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HENRY PRIMAKOFF

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A Biographical Memoir by

S.P. ROSEN

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BY S. P. ROSEN

HENRY PRIMAKOFF, THE FIRST Donner Professor of Physics at the University of Pennsylvania, was a theoretical physicist well known for his contributions to condensed matter physics and to high energy physics. His name is associated with spin waves in ferromagnetism, with the photo-production method for measuring the short lifetimes of neutral mesons, and with an underwater shock wave. He became a leading authority on weak interaction phenomena in nuclei, such as double beta decay, muon capture, and neutrino scattering. He was an outstanding teacher and had a unique influence upon all of his students.

EARLY YEARS

Henry Primakoff was born in Odessa, Russia, on February 12, 1914, and died in Philadelphia on July 25, 1983. In life he had come a long way, from an early childhood in a city beset by war and revolution, through an arduous and often dangerous journey from Russia into Romania and across more than half of Europe, from Bremen to the lower Bronx, and ultimately to the City of Brotherly Love where a long battle with cancer awaited him. In sickness and in health, Henry bore himself with great courage and zest for life and his final years were filled with as much involvement in the world around him as at any other period of his life. He died peacefully in the midst of his family.

Through his mother, Henry was descended from a large, assimilated Jewish family of merchants who had lived in Odessa for several generations. Through his father, Henry came from a Greek-Orthodox family of wealth and prestige. His paternal grandfather married a Jewish woman and was banished from the family estate; years later his father did the same. After the Russian Revolution, one granduncle rose to become a general in the Red Army, but he was executed by Stalin in the famous 1937 purge of the army. Khrushchev subsequently rehabilitated General Primakoff, and a statue in his honor is said to stand in Kiev.

Henry's father was born in Kiev, studied medicine, and graduated as a doctor in 1911. His mother, a strikingly beautiful woman, came to Kiev to study pharmacy after graduating from the gymnasium in Odessa, and it was through the medical connection that his parents met. During the First World War his father became an army doctor and was wounded while operating on soldiers behind the front lines. He joined his wife and young son in Odessa at the end of the war, but died a few months later in 1919. At his funeral the Red flag was flown and the Internationale sung.

About two years later, Henry's mother and her parents decided to leave Russia and join an uncle who had settled in New York. This required escaping across the nearest border, the Prut River, into Romania, trudging for long night hours through woods, and hiding by day in remote farmhouses. Eventually they found a haven on the farm of some relatives about five hours by train from Bucharest. Henry was instructed not to talk to his mother when they went into town, because it was too dangerous to speak Russian in that part of Romania at that time. He and his family received travel documents from the embassy of the Kerensky government in Bucharest and set out on the long train journey through war-torn Europe to Bremen, and thence on the steamship "der Flieger" to New York, where they finally settled in 1922.

Once established in the lower Bronx, Henry made rapid progress in his new language and in school, although he had some startup problems with four-letter words. They were used so frequently by his classmates that he took them to be a normal form of greeting. One day a much bigger kid approached him with a friendly "Hi!," to which Henry gave a four-letter response. The kid recoiled in astonishment and prepared to slug this insolent, foul-mouthed immigrant; but fortunately for Henry, some other kids realized the problem and saved him from a beating in the nick of time. From that time on he was much more circumspect in his usage of four-letter language.

In high school Henry took an active interest in politics and journalism, becoming editor of the school paper one year and president of his class another. He read widely and had an excellent all-around academic record, ending up as second best student in the entire school. He won scholarships to Columbia and Harvard and opted for the former despite advice from his granduncle to the contrary. In the fall of 1931 Henry began his freshman year at Columbia.

His interests in college were sufficiently broad that he did not become really serious about science until the beginning of his junior year. On entering Columbia he was quickly disabused of early ideas for a career in journalism by his experiences on the college paper, and he gave some thought to the study of literature or philosophy. The interest in philosophy stood Henry in good stead several years later when he interviewed for the Harvard Society of Fellows and was able to hold a lively conversation with the famous philosopher A. N. Whitehead for more than two hours. The fellowship, however, went to John Bardeen, a future Nobel laureate.

