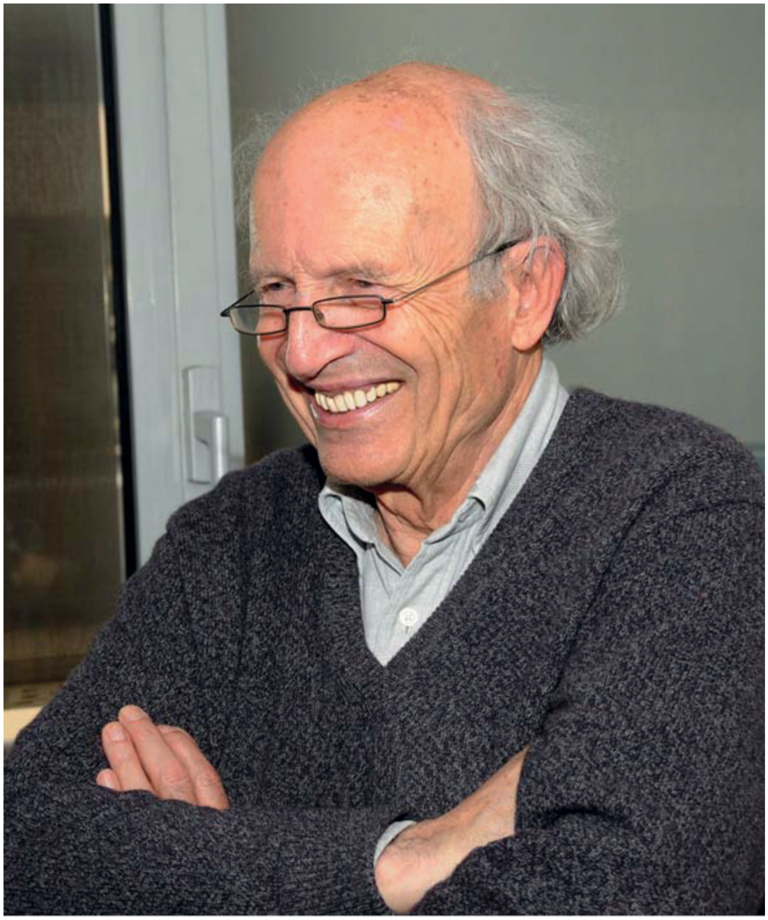

Sixty Years of Condensed Matter Physics: An Everlasting Adventure

Philippe Nozières

Institut Laue-Langevin, 38042 Grenoble Cedex 9, France; email: nozieres@ill.fr

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Abstract

Condensed matter physics has changed since the fifties: I attempt to retrace its evolution in the light of my own trajectory. It was and it remains a living field, in constant renewal. New ideas, new concepts keep appearing along with new experimental and theoretical tools. The danger lies in the bureaucratic evolution of scientific research, which might sterilize imagination and innovation. The future lies in the hands of young physicists who should defend their independence and creativity against fashions and competition.

I discovered condensed matter physics in 1952 in the semiconductor lab of Pierre Aigrain in Paris. I was 20, my boss was 28, coming every morning with ten new ideas. Nine of them would be silly, but one smart. One good idea per day remains impressive! We were trained to select the good one, a challenging education. The lab was poor and we had to build all the equipment ourselves. My experimental master's thesis was on point contact transistors, an ancestor of semiconductor devices. In those days modern physics hardly existed in French universities; we learned quantum physics by reading books. My generation was salvaged by the summer school in Les Houches, just created by Cecile De Witt. A course by Rudolph Peierls on condensed matter theory was a unique experience!

I became a theorist by accident. I was offered a one-year fellowship at the Princeton Graduate School. One year in the United States was an adventure in 1955, something like the gold rush. I accepted immediately! But one year was short and I thought I might as well learn some theory. I thus joined David Pines, who introduced me to many-body physics. He was one of its founders, and I was moved by his enthusiasm. I know my debt to him; our collaboration was sometimes fierce, but always friendly and productive. Together we discovered the power of diagrammatic techniques in statistical physics. In 1958 we organized the first topical session on many-body physics in Les Houches. The community was small and many leading figures were there: I was very much impressed by Beliaev's lectures (1) on Bose liquids.

