Joaquin M. Luttinger

1923–1997

BIOGRAPHICAL *Memoirs*

A Biographical Memoir by
Walter Kohn

NATIONAL ACADEMY OF SCIENCES

JOAQUIN MAZDAK **LUTTINGER**

December 2, 1923–April 6, 1997

Elected to the NAS, 1976

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By Walter Kohn

The brilliant mathematical and theoretical physicist Joaquin M. Luttinger died at the age of 73 years in the city of his birth, New York, which he deeply loved throughout his life. He had been in good spirits a few days earlier when he said to Walter Kohn (WK), his longtime collaborator and friend, that he was dying a happy man thanks to the loving care during his last illness by his former wife, Abigail Thomas, and by his stepdaughter, Jennifer Waddell.

Luttinger's work was marked by his exceptional ability to illuminate physical properties and phenomena through the use of appropriate and beautiful mathematics. His writings and lectures were widely appreciated for their clarity and fine literary quality. With Luttinger's death, an influential voice that helped shape the scientific discourse of his time, especially in condensed-matter physics, was stilled, but many of his ideas live on.

For example, his famous 1963 paper on condensed one-dimensional fermion systems, now known as Tomonaga-Luttinger liquids,[1, 2] or simply Luttinger liquids, continues to have a strong influence on research on 1-D electronic dynamics.

In the 1950s and '60s, Luttinger also was one of the great figures who helped construct the present canon of classic many-body theory while at the same time laying foundations for present-day revisions. In recognition of his work, Luttinger was elected to the National Academy of Sciences in 1976.

Luttinger (usually called Quin) and WK first met in the Jefferson Laboratory of Harvard University in the spring of 1947. They were among the enthusiastic crowd from Harvard, MIT, and elsewhere in Cambridge and Boston to attend a famous course of lectures on theoretical nuclear physics presented by a very young Julian Schwinger.[3]

Before each lecture, while the audience patiently awaited Schwinger's eventual arrival, Quin, often loosely dressed and wearing sandals, slipped back and forth between his friends and regaled them with ever-new stories. And while others directed their undivided attention to understanding and transcribing the challenging content of Schwinger's beautifully delivered but fast-paced lectures, WK recalls a relaxed Quin also having the time to gaze through the classroom windows at passers-by.

Upbringing and education

Quin was born in Manhattan, where he grew up. His mother, Shirley, was the daughter of Lithuanian immigrants living in New York. His father, Paul, a man of enormous energy, had come to the United States from Palestine at age 15 and to that point was self-educated; Paul worked his way through New York University, becoming a well-known physician, an able scientist, and a strong advocate of socialized medicine. At home, he encouraged his children to experiment with chemistry kits, build crystal radio sets, and read about science.[4]

During most of Quin's boyhood, the Luttinger family (Paul, Shirley, Quin, an older brother, Lionel, and an older sister, Judith) lived in comfortable circumstances in the multicultural and multilingual environment of Greenwich Village, where Quin attended private preschool and elementary school. After completing the seventh and eighth grades in a single year, he was admitted to Stuyvesant High School, one of the best public science- and mathematics-oriented secondary schools in New York State. His interest in science was strongly reinforced by his daily interactions with his older brother, Lionel, who was drawn to science, especially chemistry, and had entered Stuyvesant four years earlier. Lionel had a strong influence on Quin—they often discussed science and philosophy and went regularly to the Hayden Observatory and the American Museum of Natural History. In addition, two scientists had regular summer contact with the Luttinger family at Woods Hole, a global oceanographic center: the biologist John Kiossian and the physicist Herman Yagoda. They supported Quin in his determination to devote his life to astronomy and cosmology, though his father urged him to follow in his own footsteps into medicine.

Quin realized early that to understand astronomy and cosmology he would have to know mathematics and physics. So as a high-school freshman he began studying introductory college texts. Before long, he and a friend were working through the problems in the rigorous mathematics text *Modern Analysis* (by Whittaker and Watson), and as a high school senior Quin was strongly influenced by Lindsay and Margenau's popular college text *Foundations of Physics*.

When his father Paul Luttinger died unexpectedly in 1939, the family found itself in dire financial straits. Unable to afford the costs of study at MIT, Quin entered Brooklyn College, where he met and was encouraged by Prof. William Rarita. Eventually, in 1943, MIT offered him financial aid for grading papers and allowed him transfer credit. Fulfilling various requirements by examination, and having written a senior thesis on "Energies of Critical Dipole Arrays" under the supervision of MIT Prof. Laslo Tisza, Quin graduated from MIT with a B.S. in physics in 1944.

