unfair and secretive means" are London's and Heitler's words on their American rivals, especially "this Pauling." Gavroglu emphasizes trans-Atlantic takeover, ignoring like most American-trained authors the many British contributions to quantum chemistry. The true clash, however, was not national, but between chemists and physicists. Physicists took the Heitler-London theory as proof that physics explains chemistry. Chemists saw it as one contribution, not the first, to a much larger task, interpreting valence and stereochemistry (directionality of chemical bonds) for all molecules, not just hydrogen: "the greatest single contribution," according to Pauling, "to the chemist's conception of valence since G. N. Lewis's suggestion in 1916 that the chemical bond between two atoms consists of a pair of electrons held jointly by the two atoms."

No compliment could have been more infuriating. To London the Lewis atom, a cubical arrangement of static electrons, not nuclear, not quantized, and violating Earnshaw's theorem, was amateurish absurdity. Yet the notions of octets and pairing (expropriated from a young Englishman, Alfred Parson) had enabled Lewis, followed by Irving Langmuir, to devise rules illuminating all chemistry. How far chemists had traveled by 1927 appears from a brilliant book, N. V. Sidgwick's *Electronic Theory of Valency*, published just before the Heitler-London paper, which reconciled the Lewis-Langmuir rules with the Bohr atom and coordinated vast quantities of physico-chemical data.

London had hoped to establish a unique set of a priori concepts governing quantum chemistry. He failed. There were too many options: his and Heitler's valence bond method; Friedrich Hund's molecular orbital method; group theory; Pauling's work. To extend valence bond theory Pauling devised three new Lewis-like rules, based on chemical knowledge, for connecting complex stereochemical structures to possible underlying wavefunctions. London was upset. To his Husserlian mind Pauling's way, half quantum theory, half crude Baconian induction, was philosophical barbarism. Also his own antireductionist instincts had resurfaced.

Departures from pure reductionism range from radical ontological hypotheses, like Bergson's life-force guiding evolution, to intermediate working rules like Pauling's. Between these, deeper than either, are issues of epistemology. Take the two main analytical schemes just referred to. Computationally they agree. As explanations they diverge. Valence bond theory seems to base chemical action on continued rapid exchanges of electrons between atoms. Molecular orbital theory invokes oscillations among ionic states. Each interpretation makes physical sense. Neither is testable. "Chemical concepts," wrote C. A. Coulson in 1962,

> operate at a particular level of depth, and if we dig deeper . . . we lose them . . . [If] we operate at the appropriate level of sophistication . . . wave mechanics . . . [will] give us considerable insight into chemical behavior. But we must not press our enquiries too deep, or we shall lose all general rules and principles, and every molecule will become an isolated problem, unrelated to any other molecule.

London struggled on, less and less happy with his own book. Then came Heinz and superconductivity.

An experimenter gifted in theory, and one of the last Jews to gain a doctorate in Germany, Heinz joined Fritz and Edith at Oxford in 1934. Already he had framed several concepts often credited to Fritz—among them the "London penetration depth" characterizing the thin (~500 Å) surface layer in which electric currents flow in superconductors. His latest concern was the "Meissner effect." Surprisingly, superconductors cooled in a magnetic field expel that field from their volume. Superconductivity is something other than absence of electrical resistance. New equations were needed, and these Fritz and Heinz supplied.

The Londons' theory was "phenomenological" in both the common and the Husserlian sense. It was descriptive, not explanatory; and it involved an a priori leap (setting a constant of integration to zero) justified "an der Sache selbst." Gradually London drove deeper. From classical electrodynamics he deduced that while currents are restricted to the penetration layer, another electrical quality, the canonical momentum, extends throughout the material. Superconductivity emerges as a new kind of order. Quantum mechanics has two conjugate variables, position and momentum. Atoms in crystals are ordered with respect to position, electrons in superconductors with respect to momentum. All obey the same wavefunction, and, as London guessed in 1950, the magnetic flux through a superconducting ring is macroscopically quantized.