Princeton allowed me to stay a second year. I spent the summer of 1956 at Bell Labs to return to France in between (I had to pay for my flight—two months' salary!). That turned out to be an extraordinary experience. In addition to the Murray Hill staff (Conyers Herring, Phil Anderson, etc.), the Lab attracted the Gotha of theorists during the summer: I could chat daily with Walter Kohn, Quinn Luttinger, El Abrahams, etc.—I was immersed in a fascinating, boiling world, full of new ideas. Conyers Herring asked me to look at cyclotron resonance of graphite [my first solo paper in *Physical Review* (2)]. I was thus exposed to the queer band structure of a single layer (now known as graphene). Near Fermi level the bands have a conical point with linear dispersion. That singularity is split in 3D due to interlayer coupling, and I managed to interpret cyclotron resonance. But neither I nor any of these famous people ever suspected what was hiding behind that linear dispersion. Fifty years later, graphene became a frontier of physics with far-reaching quantum effects. The lesson is worthy of some comments: When a problem is not ripe you simply do not see it. The same situation occurred with the quantum Hall effect, which could have been discovered much earlier.

Just after Les Houches I was drafted in the French navy for two and a half years. I was in charge of building a seismic network to detect atomic explosions. I was happy to return to experimental work, which I liked. There I learned another lesson, which I never forgot. I only detected an underground explosion in Nevada because I knew where to look; the signal was comparable to that of a cow rubbing her back on a tree 100 meters away. Quite by chance I recorded the big Chile earthquake in 1960. My equipment saturated crest to crest for three hours! What man can do is negligible when compared to natural phenomena.

Back in civilian life I returned to statistical physics. I was still fascinated by the diagram formalism, hence a book written in 1962. Applications dealt with Bose liquids and electrons in metals. In 1967 Gerry Mahan described the edge singularities of X-ray absorption in metals (a core electron is excited near the Fermi level and interactions create further electron-hole pairs). A full theory must include a treatment of crossed channels, leading to the so-called parquet diagrams, a Russian specialty largely unknown in the West. The formalism was messy, but it explained the power laws of absorption spectra, with exponents fixed by final-state interactions. I remember my awkward attempt at a self-consistent calculation of these exponents. Years later I realized that it was relevant to critical behavior—too late! I was in La Jolla in the spring of 1968 (while my friends were attempting a revolution in Paris). I gave a seminar at Harvard on my parquet work. My French colleague Cirano De Dominicis was there and he kept saying, "I don't understand, it's a one-body problem, time dependent, but one body: Why all that mess?" I was proud of my achievement and I defended myself—but I was puzzled. On the plane back to San Diego I suddenly realized that Cirano was right. Within a week a simple calculation was completed and the paper (3) was written! That was a turning point in my physicist life. I knew that a theoretical formalism is only a tool that can reveal unexpected behavior. I suddenly realized that equations without a phenomenological background remain a formal game. Only simple qualitative arguments can unveil the underlying physics. A French cartoon series, "les Shadoks," had a ruling statement I liked, "Why make it simple when you can make it complicated." The reverse holds, of course.

I moved to the Laue-Langevin Institute in Grenoble in 1972. I stayed there when I was appointed as professor at College de France. I enjoyed that institution, which was atypical and therefore unknown to modern ratings. Professors give research courses, with one rule: Never repeat them! That forces constant renewal. The first years are heaven, until you exhaust what you know. Then you must broaden your scope. I enjoyed the freedom and I did it, charting new territory. As a theorist I never forgot my short experimental stretch; I am always interested by what can be measured and how it can be measured. A dialogue between experiment and theory is a difficult venture, which requires a lot of patience on both sides to find a common language. When it succeeds it is incredibly rewarding. I often made proposals to experimentalists, who always had the same initial reaction: "one more crazy theorist's idea!" But my experimentalist friends are smart and sometimes they accepted the challenge, with spectacular results. I am very grateful to them. A corollary of that view is that theorists should not live in ghettos, but be immersed in experimentalist communities. Hence my proposal to CNRS (Centre National de la Recherche Scientifique, the French NSF): Theory is a tool, like a voltmeter or an oscilloscope. If we retain a theory department we should have an oscilloscope department! Needless to say I just collected smiles.