By the middle of his junior year Henry was concentrating more and more upon physics. He and five or six like-minded students formed an informal club to study special relativity from the short volume by Tolman, which had just been published. The club went on to distinguish itself in the world of physics, its members including Norman Ramsey, Nobel laureate; Herbert Anderson, student of Fermi and co-discoverer of the (3,3) resonance in meson-nucleon scattering; Robert Marshak, co-inventor of the universal (V-A) form of weak interactions and founder of the Rochester conferences in high energy physics; and Arthur Kantrowitz, former director of the Avco Everett Research Laboratory.

Henry spent his senior year at Columbia taking graduate courses and in one of them, a laboratory course, he met Mildred Cohn, who was to become his wife and a distinguished chemist known for the application of nuclear magnetic resonance to biochemistry. During this year the club members became aware of the need for graduate school if they were to become professional physicists; Henry applied to Princeton and was accepted.

In those days there was little financial support for graduate studies. One had to be prepared to pay tuition (about $100) and support oneself for at least the first year. With help from his family and money either saved from his undergraduate scholarships or earned from various odd jobs Henry managed to stay at Princeton for a year. New York University then offered him a fellowship and he went back to New York to complete his Ph.D. He received his degree in 1938 and married Mildred in May of that year.

Despite the bleak economic climate of the times, Henry and Mildred both ended up with jobs in New York, Mildred in the Biochemistry Department of Cornell Medical School and Henry first at Brooklyn Polytechnic Institute and then at Queens College. After Pearl Harbor he began to work on a navy project concerned with sonar and submarines. Oppenheimer approached him to join the Manhattan Project, but he refused on the grounds that he wanted to work on projects for the present war and not the next one. He did not believe that an atomic bomb could be built in a reasonable time, and was greatly surprised by the news of Hiroshima.

When the war ended Henry accepted a joint physics and mathematics appointment at NYU. Richard Courant, founder of the institute that bears his name, wanted to have his mathematicians interact with a physicist and he chose Henry for the job. A year later Arthur Hughes and Eugene Feenberg persuaded Henry to join the physics faculty of Washington University in St. Louis. Mildred eventually arranged to work in Carl Cori's department at the medical school, and so in 1946 they began a new chapter in their lives in St. Louis, Missouri.

PHYSICS RESEARCH

Henry wrote his first paper while still a graduate student on "second and higher order processes in the neutrino-electron theory." In it he and co-author M. H. Johnson calculated the forces between neutrons and protons due to the exchange of virtual neutrino-electron pairs in the new Fermi theory of beta decay. It was a prophetic choice of topic because Henry came to devote a great deal of effort in the postwar era to weak interactions in nuclei.

Possibly the best and most significant paper that Henry wrote was also started while he was a graduate student. He and fellow student Ted Holstein were studying the field dependence of the intrinsic magnetization of a ferromagnet at low temperatures when they had the ingenious idea of expressing the spin operators that appear in the Heisenberg exchange interaction model in terms of boson creation and annihilation operators. With appropriate approximations, which turned out to be equivalent to approximations used by Bloch and by Moeller in a very different and less complete treatment of the problem, they were able to diagonalize the Hamiltonian, including magnetic interactions as well as exchange and dipole-dipole interactions.

The essential idea of this approach is that, while most of the atomic magnetic moments in the ferromagnet will line up with the external magnetic field, there will always be a few that, because of temperature agitation, deviate from complete alignment. By means of the boson transformation, Holstein and Primakoff showed that the spin deviations were not localized on a particular atom, but propagated through the crystal in spin waves. Spin waves, originally proposed by F. Bloch, are regarded as the principal modes of excitation of ferromagnets, and in recent years they have even entered nuclear theory.

Although this paper has become a classic and was reprinted as such in a recent Japanese collection, its importance was not recognized until several years after the end of the Second World War. The first reference to it that I have been able to find is by Akhieser in the *Journal of Physics* of the U.S.S.R. in 1946. In this country Van Vleck published a survey of the theory of ferromagnetism based on a lecture given in Paris in 1939, which was "amplified considerably," in *Reviews of Modern Physics* in 1945 without referring to Holstein and Primakoff. By 1958, in a review of spin waves with Van Kranendonk in the same journal, he was referring to their work as "the conventional approach." The story of how this change came about, at least insofar as this author can trace it, is quite interesting.