During 1944 and 1945 Quin was part of a theoretical group located in Jersey City and directed by Elliot Montroll, a prominent mathematical chemist who became a good friend. In the spring of 1945 Quin was drafted, assigned to Fort Crowder in Missouri, and set to work on a radar project that led him to spend time at the MIT Radiation Lab in Cambridge. While there, he discussed some of his ideas for the statistical mechanics of a lattice of electric dipoles with Tisza. When the radar project was aborted, Quin returned to Missouri, where he worked on triangulation methods for locating mortar targets until he was discharged in the spring of 1946.

Quin's formal graduate studies at MIT were brief. He took his Ph.D. general examinations during the winter of 1946–47 and under Tisza's supervision completed his thesis, "Dipole Interactions in Crystals," in the spring of 1947. His favorite class was Prof. Witold Hurewicz's course on topology.

It was an exciting time in physics. Dramatic discoveries were occurring in quantum electrodynamics, nuclear magnetic resonance, and pi-meson physics—some in the very building where Schwinger's class met.

Postdoctoral and junior faculty years (1947-53)

As the first American postdoctoral fellow in Wolfgang Pauli's group after World War II, Quin demonstrated his brilliance in his contributions, partly with Res Jost, to the just-developed renormalized quantum electrodynamics. Especially noteworthy was Quin's 1948 calculation of the anomalous magnetic moment of the electron—carried out independently of, and about simultaneously with, Schwinger's calculation of the same quantity.[5]

The importance of this work has been underscored by Quin's approximate contemporary, the Nobel laureate T. D. Lee, who in 2010 wrote: "Quin Luttinger was one of the world's leading theorists in condensed-matter physics and field theory. In the late 1940s, he was one of the last research assistants of the great Wolfgang Pauli. When the news of [Polykarp] Kusch's measurement of the electron's magnetic moment reached Europe, Quin quickly made a calculation that explained in a simple and direct way the experimental discovery. Because of a delay by his mentor Pauli, Luttinger's most elegant masterpiece was published after Schwinger's paper on the same subject." Both Pauli and Schwinger received Nobel Prizes for their work in quantum electrodynamics.

Remarkably, a discussion with Quin in Zürich appears to have prompted the great Werner Heisenberg to send him a four-page densely handwritten letter (in German[6]) describing his just-published theory of superconductivity[7] to the "Highly honored Mr. Luttinger." Why did Heisenberg, one of the greatest scientific figures of his century, write so detailed a letter on a highly controversial subject to the very young Quin? WK suggests that Heisenberg may have had lingering doubts about the essential correctness of his theory[8] and had seen in the 24-year-old's sharp and independent intellect and mathematical originality a rare opportunity for helpful constructive criticism.[9]

In the year 1949–50 Quin held a Jewett Fellowship at the Institute for Advanced Study in Princeton. There he formed many friendships and scientific relationships, including with Robert and Kitty Oppenheimer, Georg Placzek, Hans Jensen, John and Klara von Neumann, Res and Hilde Jost, Hermann Weyl, Tsung-Dao Lee, and Chen-Ning Yang.

Quin's first faculty position was an assistant professorship of physics at the University of Wisconsin (Madison). There, for the two years 1950–52, as well as at the Institute for Advanced Study in 1949–50 and 1952–53, he explored—alone and with others—a variety of problems in quantum electrodynamics, pion-nucleon physics, and solid-state physics.

He was offered and accepted an associate professorship at the University of Michigan (Ann Arbor) in the fall of 1953.

Collaborations

My long and close friendship with Quin and our continuing collaboration on condensed-matter problems of theoretical and practical importance date back to the summer of 1953, the first of a string of 13 summers that we spent as regular visitors at Bell Telephone Laboratories. There we interacted strongly with the Bell Labs' outstanding resident theorists, including Conyers Herring, Philip W. Anderson, and Gregory Wannier, and with other less regular visitors, such as Philippe Nozières and John Ward. We also worked closely with several groups that were doing experimental projects in semiconductor physics—including cyclotron resonance, optical spectra in magnetic fields, and impurity states. Some of this work contributed to the basic scientific underpinnings of the transistor, invented in 1947,[10] and of computer hardware. The successes of our initial summer visits were said to have played a role in the Bell Labs' decision to form and support a permanent department of solid-state theory, which was soon to be widely regarded as the best in the world.

