Book Reviews

A Physicist's Journey

Fritz London. A Scientific Biography. KOSTAS GAVROGLU. Cambridge University Press, New York, 1995. xxiv, 299 pp., illus. $69.95 or £50.

A scientific biography that begins not with its subject's youthful interest in mathematics or machinery or beetles but with his hero-worship of Goethe and his doctoral dissertation on the philosophy of Edmund Husserl evidently describes an unusual man. When that man is a physicist who contributed fundamentally in two fields, superconductivity and theoretical chemistry, we expect unusual ideas. When also he is a baptized but not Christianized Jew forced to flee Germany in 1933, we look for unusual undercurrents in human feeling. Such was Fritz London. His troubled journey from East Prussia to North Carolina, though cut short by his death at 54, turned out unexpectedly well. It did so (one may suspect) not least because of his marriage to a determined and very gifted woman, the artist Edith Caspary, whose striking portrait of London adorns the jacket of Gavroglu's book.

Goethe's hold on the German imagination resists Anglo-American understanding. That vast intellect, that chilling ego: art, science, poetry, friends, colleagues, mistresses all used to one end, its own self-refinement. At 17, London can be pardoned for missing this. His admiration centered on Goethe's antireductionist vision of science.

Reductionism and antireductionism are continuing patterns in human thought. Why some people find reductionist explanations so attractive while others find them so repellent is a question that deserves reductionist investigation. Should we look to psychoanalysis and childhood emotion? Are there reductionist and antireductionist genes? Or is the cause historical? Gavroglu expresses surprise that London had antireductionist instincts during the era of logical positivism, most reductionist of philosophies. Yet Bergson, Whitehead, and Lloyd Morgan, all antireductionists, overlapped London, and that ultimate antireductionist word "holism" was invented in 1926 by the South African philosopher-warrior-statesman Jan Christiaan Smuts.

Above all, Husserl and his allies in the phenomenology movement were sophisticated antireductionists. But we must consider what reductionism is.

To most scientists reductionism means reducing all scientific explanation to physical terms. Husserl's focus was different. Interested initially in mathematics, he came awake when his first book, *Philosophie der Arithmetik* (1892), was savaged by Gottlob Frege for "psychologism." In Hume's old dichotomy between empirical and analytic knowledge Frege accused Husserl of a category mistake. He should have been reducing arithmetic to logic. Instead he had entangled it in empirical processes like counting. Shaken, Husserl began his own much wider philosophical reconstruction, phenomenology.

London approached Husserl through Kant, and unlike those of most physicist-philosophers his interests were, as Gavroglu acutely remarks, "predominantly epistemological." Husserl's epistemology, though reached differently, may be viewed as modernizing Kant's answer to Hume. Kant had added synthetic a priori ideas to Hume's analytic ones. Science requires synthesizing principles: these already exist within our minds as a priori intuitions. Husserl's goal was educated intuition. His slogan "An der Sache selbst" ("to business," or "to the things themselves") directed philosophy toward intuiting in every sphere of thought the right a priori interpretative assumptions. The result was antireductionist in two ways, in conceiving different phenomena as requiring different thought structures and in opposing the reduction of mathematics to logic.

Likewise London in his doctoral dissertation, written in 1921 under Alexander Pfänder at Munich, saw physical theories as mathematical frameworks enmeshing some given "volume" of fact. The better the theory the greater the volume and the closer the meshing. Thus Newton's theory is by comparison with Einstein's a deformed framework. Mathematically the frames should be closed axiomatic systems treated as wholes. In other words, London opposed what Bertrand Russell called "logical atomism"—the goal of reducing all theory to primitive logical elements.

How Russell's (and Frege's) logical reductionism relates to our physical reductionism and what relations exist between the various antireductionisms hostile to each of these forms of reductionism remain profound questions.

All his life, Gavroglu notes in this biography, London had a habit of retreating into himself and "writing a long piece to clarify the conceptual issues of a particular problem." That being so, it is biographically fundamental that his two determinative scientific papers were collaborations, the first (1927) on quantum chemistry with Walther Heitler, the second (1935) on superconductivity with his younger brother Heinz. This introverted man needed other people. Even his third masterwork, the Bose-Einstein theory of superfluidity (1938), grew out of intense discussions with Laslo Tisza at one of the very few scientific conferences London attended.

How exquisite the irony that London, the antireductionist, shared in initiating such a drastic intellectual reduction as that of chemistry to physics. From quantum mechanics he and Heitler built a theory of the hydrogen molecule that accorded well with chemical evidence. Even Schrödinger was stunned. Never remotely (he told London) had he expected his equation to explain all chemistry as well as physics.


Fritz London. [AIP Niels Bohr Library; photograph by Francis Simon]

If Heitler and London did this in 1927, why are Linus Pauling and other Americans, all of whom came later, widely credited with interpreting the chemical bond? And why was London's book *Quantenmechanik und Chemie*, begun in 1929, never completed?

"Lies," "bad faith," "stealing," "the most 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 physicochemical 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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