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WILLIAM L. McMILLAN  
1936–1984

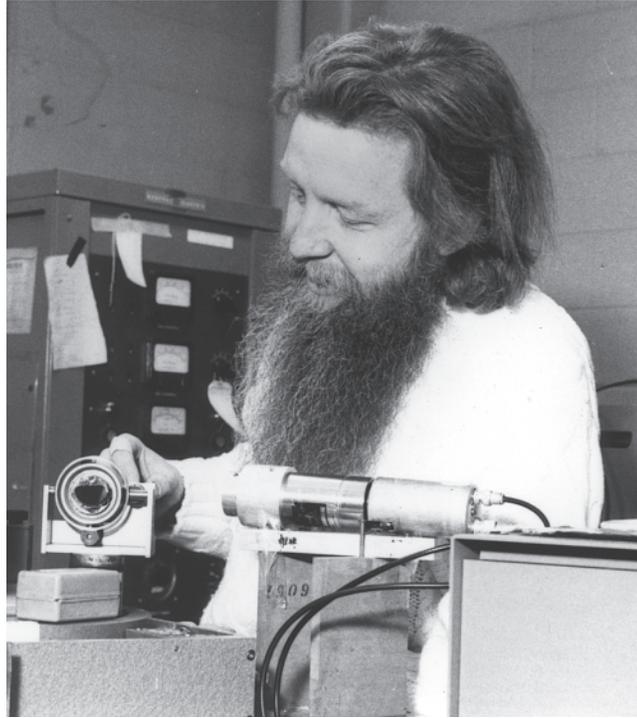
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*A Biographical Memoir by*  
P. W. ANDERSON

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William Brewster

## WILLIAM L. McMILLAN

*January 13, 1936–August 30, 1984*

BY P. W. ANDERSON

THE TRAGIC ACCIDENT THAT killed Bill McMillan in August 1984 at the age of 48 deprived the world of the ablest condensed matter physicist of his generation. The best student of John Bardeen at least since J. R. Schrieffer, the most successful of several outstanding products of the postdoctoral program at AT&T Bell Labs during its greatest period, he went on to a full professorship at Illinois in 1972 at the age of 36, where he continued to be outstanding both as a research scientist and as a teacher until he was struck by a confused teenage driver while cycling along a deserted country road. He has been appropriately commemorated by a prestigious prize lectureship for young condensed matter physicists.

A group including Bill's father's progenitors left the Isle of Skye in Scotland in 1820 and founded the small community of Union Springs, Alabama. Over a century later Bill's father, Laughlin, was the first McMillan to leave Union Springs permanently, when he went off to college at Auburn, where he played football and earned a degree in civil engineering. He then moved to Little Rock, Arkansas, where he worked for a foundry company. He married Edna Shergold Mashburn, the daughter of a judge and a member of an old southern family—her grandfather had owned the Georgia

Southern Railroad. She had some social pretensions and may have felt she had married “beneath herself”; later on, Bill told his wife, Joyce, that he had no interest in becoming rich, which she attributed to his reaction to family tensions. He also disliked team sports, in a mild rebellion from his father’s preoccupations.

Bill was born on January 13, 1936, and was named after his grandfather, William Laughlin McMillan. His namesake was something of a patriarch and demanded that he be brought to the family farm as a very young infant for inspection. Bill was the elder of two brothers, and there were also younger cousins in Little Rock. Bill was the leader of the group and was known among them as “gentle Will” for, among other things, his habit of taking the blame for any mischief. He was an outstanding student in high school and also led a jazz group that won a national contest. Later, in college, he led several bands, playing almost any instrument, and earning spare cash thereby, as well as disc-jockeying at a local radio station, despite his stutter. He took a degree in electrical engineering, spending summers at RCA (where he first conceived the ambition to build his own computer using Bell Labs transistors) and at IBM in Poughkeepsie.

In January 1958, in the middle of his senior year, he married Joyce Ann Stairs whom he had met the previous year and had proposed to by phone during the summer. Joyce is also from a small-town background: She came from Heber Springs, Arkansas. The two of them always shared equally in the demands of the household and of the four children who resulted from the marriage: (Kenneth Laughlin, 1962, at Illinois; Lawrence Edward, 1965, and Julie Anber, 1966, in Summit, N.J.; and William Albert, 1967, in Cambridge, England). Joyce was urged by Bill to finish her Ph.D. degree in spite of Kenny’s arrival, and she did so in 1963. Bill had stayed on at Arkansas for a master’s degree in

order to let Joyce graduate. Fortunately for the world of physics, the department of physics at Arkansas was quicker and more flexible in processing his application for graduate school, so he switched to physics. At some time during this year he conceived the ambition of working with John Bardeen, so he submitted a single application for a Ph.D. program, to Illinois—if it failed, apparently he would have gone to work as an engineer. After passing the qualifying exam at the top of his class, he did indeed start work with John in 1960. Later, he would apply for only one postdoctoral job, at Bell. It seems relevant to note that his stutter and quiet manner did not connote a lack of self-confidence.

