The 1954 Tenth Solvay Congress on “Electrons in Metals”: Some Professional and Personal Reflections

David Pines, Research Professor of Physics, UIUC, Distinguished Research Professor of Physics, U C Davis, Co-Founder in Residence, Santa Fe institute, and ICAM Founding Director Emeritus

Introduction 2014 marks the 60th anniversary of the first major international meeting at which the topic of electron interaction in metals was explored in depth, the 1954 Solvay Congress on “Electrons in Metals”. At 30, I was its youngest participant. The memoir below celebrates the Solvay anniversary and the participants and organizers of *SCES at 60* [http://conferences.physics.illinois.edu/SCESat60/].

The Tenth Solvay Congress The Congress was the tenth in a remarkable series of international meetings that began in 1911 under the auspices of the Solvay International Institute of Physics, an organization founded and supported by Ernest Solvay, whose fortune derived from his invention of the Solvay process, an ammonia-soda process for the manufacturing of soda ash from brine and limestone. It was organized by the Scientific Council of the Institute, whose President was Sir Lawrence Bragg, with Robert Oppenheimer, Wolfgang Pauli, Neville Mott, Christian Moeller, and Francis Perrin as its members, and Brian Pippard as its scientific secretary.

I was more than a little excited when the invitation to present a report at the Congress arrived for me in Urbana, where I was a Research Assistant Professor at the University of Illinois, working as a postdoc with John Bardeen, and occupying a desk in the corner of John’s office. It was my first invitation to a meeting abroad, and I sent my acceptance to Sir Lawrence as soon as I could learn from Fred Seitz what was the proper form to use in the salutation.

I was invited to report on the collective description of electron interaction in metals that had been the basis of my 1950 Princeton Ph.D Thesis,”The Role of Plasma Oscillations in Electron Interactions” and was further developed in the next few years in a series of papers written with my thesis advisor, David Bohm [1]. Our collaboration on the third paper in that series had to be entirely by mail, as David had been fired by Princeton University in 1950, and forced into scientific exile in Brazil in 1952 by US governmental authorities, for his refusal to testify before Congress concerning his possible Communist party membership.

As the date for the Solvay Congress approached, I learned that mine would be the first report of the meeting, a still more daunting prospect for a young emerging theorist. I therefore spent several months writing my report, taking time out that spring and early summer from my ongoing collaboration with Bardeen on a collective description of the combined consequences of electron-electron and electron-phonon interactions in metals.

My wife, Suzy, and I headed for Europe in mid- August on a small ship of the Holland-American line, the S.S. Ryndam that went from Hoboken, NJ, to France. After some three weeks of travel in France and Switzerland in a small Renault, we arrived in Brussels, where the meeting took place from Sept.13-17 on the campus of the Free University of Brussels. In addition to the six members of the scientific committee [Oppenheimer was unable to attend because his infamous security hearing was scheduled around that time] the participants included the 7 rapporteurs [Keith MacDonald, Brian Pippard, Jacques Friedel, Charles Shull, Louis Neel, Herbert Frohlich, and myself] and 13 invited participants, some of whom gave short reports [John Van Vleck, Lars Onsager, Bernd Matthias,and Charles Kittel from the United States, Kurt Mendelssohn and Harry Jones, Great Britain, Pierre Aigrain, France, C.J. Gorter and Jan Smit, Netherlands, Alfred Seeger and Walter Meissner, Germany, Per-Olav Lowdin, Sweden, and Fausto Fumi, Italy. All but Aigrain are shown in the group photo below [Fig.1] in which the rapporteurs and the members of the Scientific Council are posed in the first row.

Fig.1. Seated left to right: K. A. Mendelssohn, H. Frohlich, D. Pines, C. Moller, W. Pauli, Sir W. L. Bragg, N. F. Mott, L. Neel, W. Meissner, D. K. C. MacDonald, C. [G]. Shull, J. Friedel. Standing, left to right: C. J. Gorter, C. Kittel, B. Matthias, I. Prigogine (in front), L. Onsager, A. [B]. Pippard (in front), J. Smit, F. Fumi (in front), H. Jones, J. H. vanVleck (in front), P. O. Lowdin, A. Seeger, P. Kipfer, O. Goche (in front), [G]. Balasse, J. Geheniau (in front).