Our collaboration led to several groups of publications—many of which were stimulated by seminal results of experiments at Bell labs—that dealt with different aspects of solids' electronic structure and dynamics.

One major theoretical contribution of the Kohn-Luttinger (K-L) team was a generalized effective mass theory (EMT) applicable to the charge carriers in silicon and germanium—including non-isotropic mass-tensors for electrons near their conduction-band edges and a complex non-analytic structure representing the valence band holes. This theory is applicable to optical and magnetic effects and to weakly bound impurity states responsible for the electrical properties of these materials. EMT has become an integral part of the thinking about low-energy charge carriers in semiconductors and insulators, including the nanostructures of contemporary quantum electronics and optics.

For conduction electrons of silicon and germanium, moving in a field of static charges, this theory has the form of an effective Schrödinger equation expressed in terms of a) effective mass-tensors, mij, of the electrons in the conduction band and of the static classical dielectric constant. The phenomenological quantities entering this theory (effective masses and static dielectric constants) could either be determined experimentally or expressed in terms of the many-body wave functions of the system. For the valence band and its charge carriers, the so-called holes, the situation is analogous but more complex.

EMT invites comparison with the celebrated Landau-Fermi liquid theory for the dynamics of low-energy *metallic* electrons and holes under the action of external charges. Both theories are inspired by the physics of *non-interacting* electrons and holes, but they incorporate, through phenomenological parameters, the effective masses of electrons and holes as well as parameters describing the effective interactions between these charged carriers. In spite of these similarities, the two theories reflect, of course, the fundamental physical differences between semiconductors/insulators and metals.[11]

A second set of early K-L papers dealt with a quantum derivation of the classical transport (or Boltzmann) equation for a gas of non-interacting fermions under the influence of a weak, external, spatially uniform, accelerating electric field E(t), as well as of the action of randomly distributed scattering potentials $v(r-R_n)$. This set appeared more or less simultaneously with the very different derivation of the famous quantum theory of transport processes by R. Kubo, which, in a formal way, also include the effects of electron-electron interactions.

Other research in the mid1950s

While visiting UC Berkeley in 1954, Quin heard about some surprising Hall effect measurements. In ferromagnets, the size of this effect was three orders of magnitude larger than in non-magnetic metals; it exhibited a previously unheard-of temperature dependence; and it depended strongly on the unmagnetized resistance. Quin and Robert Karplus, who had recently moved to Berkeley from Harvard, argued that many, if not all, of these properties could be qualitatively accounted for by making reasonable assumptions about the effects of the external magnetic field and spin-orbit coupling.

A young Dutch theorist, J. Smit, questioned some of their arguments. With different assumptions, he argued that alternative explanations were possible, and the disagreement attracted considerable attention internationally for several years. WK recalls being greeted as he stepped off his plane in Moscow in 1957 with questions about the status of the Luttinger/Karplus vs. Smit controversy. In 1957–58, Luttinger revisited the problem using the Kohn-Luttinger impurity-scattering formalism. In a 1958 paper,[12] he compared and contrasted his assumptions and those of Smit, with whom he reported conferring, and he indicated, at least from his perspective, that none of the differences that remained were controversial.

Quin's research in the mid-'50s was not exclusively confined to fermions. In 1957, he coauthored with C. N. Yang and Kerson Huang a paper featuring the calculation of the first two terms of the expansion, in powers of (a/l), of the grand partition function of a low density Bose gas of interacting hard spheres (where a is the hard core diameter and l is the thermal wavelength).[13] As the authors noted, however, the expansion was valid only for the uncondensed phase.

The interacting electron gas in three dimensions (1957–64)

In 1957–58, Quin spent a fruitful year as senior postdoctoral fellow with the group of Philippe Nozières at the École Normale Supérieure in Paris. This was the period when many-body perturbation theory (to all orders) was being developed rapidly in several centers, especially in France and the Soviet Union, with important contributions also by the Englishman John Ward. From this period also dates the famous Luttinger theorem,[14] which states that, independent of the specifics of the lattice and the interactions of a condensed interacting electron gas in a periodic lattice in three dimensions, a discontinuous Fermi surface persists in the physical interaction that encloses the same volume as a non-interacting electron gas of the same average density. The theorem plays a very important role in interpreting and understanding the electronic properties of metals.

While in Paris, Quin accepted a senior professorship at the University of Pennsylvania, where he spent the next two years. In 1960, he made his final move to Columbia University and his beloved New York City, which would remain his base for the next 33 years. At Penn, and at Columbia through about 1964, the bulk of his research was devoted to interacting fermions in periodic lattices in three dimensions. He carried out some of these investigations alone or with students; others with senior collaborators (notably John Ward, Philippe Nozières, and WK). Many of these investigations had considerable impact on experimental work at Bell Labs and elsewhere.