I belong to a generation for whom condensed matter physics had no bound. I was equally challenged by semiconductors and magnetism, by liquid helium and surface physics (crystal growth and friction), by phase transitions and nonlinear phenomena. I am convinced that all these fields are cross-fertilizing each other: What you learn in one you can use in the other. As a result I dislike extreme specialization, especially when it goes together with fashions. All too often a novelty becomes a must. One of the first examples was the Kondo effect, for which many conflicting approaches were put forward. When this is the case, I hide: I want to think quietly and peacefully. The breakthrough came from Ken Wilson (4) and Phil Anderson (5). Then I put some flesh on it using Landau's theory (6). I realize that such a phenomenology is a fancy way for rationalizing my own weaknesses. Being unable to carry out the numerical work of Wilson, I try to do it with my hands using naïve methods. My conviction is that the two facets are complementary and both necessary; a numerical result is useful if you understand it. (Did I inherit this taste for concreteness from my peasant ancestry?)

Fashions have a corollary: When a domain is no longer on the front lines it is quickly forgotten. I remember in the early seventies the contempt for semiconductor physics, which was supposedly over. A few years later electron-hole droplets opened a new challenge, the quantum Hall effect changed our landscape, and layered semiconductors gave access to 2D physics. Semiconductors were reborn from their ashes and they are still alive and well. The same scenario occurred with superconductors: great excitement after the BCS breakthrough and the Josephson effect, which slowly decreased until the shock of high-Tc superconductors in

1986. In both cases problems were far from settled and many issues remained mysterious. Clearly we have just scratched the surface; there is a lot more behind what we know.

As a specific example, let me refer to the Mott transition of an electron-hole gas in a semiconductor. In 1949 Neville Mott predicted a transition from metal to insulator at zero temperature as a function of density N. At finite temperatures the transition becomes a crossover since an insulator always has thermally excited carriers. The same transition exists in a half-filled Hubbard band with hopping amplitude t and local repulsion U. If U is small, the ground state is a metal; if U is large, the carriers are localized, one per site. The issue is complicated by antiferromagnetic ordering of spins, but let us ignore that. The theory remained controversial until the advent of dynamical mean-field theory (DMFT), which clearly explained the mechanism (disappearance of a narrow resonance in the middle of a preformed gap). Alas DMFT also found that the transition was first order instead of second order. Hence a critical point at finite temperature—and a crucial question: What is the order parameter whose fluctuations diverge at the critical point? I immediately remembered a forgotten paper of Jim Wolfe and his collaborators (7) from the early eighties on the phase diagram of germanium. Electron-hole pairs are injected in a sample and gathered in a well-defined region (using a stress that acts like gravity in a pitcher): Carriers go to where the energy gap is minimal. Luminescence gives access to the phase diagram in the (N,T) plane. The results clearly display the liquid-gas critical point with the familiar parabolic maximum of N. Surprisingly, they also show a second critical point with an unusual cusp structure. A naive extension of Landau's theory can explain such a behavior if the order parameter is not N but another unknown quantity X. The density N is just a "spectator" that records what happens. There is no symmetry requirement with density, and all couplings are allowed. The first terms in an expansion immediately yield a cusp in the density discontinuity, of the order $(T–T_c)^{3/2}$—but once again what is X? As Pirandello would say, our phase transition is "in search of an order parameter." Twenty-five years later the issue is still there, largely forgotten. I have had it in the back of my mind ever since, with no hint at the answer.

Most of the joint work with experimentalists such as Bernard Castaing (8), and then for many years with Sebastien Balibar, was concerned with quantum liquids and solids. Superfluidity eliminates thermal dissipation, and a wealth of new effects appear. A simple example is the melting-crystallization wave at a liquid-solid interface. Usually crystal growth is never in thermal equilibrium. Equilibrium shapes predicted by Wulff more than a century ago are never observed, hidden as they are by growth kinetics profiles. In superfluid helium instead everything is simple (my friend Albert Libchaber used to say, "Everything you cannot do in usual materials you can do in ^{4}He"). Crystal growth becomes totally transparent: Simple experiments display facetting, giving access to the energy and interaction of crystal steps. A variety of instabilities appear under stress. An example is the oscillation of a vicinal surface with equidistant parallel steps (9). A wave vector perpendicular to the steps yields a compression wave that gives access to their interaction. A wave vector parallel to the steps yields a distortion wave that gives access to their energy. Frequency dispersion yields inertia, damping step friction. A simple experiment unveils a whole branch of materials physics.