My Report Because the approach Bohm and I had developed was new, with many of its applications still under development, my report was as much a progress report as a summary of published materials. In the interest of full disclosure, let me add that in giving it, I focused on only one member of the audience, Wolfgang Pauli, who was the conscience [and terror] of theoretical physics. I had been told that if Pauli liked something, he would shake his head from side to side [a movement in most people that indicates "no"], so I anxiously watched him as I gave my lecture. To my enormous relief, he was soon indicating he liked it…

Here are some highlights of that somewhat lengthy report on the collective description of electron interaction in metals:

*A discussion of the importance of following collective behavior, first by writing the Coulomb interaction between electrons as a coupling between density fluctuations, and then replacing the low momentum transfer part of that interaction by coordinates describing the plasma oscillations to which the Coulomb interaction gives rise

*A description of a " first principles" calculation of the resulting correlation energy of an electron gas that agreed well with experiments on the cohesive energy of the alkali metals

*A description of the emergence, in the collective description, of quasi-electrons [electrons plus a co-moving screening cloud], whose short range screened Coulomb interaction enabled one to justify and improve upon the widespread use of the independent particle model in calculating the behavior of conduction electrons in metals and explained their specific heat, x-ray band-width, etc.

*A description [2] of the significant corrections to the conduction electron paramagnetic susceptibility that come from electron interaction that turned out to explain the first direct measurements of that quantity that had just been carried out at Illinois by my colleague and close friend, Charlie Slichter.

*An extensive discussion of the physical origin of the characteristic peaks observed in the energy loss of kilovolt electrons passing through or being reflected by thin films. Bohm and I had learned from Conyers Herring a few years earlier that in Be and Al these peaks were at energies very close to those of the quantized plasma oscillations we had proposed, if one assumed that the valence electrons in these materials were free. Prior to the Solvay meeting the extent to which interband transitions, core electrons and surface effects changed the valence or core electron plasma frequency from its free electron value was not clear. Thanks to Neville Mott [see below] it became clear during the meeting that such changes might occur only infrequently.

*A progress report on the work with John Bardeen on extending the collective description of electron interaction to include electron-phonon interactions that explained the longitudinal sound velocity of simple metals and led to the net effective attractive interaction between electrons near the Fermi surface discussed below. ,

The Discussion One of the unique features of a Solvay Congress is that there is ample time for discussion after each report and that in the printed proceedings [3], the reports are followed by a transcript of much of the discussion that followed. In my case the remarks in the discussion were not only supportive of the work I had described but also included a number of significant further contributions to the topic. Three worthy of special mention are: Neville Mott's beautifully simple explanation [based on the f-sum rule] of why the measured characteristic energy loss in insulators and some metals is so often close to the plasmon energy calculated assuming the core [or valence] electrons are free, and the connection he [and later Frohlich] made between the plasma frequency and the frequency at which the dielectric function vanishes; Harry Jones' discussion of the x-ray band width of polyvalent metals, and John Van Vleck's suggestion that many of the results I described for the ground state energy might be obtained with a variational wave function of the form,

$$\psi = [\prod_{k<k_c} \exp -a_k \rho_k^* \rho_k] \, \psi_0$$

a suggestion later made independently by Feynman in a letter to me.