Interacting fermions in one dimension (1963)

While much of Quin's research was carried out with others, the work that earned him the greatest acclaim—the so-called Luttinger liquid theory of interacting one-dimensional fermions—was done independently.[15]

What motivated him to undertake this research that many, including WK, consider to be his masterpiece? We can obtain a partial but significant answer from the first paragraph of his paper titled "An Exactly Soluble Model of a Many-Fermion System":

> *We shall be concerned in this paper with a model of a many-fermion system [that] is exactly soluble. The model is quite unrealistic for two reasons: it is one-dimensional and the fermions are massless. On the other hand, it has the realistic feature that there is a true pair reaction between the particles....Our main interest in the model is in connection with a question of whether or not a sharp Fermi surface exists in the exact ground state.*[16]

This Luttinger paper had an important forerunner, "Remarks on Bloch's Method of Sound Waves Applied to Many-Fermion Problems" by S. Tomonaga,[17] but Quin's paper did not cite it. When queried in 2013, WK said he did not know whether Quin knew of the existence of the Tomonaga paper at the time he submitted his own for publication.

Within the context of Western and Russian condensed-matter theory, the Luttinger work on one-dimensional interacting fermions represented an unexpected revolution because of its radical contrast with the three-dimensional interacting-fermion theory—the so-called Landau-Fermi liquid theory—that itself had revolutionized the theory of the interacting 3-D electron gas.

Coming back to Luttinger's introduction to his paper, he describes the model as unrealistic because the Luttinger fermions are one-dimensional and massless. However, the existence of one-dimensional elementary excitations that obey Fermi-Dirac statistics had already been well established—e.g., electronic excitations at one-dimensional edges—and many more examples were on their way. Also, real one-dimensional excitations with extremely small effective masses have been shown to have physical reality. Thus, in hindsight, one might consider Quin's introductory remarks to have been too negative. In fact, it can be argued that studies of one- and two-dimensional Fermi excitations have been among the most significant areas of recent research in condensed-matter physics.

The most striking difference between the 3-D Landau-Fermi liquid and the 1-D Luttinger liquid is the nature of their elementary excitations. In the Landau-Fermi liquid, the low-lying excitations are the familiar electrons and holes with electric charges of -e and +e, respectively, and spins of +½ and -½. In the Luttinger liquid, the interaction splits the electrons into chargons, with charge -e and spin 0, and spinons with charge 0 and spin ½. The splitting of the familiar holes is analogous. There are also radical differences in the 0 temperature momentum distributions.

Superconductors, disordered systems, mathematical theorems and techniques

Although the research for which Quin will be most remembered is his work on interacting electrons in one and three dimensions in the '50s through the mid-'60s, some of his subsequent research at Columbia, although less obviously applicable, is of theoretical and mathematical interest. Many papers display his mathematical originality and versatility. A few examples bear mention. Papers with Walter Kohn and with P. W. Anderson and N. R. Werthamer on superconductivity; papers alone and with R. Friedberg on disordered systems; papers alone on path integrals and isoperimetric inequalities, with E. H. Lieb, with Marc Kac, and with Friedberg on mathematical inequalities.

Quin Luttinger as recalled by P. W. Anderson

The following paragraphs, written in 2011 by Nobel laureate P. W. Anderson, eloquently summarize the lasting influence that Quin and his work have had on one of his most outstanding contemporaries and on the condensed-matter community at large.

> *The scientifically "golden years" of Quin Luttinger's physics productivity were also the years of the creation of the presently accepted canon of condensed-matter physics, to which he contributed massively. Yet his eponymous achievements—the Luttinger theorem, the Luttinger liquid (so named by Duncan Haldane, actually; Quin was ever modest), the Luttinger-Ward identities—keep turning up in the present era of major revisions of that canon. Those were also the years in which he spent much of each summer visiting the Bell Labs, and I like to think that it was the impetus from the practical experimental problems he encountered there that sparked his productivity.*
>
> *In fact, the Walter Kohn/Quin Luttinger team started out successfully in the mid-'50s with two problems of great interest to the Labs of the time: cyclotron resonance in multiband semiconductors, and the quantitative theory of donor electron states in Si and Ge. But rather than going on with a compendium of useful effects (though Luttinger did stop to clean up the de-Haas-van Alphen effect in 1961), Kohn and Luttinger realized that there was a systematic overall approach in the form of the quantum-statistical many-body theory that lay behind their successes and that enabled a series of increasingly fundamental contributions. First there came the long 1958 papers on the general quantum theory of electrical transport; then they realized that for real metals with nonspherical fermion surfaces the Brueckner-Goldstone diagrams they had been using needed to be supplemented by "anomalous" diagrams, in 1960;[18] and finally, Luttinger went on, partly with Nozières, to give diagrammatic meaning to the semi-phenomenological structure of Landau's Fermi liquid theory, in 1962,[19] after he had earlier discussed the analytic nature*