At Illinois, Bardeen proposed several topics to Bill, but in fact it was Bill himself who selected the topic and chose the method of attack he used in his thesis. Neither was very characteristic of Bardeen's style—Bill was simply being Bill. This thesis is a classic, and is to my mind the most conclusive demonstration in existence of the nature of superfluidity in liquid helium. It starts out from a brief remark of Onsager's, that a Jastrow-type product wave function for helium is exactly equivalent in its density-density correlations to a classical Boltzmann gas with an artificial two-particle potential energy. The Jastrow function of interatomic distance is at one's disposal, but its asymptotic limits can be accurately estimated: the long range part, from the two-particle correlation implied by the known phonon spectrum, and the short-range limit, by solving the zero-energy Schrödinger equation for the fairly well-known interatomic potential. Then the intermediate portion is adjusted variationally to minimize the total energy. The density correlation can be evaluated by molecular dynamics using the Monte Carlo algorithm, and the energy and many other properties (including superfluidity) of the system evaluated. (R. B. Laughlin, much later, brilliantly exploited the same kinds

of identities for the fractional quantum Hall effect.) The method gave good agreement for the lambda point, the liquid-solid phase transition, and a value for the condensate fraction in agreement with present estimates from neutron scattering and other sources. Work over a decade later using the same methods, by Lupe Velez, improved the accuracy only fractionally. The outstanding excellence of this thesis is that very sophisticated theoretical concepts were blended with precise numerical work to the very great benefit of both. Other attempts at microscopic theories of *He* have to this date not led to any much more satisfactory description.

This thesis marked Bill as the first, and possibly the best, of a new breed of theorist. He was as comfortable as the previous generation of formalistic theorists with the mathematical methods and concepts of many-body theory, but he had the additional dimension afforded by the sophisticated use of the electronic computer, not by the brute force method of direct simulations at the atomic level but in tandem with the best formalisms available at the time. In these days computer skills are taken for granted, and software is available for almost any task; but I sense that Bill's achievements even now may still be close to the edge of the possible.

John Bardeen, who was already a Nobelist, continued the single-track progression of Bill's career by virtually directing us at Bell to employ him as a postdoc, which was all right with us. He arrived in 1964, just after the pair of *Physical Review Letters* by Rowell, Thomas, and Anderson, and by Schrieffer, Wilkins, and Scalapino had established superconducting tunneling spectroscopy as a semi-quantitative method of correlating electron and phonon data on real metals with superconductivity. The "tunnel conductance"  $dI/dV$  between two normal metals was shown by Schrieffer to be featureless, so that the striking structure in  $dI/dV$

displayed when the metals become superconducting is a precise measure of the renormalization of the quasi particles by the electron interactions. Given plausible simplifying hypotheses about the phonon spectrum and coupling constants, a set of complex integral equations due to Eliashberg could predict the tunneling characteristic with some accuracy, as the above authors showed. But it seemed to us inconceivable to do the problem the other way round: given the experimental tunneling characteristic, to try to deduce the phonon coupling constant and the entire phonon spectrum. Nonetheless, given the merest hint, this was what Bill proceeded to do: calculate the phonon spectrum, and the order-parameter as a function of energy, given accurate tunneling data. This tour de force of computational skill led to a series of magnificent papers with John Rowell who simultaneously developed tunneling as an exact spectroscopic tool.

The period of the middle 1950s had seen a remarkable development of the theory of metals in which Migdal's theorem, that the effects of lattice phonons could be handled in lowest-order perturbation theory in the mass ratio  $m_e/m_{\text{ions}}$ , played an important role. It was Eliashberg and Schrieffer's insight that the same theorem was valid for superconductors, and Morel-Anderson's approximation that the interactions were effectively local, that led to the integral equations, complicated but involving only the frequency variable, which McMillan was able to invert. This was, as I remarked in a historical lecture 25 years later, the defining moment when one could say that the "fat lady had sung" to mark the final success of the theory of metals and of superconductivity based on BCS and phonon interactions. It is fair to say that, as of then, it was the most precise calculation ever made of any thermodynamic phase transition from first principles. It was also a remarkable vindication of the

basic ideas of many-body perturbation theory in fermion systems. For this work he and John Rowell jointly received the only major award of his career, the London Prize of the international low-temperature physics community (jointly with Gunther Ahlers of Bell Labs).