It began in 1946 with the discovery by J. H. E. Griffiths of Oxford University of ferromagnetic resonance effects of unusually large frequency compared with the Larmor precession of electron spin in a magnetic field. Charles Kittel, then at MIT, gave a classical interpretation of the anomaly in 1947 and a year later D. Polder, of Bristol, derived the Kittel formula using quantum mechanics. In his derivation he used the method of Holstein and Primakoff for describing the quantum mechanical states of a ferromagnet and the corresponding energy eigenvalues. Subsequently, Luttinger and Kittel used an "ingenious but somewhat devious method" directly to calculate the ferromagnetic resonance frequencies from one term in the appropriate Hamiltonian. Once the relevance of quantum mechanics to ferromagnetism became firmly established the paper of Holstein and Primakoff received its due recognition.

It is interesting to note that, even though their work played a seminal role in theories of ferromagnetism and anti-ferromagnetism in the 1950s and even though the Holstein-Primakoff transformation is famous to this day, neither Holstein nor Henry ever worked on this subject again, and as far as I can ascertain, they never applied their methods to other problems.

Although Henry never worked on the Manhattan Project itself, he did do some research relevant to the Bikini underwater test that led to the discovery of what some authors have called the Primakoff wave. The problem with the test was whether it would set off a severe tidal wave that would do serious damage in the Pacific Basin. Applying methods used by G. I. Taylor for shock waves in air, Henry found a simple, exact solution for the shock wave problem in water at high energy and showed that the properties of this wave, including its height, are all determined solely by its energy. The result was never published in the open literature, but it is described and attributed to Henry by Courant and

Freiderichs on page 424 of their book on "Supersonic Flow and Shock Waves."

In his early years at Washington University Henry wrote two papers with Eugene Feenberg, one on collapsed nuclei and the other on the interaction of cosmic-ray primaries with starlight and sunlight. The former anticipated later ideas of T. D. Lee and G. C. Wick on super-dense matter; and the latter showed that cosmic ray primaries should consist mainly of protons because energetic electrons would undergo too much scattering from photons in intergalactic space (through the inverse Compton effect) to reach the vicinity of the Earth. Henry also wrote papers on muon decay, muon capture, and hypernuclei, which were of considerable interest to cosmic ray physicists, the intellectual forerunners of present-day high energy physicists.

In a seminal paper of this era written with S. DeBenedetti, C. E. Cowan, and W. R. Konneker, Henry derived the basic formulae for the angular distribution of photons from positron annihilation in solids; it is still quoted today. He also wrote his initial papers on the Primakoff effect and on double beta decay during this period.

While at Washington University Henry would receive offers from other institutions from time to time. In almost all cases, the letters offering him a position would end with words to the effect, "We can also find a job for your wife." There was one case, however, that involved a reversal of roles between Henry and Mildred. In 1948 Johns Hopkins made an offer to Mildred and closed it with the statement, "We can also find a job for your husband." She did not accept it, and they remained in St. Louis until 1959.

PERSONAL RECOLLECTIONS

I first met Henry Primakoff in the fall of 1955, when he was spending a sabbatical leave at the Clarendon Laboratory in Oxford, and I was a graduate student in search of a thesis topic. Henry had given a seminar on double beta decay and Roger Blin-Stoyle, my supervisor, suggested that I investigate some of the problems raised by Henry. This proved to be the beginning of an association that was to last for twenty-eight years.

Double beta decay is an extremely slow process, being in a certain sense a succession of two ordinary nuclear beta decays, but it is closely tied to important questions regarding the neutrino. If the neutrino has a mass, and if it is its own anti-particle, then it is possible for double beta decay to take place without the emission of neutrinos in the final state; the neutrino emitted in the first ordinary beta decay is reabsorbed in the second. With the advent of the standard model for particle physics, the study of neutrino properties has become an important avenue of exploration for the new physics.

In 1957 I became a research associate at Washington University in St. Louis and after crossing the Atlantic on the Queen Mary, I took the train from New York to St. Louis. Henry picked me up at the station, but locked his car with the keys inside. While we were waiting for the man from Triple A to do the old coat-hanger trick, Henry invited me to join him in writing a review article on double beta decay. I accepted without hesitation.