of the Fermi surface in detail. (The Russians somewhat resented the idea that anyone would presume to "prove" the ideas of the great Lev Landau—but it certainly made the rest of us more comfortable.)

Luttinger's personal scientific productivity seems to have slowed in the late '60s; yet I have several tokens of his visits to Bell after that period, including collaboration in 1965 when he helped clarify my ideas on the phenomenology of superconductivity. In 1969, when I was struggling with the Kondo effect and the Fermi surface orthogonality problem, I showed him the crucial but bewildering infinite determinant I had come up with and he instantly remarked "Oh, that's a Cauchy determinant" and gave me the 19th-century reference that solved my problem (again, that determinant led to modern ideas about entanglement entropy!)

Quin's scientific style, as embodied in his papers, was heavy and thorough; he never missed an opportunity to elaborate a difficulty or to qualify a conclusion; yet for all that their structure was spare and elegant, and in the end he made the crucial elements completely clear. But in personality no one could be lighter; when he came into our humdrum lives at Bell it was as an elegant and charming breath of fresh air, with his Mercedes Benz convertible coupe, his delightful sense of humor, and his broad acquaintance with the glamour figures of physics. He was also seminal in our transformation from what were really in the beginning—a group of "squalid state physicists" (in Murray Gell-Mann's immortal phrase)—into an intellectual force that I think still needs to be reckoned with.

One final personal remark: in 1956 I was insufferably pushing my ideas about localization at anyone who would listen—with very little success, I might add. Quin, of course, was at the time deeply involved in his and Walter's theory of transport, which my idea would seem to contradict, yet his response was: "Of course, it's bound to be true!" Surely my first believer.

Teaching

Lillian Hoddeson was a graduate student of Quin's at Columbia—an activity that overlapped with a research assistantship at Bell Labs—and she received her Ph.D. in physics in 1966. She subsequently became one of the leading historians of solid-state physics (an area later called condensed-matter physics). She was asked to write for this memoir her recollections of Luttinger as a teacher of graduate courses at Columbia University, and she contributed the following in 2011:

> *Quin Luttinger took his graduate lecturing extremely seriously and obviously spent much time preparing each and every class. It was intrinsically difficult to bring together as much information and insight as he attempted, and even more challenging to shape the imperfect subject of solid-state physics into a perfectly integrated account. Yet in seeking perfection and coherence, he managed to produce a seamless, logical, and economical picture of solid-state physics that was indeed beautiful, a basic message he conveyed with utter elegance.*
>
> *The beauty of Quin's lectures came partly from his gift for performance; experiencing his classes was a lot like attending a concert or opera. He was a master lecturer at the top of his game. The tones with which he spoke were well modulated and pleasant to listen to, almost melodious. He came off as natural, charming, and funny, without affectation, a bit shy, and yet clearly authoritative. Taking breaths between his sentences gave him time to find just the words and phrases he needed. His well-crafted sentences seemed to float in the air for a while, strung out across the classroom above us all like an airplane contrail.*
>
> *Unfortunately, a student could get caught up in the beauty of Quin's extraordinary performance and miss much of the physics behind it. The lectures were so clear that taking notes seemed unnecessary, as well as a rude corruption of Quin's perfect prose. In any case, one didn't want to miss a word, no matter what the lecture was on, whether it was merely categorization of symmetries of Bravais lattices, a presentation of symbols for representing the motion of harmonic crystals, or an exciting sneak preview of cutting-edge methods not yet found in the textbooks for exploring the Fermi surface or the phenomenon of superconductivity.*
>
> *Of course, it was in the end a mistake for students not to take notes. For when the contrail of the lecture dissipated, all that was left to study were the mimeographed course notes the students all purchased—notes nobly prepared by other grad students. These were invaluable when it came to exam time, but it was all too clear that they were but crude approxi-*