In the meantime, far from requiring mentoring, Bill was himself acting as mentor for a young English postdoc, David Taylor, and between them they were discovering one version of the “coherent potential” or “effective medium” approximation, which has been remarkably successful in explaining the energy band spectra of disordered alloys. It is noteworthy and typical that he let David author this work alone.

By this time Bill had become a full member of staff at Bell Labs and was working with a wide variety of people: Bob Dynes, as well as Rowell and his postdoc Lawrence Shen, on superconductivity; with me, teaching me to program (but not to make serious errors) in a project on the spectra of disordered inner-shell resonances; and with many others. His most well-known result was the “McMillan equation,” a semi-empirical distillation of the physical implications of his work with Rowell that estimated  $T_c$  on the basis of fundamental physical parameters of the metal. The BCS theory itself appears to give such a formula, the BCS equation for  $T_c$

$$kT_c = \hbar\omega_D e^{\frac{-1}{N(0)V}}$$

but the parameters  $\omega_D$  and  $V$  are those of a schematic model, only vaguely related to the real physics of the electrons in the metal. This BCS formula often led to great confusion if taken literally. What McMillan did was to combine insights into the real physics of electron screening, phonon restoring forces, and the “dynamic screening” of the repulsive

electron-electron interaction, with semi-empirical knowledge gained from his actual work with Rowell's experiments, to produce a meaningful estimate of  $T_c$  valid for all conventional metals, and that showed which material properties of the metal were important. This in turn was the basis on which Cohen and I estimated possible upper limits for  $T_c$  from the BCS phonon mechanism, violated by the new cuprate superconductors. I cannot help deeply regretting Bill's absence from the post-1986 discussions on high  $T_c$  superconductivity as a voice invariably to be found on the side of rationality. I expect that he would have mocked out of existence the school of theorists who drag his equation and his programs far out of context and attempt to apply them to the high- $T_c$  cuprates. Bill was at all times aware of the limitations of the assumptions he relied upon and every paper contained an accurate and honest estimate of possible errors.

His work in superconductivity was of real importance entirely aside from the tunneling method and the McMillan equation. We began a collaboration that continued in Cambridge on the "Tomasch effect" and electron-hole interference in proximity junctions, and he collaborated with Dick Werthamer on strong-coupling theory. The "Tomasch effect" work, which exploited the ideas of interference between hole-like and electron-like components of the quasi particles, and of what is now called "Andreev" reflection, led him to produce a working mathematical model for metal-metal junctions that was well ahead of its time and has been recently revived by a French group.

He was one of the early visitors to our Theory of Condensed Matter group in Cambridge, spending a sabbatical year and bringing Joyce and the children. Kenny quickly began to speak in Cambridge cockney, and it was occasionally necessary to rescue Joyce or Bill when the "old banger"

they had bought broke down. It was a household of small children, and Joyce was pregnant with Billy. To the casual observer it would have seemed that Bill's devotion to sharing the domestic demands would have left him little time at the Cavendish, but his productivity did not seem to abate; and at the end of this Cambridge visit, he spent a couple of months at the Orsay Laboratories near Paris, learning about liquid crystals from P.-G. de Gennes. This was the beginning of a friendship and collaboration that was strong enough that de Gennes alluded to it in his Nobel address in 1991. Bill was sufficiently intrigued by liquid crystals to try his own hand at experimental work at Bell with castoff apparatus and a legendarily messy laboratory. But the work he produced was fruitful; he submitted three papers on this subject before leaving Bell in 1972 for the University of Illinois. One of these was his first basic contribution to the field, a Ginsburg-Landau theory for the smectic A phase, and the other two were focused on verification of the fluctuations to be expected from this theory. He continued to publish significant results on liquid crystals for a number of years, culminating in two significant review articles on phase transitions and on molecular theories of the various phases. He returned to Orsay, spending another sabbatical year in de Gennes's group in 1978-79.

The move to the University of Illinois was, I regret to say, an excellent one for Bill and for Joyce. She had been working at Princeton with no formal position, learning protein crystallography, but was able to take a permanent job at Illinois; Bill himself blossomed as one of the most popular and dynamic teachers on the staff: In fact, he won a teaching award. He was able to attract able students and collaborators at will.