He had been asked to write the article the previous year and had already missed one deadline; he needed a collaborator to help him meet the next one. As it happened, we missed that deadline, too, by a few weeks and our article was not published until 1959. It formed a bridge between the original work of Maria Goeppert-Mayer, Majorana, Racah, and Furry in the 1930s and the important developments brought about by the discovery of parity nonconservation and the two-component neutrino in the second half of the

1950s, and it remained a standard reference for many years. It also provided a useful starting point for the modern developments in double beta decay of the 1980s.

My appointment at Washington University ended in 1959, and I went to work for the Midwestern Universities Research Association in Madison, Wisconsin. A year later Henry accepted an appointment as Donner Professor of Physics at the University of Pennsylvania; it was to be the final move of his career.

For the next ten years Henry and I kept in close touch with one another, but our interests went in different directions. Henry investigated the fundamental properties of the weak interaction in various settings, while I began work on flavor symmetries of elementary particles. With the introduction of the universal V-A interaction for all four-fermion weak interactions in 1958, Henry had realized that the rate for muon capture in nuclei, the second leg in the Puppi triangle, would be sensitive to the hyperfine splitting of the parent atom. Jeremy Bernstein, T. D. Lee, and C. N. Yang had independently discovered the same effect and they invited Henry to join them in publishing the result. He went on to develop an extensive theory of muon capture in nuclei, which culminated in the elementary particle treatment put forward by C. W. Kim and himself and relied upon the use of the Goldberger-Treiman relation in complex nuclei. At the same time Henry examined the behavior of elementary particles themselves; with P. Dennery and J. Dreitlein he studied rare decays of the muon, the relationship between the decays of charged and neutral Sigma hyperons, and semi-leptonic decays of K-mesons. With Ephraim Fischbach and other students he investigated parity-violating nuclear forces, a topic that reached back to his very first paper.

In the overlap between Washington University and Pennsylvania Henry wrote two seminal papers with Arden Sher on the approach to equilibrium in quantal systems. Max Dresden has described these papers as a "brilliant exposition" that clarified a field in which much confusion had existed.

Another idea that Henry extended in this era had its origins in a paper he wrote in 1951 on the photo-production of the neutral pion in the electric field of the nucleus. The essential point of this paper, which has come to be known as the Primakoff effect, is that, under certain well-defined kinematic conditions, the photo-production of neutral pions is controlled by exactly the same interaction as the decay of the pion into two photons. This means that the lifetime of the meson, which is difficult to measure directly, can be extracted from measurements of photo-production, an easier task. In collaboration with C. M. Andersen and A. Halprin he extended this approach to the lifetime of the newly discovered eta meson, a pseudoscalar meson like the neutral pion, and to the production of vector mesons. The Primakoff effect has proved to be a most effective method of measuring neutral meson lifetimes and it is now the standard one.

In 1969 Henry invited me to work once more with him on double beta decay. He had been intrigued by a clever argument by Pontecorvo indicating some tentative evidence for the occurrence of no-neutrino double beta decay. One motivation for searching for the decay mode without neutrinos is the identity of the neutrino and its anti-particle, or equivalently the question of a conservation law for leptons analogous to that for electric charge. There is considerable empirical evidence for such a law, but the most sensitive test occurs in double beta decay, partly because of the energies available to the exchanged virtual neutrino and partly because of the helicity suppression imposed by the two-component neutrino. The lack of complete helicity suppression can be parameterized either by a neutrino mass or by a small admixture of a leptonic current with opposite helicity from the dominant left-handed current. We chose the opposite helicity current on the grounds that the then existing limit on the neutrino mass was much too small to yield an effect of the magnitude anticipated by Pontecorvo, and by ourselves. In recent years the nuclear physics argument proposed by Pontecorvo, simple and elegant though it is, has fallen out of favor, and the modern particle physics point of view emphasizes neutrino mass rather than right-handed currents as the basic helicity parameter.

We did eventually write a paper about neutrino mass and double beta decay, but we dealt with heavy neutrinos instead of light ones. In 1976 Gell-Mann, Ramond, and Slansky proposed the seesaw mechanism for neutrino masses; they argued that the very light, left-handed neutrinos participating in weak interactions were Majorana particles and should have very heavy, right-handed partners. Arthur Halprin realized that heavy Majorana neutrinos could also give rise to no-neutrino double beta decay, and in collaboration with him and P. Minkowski, we showed that the existing limits on the process gave rise to a lower bound on the masses of such heavy neutrinos of several times the proton mass. This is entirely consistent with the seesaw model.