> *mations of Quin's perfect lectures. Still, they offered something to study. Quin may not have been aware of it, but just because his lectures were so beautiful, the students who most appreciated them often ended up learning less in class. Happily, for these students, Quin's approach to his graduate teaching offered a still deeper gift, that of motivation to work out the physics on their own and carry it farther with their own contributions.*

T. V. Ramakrishnan began his professional career in 1966 with a doctorate in physics from Columbia, having been mentored by Quin; his thesis was titled "Transport Properties of Metal in a Pseudopotential Model." Since then, Ramakrishnan has had a fruitful career as an intellectual leader in condensed-matter theory, with dozens of major recognitions from all corners of the world. About his experience as Luttinger's graduate student, he writes:

> *Quin Luttinger was an exceptional teacher. The two best teachers I had in graduate school were perhaps T. D. Lee, the particle physicist, and Quin Luttinger. Both were very clear lecturers, but with a difference. A student was more relaxed in Luttinger's lectures; the statements were as rigorous and careful, but there was an unusual ease and openness. I am told that Luttinger was later voted the best undergraduate teacher. I think of this as the greatest academic accolade, because undergraduates sample a lot and are not the acolytes that graduate students often are; and at Columbia, there were some really great scholar–teachers, so the competition for the honor was fierce.*
>
> *In Quin's lectures, the subject—however complex—seemed very simple. The solid-state physics course concentrated on effective mass theory for band semiconductors and on the theory behind various methods for the investigation of Fermi surfaces in pure metals. The many-body theory course was entirely about many-fermion systems. But nothing was ever evaded or pushed under the rug. Every single step was brought out and worked out. Because the starting point was well defined and familiar (e.g., the existence of bands or the free Fermi gas ground state), subsequent developments—such as the approximations used, the necessary formalisms, the small number of applications worked out in great detail and depth—all seemed inevitable and natural.*

> *For some years after the courses, as I groped my way into research in solid-state (or, more contemporarily, condensed-matter) physics, I resented having been exposed to so little, to not knowing the diversities , and perhaps to "struggles" culminating in the things described in the lectures. I now realize that this was a great blessing; the relatively timeless essence of the subject, often finally originating from Luttinger, was what we had been given, not a lot of words and vague approximations.*

Quin Luttinger as recalled by his nephew

Karl Luttinger, Quin's mathematician-nephew, wrote the following remarks in 2011 for this memoir:

> *Quin was my uncle and also a second father to me. He and my father Lionel (four years Quin's senior) also were my greatest teachers, and Quin, in turn, told me that Lionel was his own best teacher. So the three of us shared a special connection.*
>
> *Quin was an unusually cultured man and, in many meandering conversations, he taught me a great deal about his broad interests. In mathematics (my field), I would compare him to one of the great 19th-century analysts, or to Jürgen Moser of New York University, a mutual friend. Quin had deep insight and his abilities were truly breathtaking—I witnessed him constructing proofs of famous theorems within minutes of knowing only the statement of the result, with no clue as to how it was found. This was a kind of game we would sometimes play and he did things like this more than I could keep track of. As an example, I once told him about the Uniformization Theorem for Riemann surfaces, due to Poincaré, Klein, and Kobe, which is considered by many as one of the greatest results in 19th-century mathematics. Within less than two minutes he began outlining a proof built around a handful of simple details.*
>
> *Quin saw nature both as a source of wonderful mathematical questions as well as of wonderful answers. He had an outstanding intuition in physics, which he tried to explain to me. He felt that many mathematicians failed to grasp important features of the relationship between the two subjects. His youthful and flexible mind was capable of having very strong and persuasive opinions but even more capable of changing those opinions. He often said that one thing that helped him be a good scientist*

> *was seeing things from new vantage points with new sets of interpretations. He could radically change his thinking within an instant.*
>
> *Quin carried this openness into his personal life; it was connected to his irreverent and now sorely missed sense of humor. He was a remarkably touching, forgiving, and generous person. I still often find myself in delightful mental dialogues with him.*

Family

A transformative event in Quin's life was his marriage to Abigail ("Abby") Thomas—his first, her second—in 1970, when he was 46 and she was 29. Quin, who had been a footloose bachelor in New York City, became a family man virtually overnight in the suburbs with Abby, her three children—Sarah, Jennifer, and Ralph—from her first marriage, and a fourth child Catherine (his and Abby's) born soon afterward.