More recently I have been concerned with superfluid and solid ^{4}He. A famous quotation of Feynman (10) states that "the roton is the ghost of a vanishing vortex ring." I was always puzzled by that statement and I desperately tried to put some flesh on it. A vortex ring goes faster and faster as it shrinks (Helmholtz!), and it should cease being a metastable entity when its velocity reaches the sound velocity. In the weak coupling regime the minimal size is large and the decay is certainly not in a single roton. In strong coupling I do not see how to ensure energy and momentum conservation. I now believe that the roton has nothing to do with superfluidity; it is the soft density fluctuation mode that will eventually lead to a spontaneous density wave, i.e., to a crystal. In a recent paper (11) I argued that superfluidity is not the cause of the roton, but its consequence. I know my views are heretical, but at nearly 80 I am not afraid of being burned! The physics of density waves with wave vector G was studied extensively by Levanyuk (12). In 1D, freezing of a soft mode creates a density pattern with Bragg wave vectors $\pm G$. Momentum space folds onto a Brillouin zone and a new gapless mode, the phason, appears at zone center, describing translation of the lattice modulation. This phason couples to the usual phonon, the Goldstone mode of superfluidity, which describes matter motion. If the phason and phonon are locked a real solid ensues, with a single soft sound mode. If they unlock superfluidity and a density wave coexist, hence a supersolid, a hot issue at the moment. Such a process is just a Mott transition of the Bose liquid. In 3D only a discrete star of Bragg vectors freezes, building the crystal lattice. There are several phason modes, three of them gapless, corresponding to compression and shear of the lattice. The locked phase is a usual shear-resistant solid. If it exists the unlocked phase is a superfluid liquid crystal. All of this is very speculative; I mention it only to show that old physics is alive and well.

My own trajectory is of limited interest. I would like now to comment on the evolution of our field, first as regards its content and perspective, which are exciting, then its sociology, which I find worrisome. Condensed matter physics in the fifties was blooming. Nearly every issue of *Physical Review* had an important breakthrough. Leafing through the journal, the reader could sense the incredible variety of problems, from semiconductors to metallurgy, from crystallography to magnetism. Curiosity precluded extreme specialization. Sure, fashions existed: The rush on superconductivity after the breakthrough of Bardeen, Cooper, and Schrieffer was spectacular! But other fields in condensed matter physics remained as lively as ever: Magnetism and semiconductors kept blooming. Even more important, condensed matter physics broadened its scope toward material science (a natural partner) and toward chemistry. I am convinced that the latter link should grow stronger: The views of chemists and physicists are complementary and we have a lot to teach each other.

Looking back at 60 years of condensed matter physics I am impressed by its permanent renewal: It regularly produces major conceptual breakthroughs (disorder and localization, quantum effects, etc.). As an old-timer I could be tempted by "the good old days." Certainly they were exciting and I thoroughly enjoyed them. But I am convinced that the present is equally fascinating. Let me take a single example, the so-called multiferroics in which two different orders coexist. Do they compete? Do they cooperate? The problem is incredibly rich: I just heard a wonderful seminar in Grenoble, which showed how the standard Landau theory is enriched when a second-order parameter is included. I was impressed by the conceptual originality without any formal complexity. I always knew that cross experiments involving the joint response to two controlled probes A and B were far more efficient than a mere addition. [My taste for synchronous detection is a remnant of my experimental days: I even wrote a note (13) on spectroscopy without a spectroscope!] The same is true for multiferroics: Coupled symmetry breakings are much richer than their plain superposition. A variety of fields open, such as magnetic superconductors, supersolids, etc. Ferroelectricity may be married to any other symmetry breaking, with a wealth of possible applications.

Condensed matter physics is no longer the one of yesterday, but it is as young as ever. Again and again I hear that the field is dying, with only complicated problems that discourage the enthusiasm of young people. I totally disagree with that view; we are faced with a wealth of exciting new issues, often simple, always challenging. With curiosity and enthusiasm the harvest may be exciting. But it means accepting risks. Focusing on fashionable subjects with fashionable techniques is incompatible with the adventures of research.