Our last paper together, like the first, was a review article. Henry had been asked to review the subject of baryon number and lepton number conservation laws for the *Annual Reviews of Nuclear and Particle Science,* and again he invited me to join him. "You realize that you may have to finish it by yourself," he said. When I put down the phone I cried; I knew of his illness, but the stark confrontation with its severity overwhelmed me.

Fortunately the worst did not happen, and we finished the paper together. We met frequently for the next year, worked hard on the paper, and this time we beat the deadline. Most of our meetings were in Philadelphia, but the last one was in West Lafayette. Henry spent a week with me putting the finishing touches on the paper and he seemed to thrive on a demanding regimen of early mornings and late nights even though one leg was impaired by his illness.

Our review summarized all that we had learned over the years about particle number conservation laws and how any breakdowns of these laws would become physically manifest. Up to now none of these manifestations has been detected, but there is one that does have a chance of being definitively observed in the near future, namely neutrino oscillations. Pontecorvo proposed oscillations as the solution to the solar neutrino problem in 1968 and Henry was excited by the idea almost as soon as he read about it. He told us all about oscillations and encouraged us to pay serious attention to the possibility that they might actually play an important role in the physical world. It is sad that Henry did not live to see the most recent developments in the field, both the matter enhancement theory of Mikheyev, Smirnov, and Wolfenstein, and the beautiful experiments on solar neutrino-electron scattering, which were originated by his colleague A. K. Mann, and on the gallium reaction. He would have been delighted.

Henry was a master of the old school of theoretical physics. Not only did he make major contributions to several diverse fields of physics, but his interests and attitude towards physics brought him into close contact with experiment. He did not like to stray too far from experiment and he rarely speculated about things that were beyond experimental reach. Nevertheless he had his own share of clever ideas, and he greatly admired the ingenious ideas of other people. Bruno Pontecorvo was one such person. Henry immensely admired him for his inventiveness and thoroughly enjoyed the clever ideas he put forward. I do not know whether the two men ever met, but I am certain that any meeting between them would have sparkled with wit and warm admiration.

Given Henry's liking for scintillating ideas, which might be categorized as belonging to the elegance of Paris and Rome, it is somewhat surprising that his writing style, at least in physics, tended toward the Germanic. He seemed compelled to pile on top of one another all the qualifications required for a particular word or phrase; he could not bear to postpone a qualification for one or two sentences for fear that his statement might appear ambiguous or inaccurate. Likewise with his notation. Henry never omitted a single subscript, superscript, or any other qualification. He put down everything! His notes are so unusual that they have been used to decorate the covers of conference proceedings.

Discussing physics with Henry was always a pleasure. He was so full of knowledge and ideas that we would go on for hours without noticing it. But conversations with him were never completely serious. He had a lively and whimsical sense of humor, and he always enjoyed a fanciful diversion. One always came away from them encouraged and happy to go on.

I would like to close this memoir with the remarks I made at a dinner in honor of Henry nineteen months before he died.

REMARKS IN HONOR OF HENRY PRIMAKOFF
DECEMBER 18, 1981 AT PHILADELPHIA

I have been asked to speak tonight on the topic of "working with Henry." It will be a great pleasure to do so, but I would like to begin by drawing upon the vast reservoir of "Henry stories." Everyone has his own particular favorites, and so here are a few of mine.

The first one, which may be apocryphal, is about a seminar at Queens College years ago. When the talk was over, Henry walked out of the lecture and took a coat from the racks by the door. He put it on, thrust his hands in the pockets, and wandered off. Within a few minutes Henry returned, the skirts of the coat trailing on the floor. A rather tall chap was now poking around the racks, obviously looking for a missing coat. Henry came up to him and, drawing his hands out of the pockets, said "Excuse me please, but are these your gloves?"

Another story is told of a dinner at Washington University, much more formal than this one, honoring some eminent man of academe. Henry, seated at the head table, must have found the after-dinner speeches a trifle tedious, because he soon fell asleep. Now in those days Henry was a smoker, and on this particular occasion he had neglected to put out his cigarette. Thus, when the eminent speaker suddenly sensed in the middle of his speech that he had completely lost the attention of the audience, he looked around to see Henry asleep with his hair on fire!