Abby writes:

> *Quin's curiosity and capacity for pleasure seemed boundless. He knew as much about art and literature and music as anyone in those professions. Tuesdays were his gallery days. He spent the afternoon wandering from place to place, looking at the work that interested him. He took his sabbatical in order to paint, and his paintings filled our house. He imitated the people he admired—his "Jim Dine" hung over the fireplace in our living room, his "Rothkos" were everywhere; he made light shows out of blinking Christmas lights stuck inside yards of air-conditioning tubing. He was always making something, doodling with whatever materials were at hand. He would invent a city out of watermelon rind when supper was over; the packaging was of more interest to him than what had been packed in it. He made sculptures out of styrofoam and wire and old cigar butts and twisted bits of this and that. I remember a drawing he made of a dog staring up at the moon. The caption read "Dog contemplating the universe." That was my favorite.*
>
> *The lovely part was that he painted and constructed these things for his own pleasure; he had no illusions and no ambition beyond delighting himself. He couldn't have stopped if he'd tried. He worked for years inventing a wind-up radio. I can't remember now how it was going to be powered, only that it was going to be far too heavy to be useful. He kept*

diaries; he wrote a wonderful science-fiction story that I incorporated in a story I published, which pleased him.

He couldn't really sing (which didn't stop him), and although he had taken piano lessons he never mastered the piano. But he loved music, rock 'n' roll as well as classical music and the then-new experimental stuff. He had as big a collection of rock 'n' roll songs as I did when we got married.

He was a snob. I remember falling in love with the novel Fear of Flying *and urging him to read it. He shook his head. "Why not?" I asked, "It's great." (I was in my late 20s, he was in his mid-40s). "Trivial," he said, which irritated me no end at the time. But his voice echoes in my ears when someone recommends a piece of contemporary fiction, and I think "Nope, trivial," but don't say it. Quin was also a great resource. If a line of poetry was rattling around in my head I'd call him and he could tell me the name of the poem, who had written it, when, and then go on to recite the rest.*

He was a good friend. He had a gift for friendship, particularly with women. He loved women, and women knew it and they loved him back. He was that rare find—a wonderful listener who wasn't waiting for the moment when he could chime in with his own two cents. He was genuinely curious and interested. He also had a very long span of attention.

Quin was by no means perfect. He had his faults, the most grievous being that he believed for a long time that he was wiser than anyone else. Then his daughter Catherine was born, and that knocked some sense into him. We were married for eight rather unhappy years but became good friends when we divorced. We remained close friends until the day he died. Quin was, as someone once described him, the most cheerful depressed person around. He was proud of a colleague having said, "What good is happiness? Look at Luttinger, he's happy without it."

Did I mention how funny he was? He was very, very funny. I remember him saying, often, "We only live once, if at all." He lived once. He enjoyed himself. He loved and was loved. Not a bad epitaph.

WK recalls Quin's special affection for children, from the time that they began working together during summers at Bell Labs in 1953. WK's children, at the time Marilyn and Ingrid, could hardly wait to meet him again and again.

Quin's stepdaughter Jennifer writes:

> *Quin was my stepfather and I adored him. He rode around New York City on his bicycle wearing a Batman hat well into his late 60s and went to art galleries. He was helpful to all in need. He stapled his clothes when they fell apart, made sculptures out of grapefruit rinds, and mostly had a good time. He loved his daughter Catherine like no one's business, and the rest of us as well. I am lucky to have had him in my life as long as I did. He took good care of my siblings and me in hard times. I miss him. Oh, and I know he was a hell of a scientist, and he loved his work and his students and colleagues as well.*

Quin's sister Judith writes:

> *Quin once told me, when I said I admired his achievements, that he always thought Lionel was the genius in the family. But Lionel's interpretation of their relationship was that although Quin was a little brat for the first 10 years of his life, Lionel thought that Quin, as he grew up, was the brilliant one and was very proud of him. To me they were both my brothers and I loved them.*

Quin's daughter Catherine writes:

> *[His passing] is the great grief of my life. Someone once said that when her father died it was like the doors to an enormous library swung shut forever. I feel the truth of this more and more as the years flip past and many (most) of my larger life questions remain unanswered. In a world where I can Google almost any curiosity conceivable, I seek answers from someone human. I seek answers from my father.*

Among scientists of his caliber, Quin was an exceptionally many-sided person. His family and friends—especially children—remember him primarily as a free spirit who brightened their lives.

His classroom students (mostly physicists, but also some others) considered him, and

Nobel laureate T. D. Lee, as the most gifted physics teachers at Columbia. Quin prepared his lectures with the greatest of care and presented them beautifully, always mindful of how his audiences followed and reacted to them. His lecture notes on solid-state physics (intended to become part of a book with WK) are on deposit at the Columbia physics library.