Toward a microscopic theory of superconductivity One topic I discussed that did not make it into the proceedings was the effective interaction between electrons that Bardeen and I obtained when both electron-electron and electron-phonon interactions are taken into account using the Bohm-Pines collective description. Our result was important because Frohlich had proposed that a phonon-induced attractive interaction between electrons could give rise to superconductivity, but his proposal had received little attention because he had not included the expected much larger role of the screened Coulomb interaction between electrons. In the weeks between the submission of my report and my departure for Europe, Bardeen and I had found that when Coulomb interactions are treated in self-consistent fashion, for quasi-electrons lying close to the Fermi surface, at finite frequencies their screened phonon-induced interaction wins out over their screened Coulomb repulsion, so one gets a net attractive interaction of the general form proposed by Frohlich. We concluded in our subsequent paper [4] that our interaction should provide a good starting point for the development of a microscopic theory of superconductivity.

I discussed this result in my talk, pointing out that our work demonstrated that Heisenberg’s attempts to get superconductivity solely from electron-electron interactions was doomed to failure. When I finished, Pauli made the first remark: “I always told that fool Heisenberg that he was wrong”, a remark that understandably did not make it into print.

Other reports and talks A look at the Table of Contents for the Proceedings below shows that electron interactions were also discussed by Per-Olav Lowdin, as he outlined his efforts to go beyond the Hartree-Fock approximation. Emergent ordered electronic states were considered by Louis Neel [antiferromagnetism] and by Herbert Frohlich and Bernd Matthias [superconductivity]. In his report, Frohlich summarized his work on a “toy” model in which superconductivity is induced in a one-dimensional system by the electron-phonon interaction, while Matthias gave a brief talk summarized his some of his famous rules for finding superconductors. New experimental probes of matter were discussed by Charles Kittel [cyclotron resonance, nuclear spin resonance, and electron spin resonance] and Clifford Shull [neutron scattering experiments on transition elements and their alloys], while Brian Pippard reviewed the progress being made on two new experimental ways to determine the Fermi surface in metals: the anomalous skin effect and the de Haas van Alphen effect. The results of two “classic” approaches to understanding the behavior of pure metals at low temperatures were summarized by Keith MacDonald [resistivity] and Kurt Mendelssohn [thermal conductivity]. Finally, there is an elegant report on his work on alloys by Jacques Friedel, and short papers by Fausto Fumi [on vacancies in metals], Jan Smit [on the Hall effect in ferromagnetic materials], and Alfred Seeger [on his electron theory of transition metals].

TABLE DES MATIÈRES

The published discussions after some of these reports contain some further gems: following the report by Pippard, Onsager reflects on his theory of the de Haas-van Alphen effect [pp.153-4] and takes part in an interchange with Pauli on magneto-resistance in metals [pp155-56]; following the report by Frohlich, Van Vleck discusses the importance and difficulty of calculating a key property of a superconductor, its “perfect diamagnetism”, and Onsager discusses flux quantization in superconductors [pp 310-312].

Personal Impact The Solvay Congress played a significant formative role in my subsequent scientific life in a number of ways. The reaction to my report gave me greatly increased confidence in my abilities and prospects as a theoretical physicist. The great attention paid to experiment by all the distinguished theorists present underlined for me the importance of so doing. The intimacy of the meeting encouraged personal contacts among the participants and their wives; the invitations Suzy and I received there led to our first visits to a number of world centers of physics research and the start of many life-long friendships.

We had allowed for a month of further exploration of Europe before the return voyage to New York, but had made almost no specific reservations for where we might go, other than brief stays in Rome and Paris. This was fortunate, as we were then able to accept invitations to visit from so many of the new friends we had made during the meeting--Kurt and Jutta Mendelssohn in Oxford, Neville and Ruth Mott and Brian Pippard in Cambridge, Harry Jones and his family in London, Fausto and Lina Fumi in Milan--and to accept an invitation from Christian Moeller to visit the Institute for Physics in Copenhagen and meet Niels and Aage Bohr and their families. Back in Paris for a second time, our host was Pierre Aigrain at the Ecole Normale Superieure while I visited Jacques Friedel in his office at the Ecole des Mines and we spent time as well with Jacques and his wife, Mary.

Lunches A sumptuous lunch accompanied by excellent white and red wines was served every day in what was normally a Chemistry Lab adjoining our meeting hall. Nearly all of us thoroughly enjoyed this break in the meeting, but Neville Mott was the exception. He complained to several of us that serving so much good food and wine in the middle of the day was counterproductive, as it made it far more difficult to concentrate on science in the afternoon sessions.