Again and again, he would see the relevance of what he was doing to some new field and enter this field with a break-

through paper. From complex order parameters in liquid crystals to charge-density waves in dichalcogenides was to him an obvious step, but here he brought in a new and valuable idea, the discommensuration. The discommensuration concept was an immediate success and came into its own with the later discovery of “sliding” charge-density waves, when the sliding motion could be visualized as motion of the discommensurations (which in this case are charged). The concept of the discommensuration foreshadowed the important work of Su, Schrieffer, and Heeger on anomalous quantum defects in one-dimensional charge-density waves. From this to the puzzling Al<sub>5</sub> martensitic transitions is also a small step. The effort here was, as in the liquid crystals, to develop a heuristic (Ginsburg-Landau) description of these phases. This was carried out with Ravin Bhatt, his student, now at Princeton.

It is not so logical how he came to his next subject. This was the result of being called in to help understand results by Jack Mochel on disordered Ge-Au alloys, where he used both his experimental expertise as a tunneler and his theoretical insight to construct and verify a general theory of the metal-insulator transition, including interactions as well as localization. Here, in one paper, he leapfrogged over work of Abrahams et al. and Altshuler and Aronov and took the next step. This subject remains very difficult and controversial and may not to this day have reached a full solution; but Bill’s brilliant paper applying Thouless’s scaling ideas to the problem of interactions was an important and promising start.

In his final two years he was preoccupied with the random Ising model (the spin glass), for which he built a special purpose computer in his basement using special high-speed chips and all of his programming wiles. I am not sure results exist in the literature that are more reliable yet than the

several papers he published in those years. In particular, he demonstrated conclusively that the two-dimensional spin glass did not have a phase transition.

I have had to pass over quite a number of isolated works—on such things, for instance, as  $He_3$ - $He_4$  mixtures,  $He_4$  thin films, and so on. At the time of his death 12 papers were in preparation or submitted, of which apparently at least 5 were posthumously printed. Bill may have been one of the last examples of what one might call the complete physicist. He was equally at home with abstract field theory and complicated mathematics, he was one of the first to integrate computational techniques and simulation into his papers, and he was at home doing his own experimental work and electronics. This gave him a great advantage in collaborations with experimentalists, since he understood their problems as well as his own.

Bill was a striking, even charismatic character, towards whom almost everyone felt an instant attraction. His very pronounced stutter was an affliction that never left him, but as is usual and particularly difficult for Bill, it was strongest before large audiences and when he was directly questioned. I always felt that his stutter was part of the reason why his written papers—and his work—were always concise but very complete and crystal clear. They were never overstated and he never explained an idea twice, but every idea was clearly stated: Nothing was missing.

He immensely enjoyed sight gags, of which the most continuous was his own appearance. He had a rather luxuriant red beard, which changed length and style from month to month—this month he would appear with a van Dyke and flowing mustache, another a bushy full Stracheyan beard, the next clean-shaven with long red hair. He started right out at Bell Labs in the iconoclastic vein. When he allowed his beard to grow, a guard noticed that the clean-shaven

graduate student on his pass was not the bushy-bearded individual who was wearing it. Bill's response was to allow himself to be re-photographed, and as soon as the new pass was issued he shaved off the beard.

An early exponent of the graphic T-shirt, he could be counted on to grace any occasion with some inappropriate piece of clothing: stocking cap, baggy corduroys, or whatever. For our formal celebration of the award of the London Prize he gave his talk in a T-shirt reading, "Where in Hell is Urbana Illinois," the rest of us being in ties and jackets. He was capable of making the best of an opportunity: I well remember the glee with which he reversed the order of my transparencies when I dropped them during a talk at John Bardeen's retirement conference.

He was also an early member of the new generation in his attitude to family life: He was always ready to take on his full share of the duties of parenthood and made it clear that he was an equal partner in raising the family. Everyone who knew them well could easily sense the strong affection between Bill and Joyce.

Bill's sense of humor was acute and very much in evidence. On occasion he could come up with very witty remarks, which were no respecters either of occasions or of persons. Nonetheless he was good-natured and self-deprecating enough that it was hard to take offense. Perhaps his best sight gag was when he was awarded an honorary D.Sc. at the University of Arkansas. The person who was to embellish him with a hood was bewildered by the beard, until Bill simply pulled it up over his head. He reserved his strongest barbs for pretense or pomposity; I think he always hoped that others would live up to his own high scientific and ethical standards and was capable of inserting a needle when he felt they were violated.

All those who knew him consider themselves extremely fortunate to have known Bill during the few productive years he was to be given. I feel that his loss was not only a personal tragedy but a tragedy for the progress and the intellectual health of our field of condensed matter physics.

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