Quin had a relatively small number of fine postdoctoral fellows, including one who became truly extraordinary: T. V. Ramakrishnan. Following his Ph.D. thesis under Quin, Ramakrishnan published several seminal papers, including "First-Principles Order-Parameter Theory of Freezing" (with M. Yussouff)[20] and "Disordered Electronic Systems" (with P. A. Lee),[21] and was honored by his election to the Royal Society of London and to the presidency of the Indian Academy of Sciences.

Quin's colleagues, particularly semiconductor physicists, valued his work highly from the early '50s on. But not until the publication, in 1963, of his transformative masterpiece "An Exactly Soluble Model of a Many-Fermion System" (in which he presented the Luttinger liquid) did they begin to realize his great impact on the subsequent evolution of condensed-matter physics.

NOTES

1. Luttinger, J. M. 1963. An exactly soluble model of a many-fermion system. *J. Math. Phys.* 4:1154.
2. An earlier (1950) paper by Sin-Itiro Tomonaga (*Prog. Theor. Phys.* 5:544) with many of the same concepts as Reference 1 was not known to Luttinger at the time he manuscript .
3. Independently of Tomonaga and Richard Feynman, Schwinger was engaged at the time in removing a tremendous obstacle posed by quantum electrodynamics—his aim was to make this internally inconsistent theory nevertheless usable for the quantitative interpretation of experimental data. He finally accomplished this goal in 1956, and the three shared the 1965 Nobel Prize in Physics.
4. Luttinger, J. M. 1948. Magnetic moment of the electron. *Phys. Rev*. 74:893.
5. Schwinger considered this specific part of his own scientific work as especially significant; the expression $\alpha/2\pi$, which referred to it, is engraved above his name on his tombstone.
6. German and English versions of this letter are accessible through the library of Columbia University.
7. The term "superconductivity" refers to the sudden and complete disappearance of electrical resistance of many metals as they are cooled down through their so-called critical temperatures. This phenomenon had been experimentally discovered by Dutch physicist Heike Kamerlingh Onnes in 1911 and its microscopic origin had remained a deeply frustrating mystery since then, even after Heisenberg's formulation of quantum mechanics.
8. Today, in hindsight, we know that Heisenberg's theory lacked a crucial element: the condensation of electron pairs.
9. I want to add the following from my own acquaintance with Luttinger's scientific work over almost his entire career: While hundreds of physicists could not resist the mystery of superconductivity, Luttinger, who by his nature often preferred to "march to his own drummer," (e.g., the "strange" 1-dimensional Luttinger liquid), was not one of them.
10. Bardeen, J., and W. H. Brattain. 1948. The transistor, a semiconductor triode. *Phys. Rev.* 74:230.
11. Although the K-L derivation of the EMT for semiconductors was well known in the Soviet Union, this writer is not aware that it influenced Landau in his formulation of the Fermi-liquid theory of metallic electrons, which followed a few years later.
12. Luttinger, J. M. 1958. Quantum theory of electrical transport II. *Phys. Rev.* 109:1892; OR Luttinger, J. M. 1958. Theory of the Hall effect in ferromagnetic substances. *Phys. Rev. 1* 12:739.

13. Huang, K., C. N. Yang, and J. M. Luttinger. 1957. Imperfect Bose gas with hard sphere interaction. *Phys. Rev.* 105:776.

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16. Ibid.

17. Tomonaga, S. 1950. Remarks on Bloch's method of sound waves applied to many-fermion problems. *Prog. Theor. Phys.* 5:544.

18. Kohn, W., and J. M. Luttinger. 1960. Ground-state energy of a many-fermion system. *Phys. Rev.* 118:41.

19. Luttinger, J. M., and P. Nozières. 1962. Derivation of the Landau theory of Fermi liquids I. Formal preliminaries. *Phys. Rev.* 127:1413; or Luttinger, J. M., and P. Nozières. 1962. Derivation of the Landau theory of Fermi liquids II. Equilibrium properties and transport Equation. *Phys. Rev.* 127:1431.

20. Ramakrishnan, T. V., and M. Yussouff. 1979. First-principles order-parameter theory of freezing. *Phys. Rev. B* 19:2775.

21. Lee, P. A., and T. V. Ramakrishnan. 1985. Disordered electronic systems. *Rev. Mod. Phys.* 57:287.

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