of this team.

At first, Weart's story is chiefly of traditional, small-scale laboratory investigations. As the scientists gained the confidence to work toward a reactor, however, they needed additional resources. They approached industry and government, offering the uranium mining firms the possibility of new markets and France the hope of energy and armaments in return for money, personnel, and materials for their experiments. But "Joliot and his team, unlike most scientists in other countries, determined to take over the development of the practical applications of fission themselves" (p. 103). The stage was thus set for struggle over the control of nuclear technology and hence also over end use; the scientists aimed principally at providing a new source of power, the government emphasized bombs.

In the spring of 1940, Paris fell and the paths of the protagonists diverged. Joliot ceased fission research and remained in Paris to sustain French science in the harsh environment of the Occupation. He soon added to his role of seeming collaborator a second as a Resistance fighter. In 1942, he joined the most active of the French anti-Nazi groups, the Com-

Paul Langevin (left), Paul Rivet, and Pierre Cot giving the Communist salute atop a taxicab during a demonstration, 14 July 1935. [From *Scientists in Power*]

munist Party. Halban and Kowarski went to England. Ultimately, although by this time separated by a growing animosity, both went to Montreal, where Halban had recruited French scientists-in-exile to build a heavy-water reactor.

When the war ended, Joliot and some of the scientists who had returned from Canada joined to become the backbone for the new French Commissariat à l'Energie Atomique (CEA). Here the first French reactors were built, under the control of the Scientific Committee of the CEA and against formidable political obstacles. Joliot, High Commissioner and dominant force in the Scientific Committee, took a position of vigorous opposition to research on nuclear bombs, a position deriving from both humanitarian considerations and the exigencies of Communist political strategy. In 1950, however, first Joliot and gradually other leftists were forced out of the CEA. Simultaneously, power passed from the Commissariat's Scientific Committee to an administrative division whose sympathies lay with industry and government. The scientists, in effect, ceased to set their own goals. By mid-1951, research at the CEA had drifted onto the path leading to weapons.

Weart generalizes from this history in his Afterword. His discussion here lacks the lucidity of the preceding narrative; nevertheless its main points are easily seen and the model he gives can fruitfully be extended to other cases. Technical inventions are not simply dictated by the laws of nature or of existing hardware, in the author's view. Neither are they completely determined by societal structures. Rather, they are born from an interaction between nature and society that is mediated by a community—the professional scientists—with its own characteristic psychology and special interests. Scientists want knowledge, prestige, and pecuniary rewards. On the one hand they must study nature to achieve these goals, and on the other they must apply to society for the resources with which to prosecute their researches. Society, in supporting science, buys itself knowledge, national prestige, medical and industrial applications, and instruments of war. This process of negotiation operates to select out of all the innovations compatible with natural laws those that are realized.

How can we improve the selection mechanism and obtain more beneficent technologies? Weart directs our attention to the scientist-mediator. Scientists and engineers are often guilty of ignoring the social dimensions of their work. Weart suggests that, along with others, they "step outside the boundaries of [their] jobs in order to act publicly." Perhaps we now uncover an unexpressed reason for the author's choice of the French case; Joliot exemplifies just this kind of broad social and moral vision. "If we also work to escape the constrained thinking and activities of narrowly defined roles," Weart concludes, "we might look with more confidence to the future" (p. 276).

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Rutherford and His Times

Rutherford and Physics at the Turn of the Century. Papers from a symposium, Montreal, Oct. 1977. MARIO BUNGE and WILLIAM R. SHEA, Eds. Dawson, Folkestone, Kent, England, and Science History Publications (Neale Watson), New York, 1979. viii, 184 pp. $20.

Forty years after the death of Ernest Rutherford a symposium was held at McGill University to discuss his work as Macdonald Professor of Physics there within the context of fin de siècle physics. The present volume constitutes the proceedings of this symposium, though two or three papers in it were added subsequently (including Norman Feather's Rutherford Memorial Lecture, delivered a day or two earlier at McGill during independent celebrations at the dedication of the new Rutherford Physical Laboratory) and at least one paper and all the comments or discussions have been omitted.

The volume as a whole succeeds in