My optimism for condensed matter physics is supported by the variety of tools available, which were unthinkable only 10 years ago. Intelligent use of more and more performant computers is a revolution in theory. I do not mean fitting a theory to an experimental curve. Even if it is done honestly, with few adjustable parameters and reproducible experiments, it is not real proof. I often repeat to my young colleagues, "A crazy theory agreeing with experiment remains a crazy theory." The theory must have a respectable internal consistency! I mentioned DMFT (14), which extends mean-field approximations to time-dependent phenomena: A self-consistent treatment of time and inelasticity is a revolution, first done on a single site, now on clusters. Beyond the Mott transition, it has changed our understanding of many problems and it opens the way to a new understanding of new physical issues. Speculations that could never be checked are now within reach. I am unable to use computers, but I respect them as an exploratory tool. In the end a qualitative understanding remains an absolute rule for me, but I learn a lot from numerical work: I know what to look for!

On the experimental side the most impressive breakthrough is the advent of performant nanotechnologies. Nowadays one can manipulate single atoms and measure their electric and optical properties. The old plague of uncontrolled polycrystalline samples is no longer a fate! Fitting these well-controlled local properties to theoretical models becomes possible. But it should be remembered that progress in nanophysics depends on more traditional aspects of materials sciences. There would be no nanophysics without a perfect mastery of crystal growth and surface physics. The bosses who govern us should realize that putting all efforts on a few fashionable topics, strangling more traditional work that provides their technical background, is tantamount to killing the hen that lays golden eggs.

All my comments deal only with hard matter (which abusively includes liquid helium). Since 1960 a new world appeared, that of soft matter, liquid crystals, polymers, etc. I admire it, but I know very little about it—hence my silence. There also new concepts emerged, and the field has now reached full maturity. I hope that future issues of the *Annual Review of Condensed Matter Physics* will be devoted to that new world. I cannot do it!

The scientific content of condensed matter physics is as exciting as ever, but how about its sociological background? On that issue I worry. In the old days scientific research was governed by respected scientists who knew what research was: a difficult adventure that required freedom. If not competent themselves they knew where to find a motivated judgment. Nowadays we are governed by bureaucrats, for whom everything should be measured—hence the success of bibliometry, which provides figures. The h-index is a blessing, curing scientific incompetence (you do not need to read papers) and cowardice (it's not my fault, it's the h-index!). Such a race for citations obviously encourages conformism. To be recognized you must be quoted; to be quoted you must belong to a large community. Hence, the rush on any novelty. Far-reaching original ideas are doomed to death. I am fighting a lost battle against bibliometry, which is changing the nature of our job as physicists. My career is behind me and I can take as an example one of my own papers (15), written with my Paris colleagues in 1971 on the formulation of inelastic tunneling (the so-called Kjeldysh approach). Quoted for a few years by those who defended themselves against our critiques (which is perfectly respectable), it nearly disappeared for 20 years, until 1990 when it became an essential tool for developing nanophysics. In a completely different field, a paper (16) on reversals of the Earth's magnetic field has been totally ignored for 30 years. The recent successful construction (a tour de force) of a dynamo in France shows that orders of magnitude were there. Bibliometry has devastating effects: Thinking ahead is forbidden.

That race for measurements extends to institutions (the Shanghai rating). Putting on a single scale institutions with different histories and backgrounds is nonsense. Teaching and research slowly lose their soul. The emotion carried by Mona Lisa is not measured by the number of visitors! I admit that I have strong feelings, but condensed matter physics has been my life and I am sad to see it endangered. Sure, life is more difficult and fighting for recognition and careers is tough. But the scientific challenge is more exciting than ever, with one absolute condition: Live it as an adventure, not as a civil servant routine.

I often ask myself, "What would I choose if I were 20?" Astrophysics is tempting as it retains a part of dream: "Where do we come from?" But it is purely cerebral: You can observe but you cannot change anything. Condensed matter physics is more concrete and you can blend observation, understanding, and action. My answer is thus clear: I would enroll again! Ten years after retirement I am in my old lab everyday, surrounded by a flock of incredibly bright postdocs. They can achieve things I would never dream of, but I can help put them in perspective. The exchange with these bright young theorists is wonderful for both of us. I hope that a majority of our community will be as they are: curious, enthusiastic, and eager to understand the qualitative background of their work. Then I am confident that condensed matter physics will remain as exciting as ever.

DISCLOSURE STATEMENT

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Errata

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