Sleep has always been important to Henry. For many years he has practiced the fine art of falling asleep during the body of a seminar, but waking up just in time to ask the most penetrating question. Even when his question revealed his previously dormant state, he rarely failed to put the poor speaker on the defensive. Perhaps Henry's greatest tribute to this high art is his definition of insomnia: "When you can't fall asleep in a seminar!"

Now let me turn to the principal subject of these remarks, "working with Henry." You have all seen this "everywhere dense" page of Henry's notes reproduced on the cover of the program. Well, I can summarize the job of a collaborator, and I speak from more than twenty years experience, by saying that he has to translate every mark on this page into words and sentences that others can comprehend. Bob Marshak observed in his talk this afternoon that, whereas most people tend to put down the first factors in an expression explicitly and indicate the rest by dots, Henry never likes to use dots. He puts down everything. His symbols are always fully clothed with all their superscripts and subscripts, parentheses and sundry other brackets. As a result, his papers make a unique visual impact upon the reader and you can readily spot one of them as you flip through the pages of the *Physical Review.*

Henry's writing is really no different from his symbols. Whenever a word or a phrase requires several qualifications, he feels impelled to get them in all at once; and so he piles them in before, during, and after the phrase or the word. He seems not to want to break up one complicated sentence into several simpler ones for fear that the reader may lose track of what qualifies what.

I have been fortunate to work with Henry on very rare and long-lived processes, on whose time scale twenty years is but the blinking of an eye. So we have been able to come back to them every few years and still find new and interesting things to say. When our first article appeared, in 1959, a reviewer wrote in the English *Journal of Physics* that he could not understand how two people could write so much about a process that no one had ever seen! Well, rather than be deterred by that remark, we have gone on from slower to even slower processes, and in our latest work we write about one process, proton decay, which would make our original process, double beta decay, seem like a horse race.

An occasion like this brings back memories of my earliest meeting with Henry. He had come to Oxford on a sabbatical leave, and I was a young graduate student just finished with prelims. How fortunate I have been that a series of happy accidents brought us together! That Henry would come when he did, and Roger Blin-Stoyle should suggest to me, instead of any one of four or five students, that I do a thesis on a subject of interest to Henry. That Henry should have a job when I needed one! So many turnings could have been missed, so many roads not taken! But, happily for me, nothing did go amiss.

I remember being fascinated by Henry's voice and gestures when he lectured. He has that wonderful, melodic accent, the likes of which I had never heard before, and I decided that it had to come from the South—the home, for many Englishmen like myself, of all things gracious and exotic. It was not until several years later that I learned that the accent had really come from Russia by way of New York.

Henry's gestures were to his lectures and to his conversation as punctuation marks are to the written word. His right hand would reach out and carefully place a full stop in the air; his whole body would reach out as though it were an exclamation mark or curl around to form a question mark. Whenever you had a conversation with Henry you would find by the end of it that he was either wrapped around the chair or wrapped around you.

As his students have already testified tonight Henry is a very kind and considerate man. Moreover, he does his good deeds in a quiet and unobtrusive way. In my own case he was very helpful at a time when I thought seriously of leaving physics as a career. He led me gently back into the field, not by exhortation, but by discussing the problems in physics that had brought us together a decade earlier. I shall always be grateful for his friendship and his wisdom.

We all know that, in the last few years, Henry has been afflicted with a grave illness. Indeed, all of us have suffered with him during this period of his life; but we cannot fail to admire the fortitude with which he has borne his affliction, the grace and nobility with which he has faced it. If there is amongst us a true prince of the spirit, then surely that man must be Henry Efromovitch Primakoff.

Thank you, Henry.

I am very glad that I had the chance to say these things directly to Henry. The pain of his passing, though it will never cease, does diminish with time and those who knew him well are left with many happy memories to enjoy.

I THANK MILDRED COHN Primakoff for encouraging me to write this memoir and for allowing me to view the transcript of a memoir dictated by Henry in his last years. I am grateful to Norman Ramsey and Robert Marshak for sharing their recollections of the junior year club at Columbia with me, and to J. B. Keller and Cathleen Morawetz for telling me about the Primakoff wave. Max Dresden emphasized the seminal character of the work on statistical mechanics. P. W. Anderson and Charles Kittel provided me with valuable clues on the early history of the Holstein-Primakoff transformation. However, I take full responsibility for the interpretation of history presented here.

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