London's reach here contrasts impressively with the powerful but more conventional intellect of Felix Bloch, who, taking superconduction as absence of resistance, "proved" in 1929 that no quantum mechanical theory of superconductivity was possible. Equally fine was his identification (1938) of superfluidity with Bose-Einstein condensation. Gavroglu rightly connects these insights with London's antireductionist philosophizing, but biographical truth runs deeper. London the philosopher was ponderous and dull, prisoner of a dreadful German academic style. London the physicist was inspired.

Even so, neither in superfluidity nor in superconductivity could he construct a detailed theory. New ideas of another kind were needed, experimental ideas. In these the seminal mind was not Fritz but Heinz, whose pioneering of thin-film and high-frequency superconductivity opened ways to a number of discoveries basic to the later Bardeen-Cooper-Schrieffer theory.

A man whose "warm, welcoming smile" charmed the young Brian Pippard and a man mired in scientific quarrels: that was Fritz London. Quarrels, of course, have two sides. Of Landau, London's Russian enemy, Kapitza spoke truth when he told Stalin in another context that Landau was no counterrevolutionary, only "a horrible man." Still, London cannot escape blame. His was a strangely asymmetric worldview. His failures to cite other men's work were a consequence of writing logically rather than historically; their failures to cite him were sinister malevolence. Saddest was London's dispute about the Meissner effect with von Laue. Gavroglu takes London's side, probably correctly. But Laue was right about something more important than the Meissner effect. Alone among senior German physicists he, overcoming marked limitations of background and temperament, stood unflinchingly against Nazi villainy. London did ill to withhold after the war the generous word that would have comforted a broken old man. Einstein was more clear-sighted. "Gruss an Laue [greet Laue]," he said to P. P. Ewald when the latter visited Germany in 1938. And, when Ewald asked for other messages, simply repeated with terrible silent eloquence, "Gruss an Laue."

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Vignettes: Career Economics

Is a Harvard education more valuable than an education from Clinch Valley Community College because of its professors' skills and efforts or because of the students' skills and efforts? There is little market at universities for so-called good teachers. That is, whatever investments a professor makes in being a good teacher is specific to the university and cannot be exported or capitalized in the market. Moreover, the professor is not able to capture any of the residual that accrues to the students in the form of increased earnings. There is an analogy with the theory of the firm in that the owners are the ones who monitor the inputs because they own the specific capital that is at risk if there is shirking or a diminished brand-name. A research professor, on the other hand, invests in capital that is more general and can be exported in the market. That is, if a university experiences a decline in its reputation and financial base, research professors can still maintain a return on their capital by moving to another university.

—*Roger E. Meiners* and *Robert J. Staaf*, in *The Academy in Crisis: The Political Economy of Higher Education* (John W. Sommer, Ed.; Transaction Publishers)

The aging scientist might decide to reallocate research and writing time to administration. . . . This tendency will be accelerated if his accumulated experience enables him to obtain a high salary in an administrative post. By raising his potential earnings in an alternative activity (administration), experience increases the opportunity cost of his remaining a researcher. Another thing that increases that cost is—success. Successful scientists and other successful creative people are invited, sometimes even badgered, to give prestigious lectures, accept honorary degrees, serve on boards and committees, consult, advise, write popular papers, give memorial addresses, appear on television, write letters of recommendation, and so forth, and to the extent that they yield to these importunings, as most of them do (because these activities produce psychic and sometimes pecuniary income), they have less time for research. A partially offsetting factor is that the successful, prominent person is in a better position to obtain criticisms from able people; indeed, his success, his prominence, may make him a target for criticism—which he can learn from, if he does not dismiss it as a product of envy. But that is what he is apt to do. I am led to predict that creative people who remain obscure throughout their lifetime will reach their creative peak later than those who are successful in their lifetime.

—*Richard A. Posner*, in *Aging and Old Age* (University of Chicago Press)

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