Dinners We were on our own for dinner. The first dinner Suzy and I had in Brussels on the Sunday night before the meeting was memorable. Since I was giving the first talk, we headed out from our hotel in the center of the city around 6pm in search of a restaurant at which we could have an early light meal. Within a few block's walk we decided to try an old-fashioned bistro that looked appealing but whose name I do not recall [and which had vanished due to highway construction when we searched for it three years later]. After we were seated at our table, all thoughts of a light meal vanished when we discovered on the wine list that we could buy a 1945 Lafite-Rothschild for $6. I knew almost nothing about wine, but that seemed a major opportunity, bargain so we embarked on a three-course meal to accompany it. In retrospect it was clearly the ideal preparation for my talk the next day.

We went back with Charlie Kittel who could not believe the Lafite was available that price until he had seen and sampled it for himself.

Our other memorable evening meal was at a restaurant on the Grand Place with Wolfgang Pauli and Berndt Matthias, who had come to know Pauli well during Bernd's time as a student at the ETH in Zurich. It was our first chance to be with Pauli; he was a most enjoyable dinner companion, so much less formidable over dinner than in the audience at a physics lecture.

Banquet When Suzy and I arrived at the Congress banquet, we discovered, to our dismay, that the seating plan required that we be separated. As a rapporteur, I was assigned a seat at the "high" table, a long narrow rectangular affair at which we sat side-by-side overlooking the other guests. Suzy was left to fend on on her own. Fortunately, the Onsagers recognized our situation and invited Suzy to sit with them. Lars had a formidable reputation for being not communicative, but he could not have been a better and more charming host for Suzy that evening.

Hotel and travel arrangements Suzy and I were put up at the best hotel in Brussels, the newly refurbished Metropole; it had sumptuous rooms and linens plus maids who changed the towels twice a day and made up our bed every night, a rare luxury for us, and indeed, for almost anyone in 1954 Europe. We soon discovered that not every participant was staying there; rather it was mainly just the younger people—us, Jacques Friedel, and Fausto Fumi. Our very distinguished senior colleagues, the Motts, Onsagers, and VanVlecks, were put up at a far more modest hotel across the road. Go figure..

Communication about travel arrangements was opaque. Before leaving we had received no information about travel reimbursement, so had booked the least expensive cabin on our small trans-Atlantic vessel, and made every effort to see Europe on $5/day as we traveled in the weeks preceding the Congress. After our arrival in Brussels, I went to the secretariat to inquire about possible reimbursement for our travel. As I started to tell them how we had traveled, I was asked why we had not chosen to fly first class across the Atlantic; the Solvay Congress was ready to cover that fare and had expected us to travel that way….

On being 30 At 30 I was the youngest participant in the Tenth Solvay Congress, and I learned from Van Vleck that at the same age he had been the youngest participant at the Sixth Solvay Congress in 1930. Van told us the story of being introduced to the King in a receiving line before the banquet; the King said to him, "Aren't you rather young to be at such a distinguished gathering" to which Van began his reply, "Oh no…" and then stopped before giving the rest of his intended reply- that he was so fortunate to have been selected---having remembered the advice he had been given-- that one is never supposed to contradict a monarch.

What came next In the years immediately following the Solvay Congress, there were a number of further advances in our understanding of electron interactions In metals, and a detailed description of many of these may be found in a 1955 review I wrote for the "Seitzschrift" [5], and two lecture note volumes that appeared in the early 1960's [6], [7].To cite but a few:

*By the spring of 1955 it turned out that a great many {~50] of the measured characteristic energy losses in solids were due to quantized plasma oscillations at very nearly the free electron energy so I was able to make the argument that plasmons should join phonons and magnons as leading members of the family of elementary collective excitations in solids [8]. Soon afterwards, Robert Ritchie [9] called attention to the possibility of surface plasmons produced by the interaction between electrons in surface states, and in recent years, *plasmonics*, the study of devices based on controlling surface plasmons, has emerged as a major subfield of nanoscience [10].

*It became clear that the subsidiary conditions that had been of such concern to us could be ignored as these turned out to play no role in determining either the ground state energy or quasiparticle spectrum

*A perturbation-theoretic calculation of the ground state energy of an electron gas was made by Gell-Mann and Brueckner in a diagrammatic approach that was based on the key approximation that Bohm and I had made in our collective description, the random phase approximation, or RPA [11]; it was shown by them to be valid only at densities large compared to those found in metals.

*Independent calculations of the ground state and correlation energy at metallic densities that agreed well with experiment were carried out by Hubbard [12] and Nozieres and me [13]; in common with the earlier approaches, these were based on using the RPA to deal with only the long-range part of the Coulomb interaction, with perturbation theory applied to the short-range part.

*It also became clear that while there were many different ways to derive our results on screening and plasmons, the simplest way to do so was to introduce a frequency and wavevector dependent dielectric function as emphasized by Lindhard [14] and Hubbard [9].

*It was subsequently recognized that the long wavelength emergent behavior of electron liquids and gases, screening and collective oscillation, is “protected” in that whether one deals with a classical plasma or a quantum electron liquid, the exact screening length is s/ω_p, where s is the adiabatic sound velocity, and $\omega_p = [4\pi ne^2/m]^{1/2}$ is the exact frequency at long wavelengths of their plasma oscillations [15].

*Most importantly, two years later Leon Cooper found, using a simple model of the Bardeen/Pines/Frohlich attractive interaction, that it gave rise to a bound state near the Fermi surface [more correctly, an instability of a normal Fermi liquid against pair creation], and in early 1957 Bob Schrieffer came up with his proposed wave function for the ground state of a superconductor, one in which pairs of electrons condense into a single quantum state. Schrieffer’s wave function formed the basis of the microscopic theory of superconductivity published a few weeks later by Bardeen, Cooper, and Schrieffer, who had solved, at long last, what had been the major challenge in correlated electron systems, and indeed in all of theoretical physics, for some 46 years.

*On a more personal note, the visits Suzy and I made following the Solvay Congress laid the foundation for our extended stay in Europe in 1957-58, during which I developed long-lasting scientific collaborations while in residence at the Bohr Institute and the Ecole Normale, visited once more Cambridge, Oxford, and Imperial College, and lectured at summer schools in Varenna, Paris, and Les Houches. Moreover, thanks to our meetings in Brussels and Paris, Pierre Aigrain had encouraged one of his most promising young students at the Ecole Normale to go to Princeton to work with me. So my short stay on the faculty at Princeton University had begun really well, with Philippe Nozieres becoming my first graduate student there in the fall of 1955

60 years later While the collective description of electron interactions that David Bohm and I developed did not turn out to be the simplest way to obtain a quantitative description of screening and plasmons in metals, it had the advantage of focusing attention on the density fluctuations of an electron liquid and making it clear at the outset that the main consequence of the long range part of the Coulomb interaction was to bring about plasma oscillation, while offering a simple microscopic approach to calculating the properties of the resulting quasi-electrons. Looking back, since the coupling between electrons at metallic densities is strong, mine may have been the first successful microscopic calculation of the properties of a strongly correlated electron system, since the coupling constant for an electron liquid is r_s, the inter-electron spacing in units of the Bohr radius, and for Li and Na, whose conduction electron paramagnetic susceptibility I had calculated, it is 3.2 and 4.0 respectively.

I continue to be amazed at the ability displayed by the Congress organizers in choosing participants who would make a difference. Among the younger participants, Jacques Friedel and Pierre Aigrain became the founders of condensed matter theory and experiment in France while Fausto Fumi played a similar role in Italy. Brian Pippard went on to play a leading role in developing an understanding of coherent behavior in superconductors while leading a major experimental group in Cambridge; Keith MacDonald would have continued for decades as a leading Canadian experimental condensed matter physicist, but for a 1957 debilitating illness [ALS] that led to his death five years later. Charlie Kittel soon wrote what became the standard introduction to solid state physics for decades, and Bernd Matthias continued to lead the world in discovering new superconductors until his untimely death in 1980.

At the time of the Congress, two of the members of its Scientific Council [Bragg, Pauli] had already won the Nobel Prize; six of its participants [Mott, Neel, Onsager, Prigogine, Shull, and Van Vleck] went on to do so.

In retrospect, I wish the local organizers had done a better job in putting out the Proceedings of the Congress. Although these make for fascinating reading and could have been a significant educational tool for the condensed matter community for decades, they were never widely distributed and are marred by many unnecessary typos. I hope this Memoir will encourage its readers to search out a copy in their institutional library or find a way to obtain one on inter-library loan. Do so, and enjoy!

Footnotes

1. D. Bohm and D.Pines, A Collective Description of Electron Interactions: I. Magnetic Interactions, Phys. Rev. **82**, 625-634 (1951); D.Pines and D. Bohm, A Collective Description of Electron Interactions: II. Collective vs. Individual Particle Aspects of the

Interactions, Phys. Rev. **85**, 338-353 (1952); D. Bohm and D.Pines, A Collective Description of Electron Interactions: III. Coulomb Interactions in a Degenerate Electron Gas, Phys. Rev. **92**, 609-625 (1953); D.Pines, A Collective Description of Electron Interactions: IV. Electron Interaction in Metals, Phys. Rev. **92**, 626-636 (1953).

2. D.Pines, Paramagnetic Susceptibility of Conduction Electrons. Phys. Rev. **95**, 1090-1091 (1953).

3. *Electrons in Metals*, Proceedings of the Tenth Solvay Congress on Physics, R.Stoops, Ed., Institute International de Physique Solvay, Bruxelles, 1955

4. J.Bardeen and D.Pines, Electron-Phonon Interaction in Metals, Phys. Rev. **99**, 1140-1150 (1955).

5. D.Pines, Electron Interaction in Metals. *Solid State Physics*, eds. F. Seitz and D. Turnbull, Academic, N.Y., **1**, 3-51 (1955).

6. D.Pines, *The Many-Body Problem*. (W. A. Benjamin: N.Y) 456 pp. (1961) (Russian translation, State Publishing House, Moscow, 1963).

7. D.Pines, *Elementary Excitations in Solids*. (W. A. Benjamin: N. Y.) 312 pp. (1963) (Russian translation, State Publishing House, Moscow, 1965). Japanese translation (Syokabo Press, Tokyo, 1974).

8. D. Pines, Collective Energy Losses in Solids, Rev. Mod. Phys. **28**, 184-199 (1956).

9. R.H.Ritchie, Plasma Losses by Fast Electrons in Thin Films, Phys. Rev. **106**, 874 [1957].

10. H.A. Atwater, The Promise of Plasmonics, Scientific American **17**, 56-73 [2007].

11. M.Gell-Mann and K. Brueckner, Correlation of an Electron Gas at High Density, Phys. Rev.**196**, 364-68 [1957].

12. J. Hubbard, The Description of Collective Motions in Terms of Many-Body Perturbation Theory II.The Correlation Energy of a Free Electron Gas, Proc. Roy. Soc [London] **A243**, 336-352 [1957].

13. P. Nozieres and D. Pines,The Correlation Energy of a Free Electron Gas, Phys. Rev. **111**, 442-454 (1958).

14.J.Lindhard, On the Properties of a Gas of Charged Particles, Kgl. Danske Videnskab,Mat-fys Medd. **28**, 8 [1954]

15. D.Pines and P.Nozieres, *The Theory of Quantum Liquids, Vol. I Normal Fermi Liquids*. W. A. Benjamin: NY, **1**, 355 pp. (1966). (Russian Translation, Publishing House MIR, Moscow, 1968)