

## Chapter 1

# THOUGHTS ON LOCALIZATION

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The outlines of the history which led to the idea of localization are available in a number of places, including my Nobel lecture. It seems pointless to repeat those reminiscences; so instead I choose to set down here the answer to “what happened next?” which is also a source of some amusement and of some modern interest.

A second set of ideas about localization has come into my thinking recently and is, again, of some modern interest: a relation between the “transport channel” ideas which began with Landauer, and many-body theory.

I have several times described the series of experiments, mostly on phosphorus impurities doped into Si (Si-P), done by Feher in 1955–56 in the course of inventing his ENDOR method, where he studies the nuclear spins coupled via hyperfine interaction to a given electron spin, via the effect of a nuclear resonance RF signal on the EPR of the electron spin. My study of these led to what Mott called “the 1958 paper” but in fact there is tangible evidence that the crucial part of the work dated to 1956–7. I have referred to the published discussion by David Pines which immediately followed Mott’s famous paper in *Can. J. Phys.*, 1956, where he described the joint, inconclusive efforts of the little group of E. Abrahams, D. Pines and myself to understand Feher’s observations. I also actually broke into print, at least in the form of two abstracts of talks, during that crucial period, and I show here facsimiles of these two abstracts (Figs. 1.1 and 1.2). The first is for the talk I gave at the International Conference on Theoretical Physics, in Seattle, October 1956. That conference was dominated by the parity violation talks of Lee and Yang, and by the appearance of Bogoliubov leading a Russian delegation; only third came the magnificent talk by Feynman on superfluid He and his failure to solve superconductivity. (A memory — on

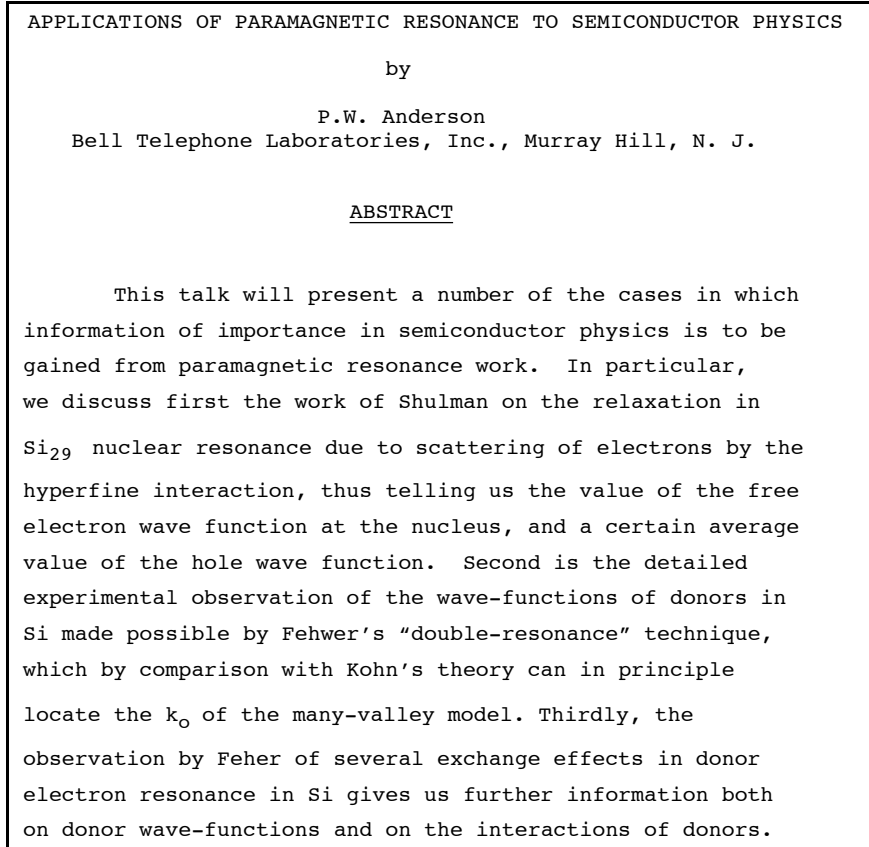


Fig. 1.1. Anderson abstract, Seattle, 1956.

a ferry crossing Puget Sound in thick fog with T. D. Lee explaining that we were being guided by phonon exchange between the foghorns!)

The second, which is a fortuitously preserved abstract of a talk I gave at the RCA laboratories in February 1957, makes it quite clear that by now the "fog" had cleared and I was willing to point out the absence of spin diffusion in these experiments. But actually, if my memory is no more fallible than usual, the first talk used the idea of localization without trumpeting that use, so it is essentially the first appearance of the localization scheme: a minor sideline to all the great things reported at Seattle.

But what I want to point out is how both George Feher and I missed the opportunity of a lifetime to jump five decades in time and invent modern quantum information theory in 1957. What my newly-fledged localization idea had left us with is the fact that suddenly the phosphorus spins had become true Qbits, independent sites containing a quantum entity with two

INTERACTIONS OF DONOR SPINS IN SILICON

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ABSTRACT

In this talk, I will describe some of the magnetic resonance experiments of Feher on donor atoms in Si, and discuss in particular those effects which depend on the interactions of the different donors.

Some of the topics which come under this general heading are: (1) the satellite lines, explained by Slichter as being caused by clusters, and what they tell us about the exchange integrals and the distribution of donors. (2) The non-existence of rapid relaxation because of motion within the "impurity band". This has important quantitative implications on transport in the impurity band. (3) Measurements involving "burning a hole in the line" show that "spin diffusion" is absent, and we discuss this theoretically. (4) Some more complicated minor effects involving exchange are discussed.

Fig. 1.2. Anderson abstract, RCA Laboratories, Princeton NJ, 1957.

states and an  $SU(2)$  state space which could, via the proper sequence of magnetic field variation, saturating monochromatic signals, and " $\pi$  pulses", be run through almost any unitary transformation we liked. Localization provided them the appropriate isolation, the reassurance that at least for several seconds or minutes there was no loss of quantum coherence until we came back to them with RF excitation. We could even hope to label the spins by tickling them with the appropriate RF signals. In other words, we had available the program which Steve Lyon is working on at Princeton right now. Of course, the very words like "Qbit" were far in the future, but we certainly saw those spins do some very strange things, some of which I tried to explain in my talk at Seattle.

But what did happen? George was in the course of getting divorced, remarried, and moving to UCSD at La Jolla and into the field of photosynthesis. While doing this he passed the whole problem — which we called

“passage effects” — on to his postdoc, Meir Weger. I, on the other hand, in the summer and fall of 1957 met the BCS theory and had an idea about it which eventually turned out to be the “Anderson–Higgs” mechanism, which was exciting and totally absorbing; I also did not find Meir as imaginative and as eager to hear my thoughts as George had been. So both of us shamelessly abandoned the subject — much, I suspect, to our eventual benefit. It really was too early and everything seemed — and was — too complicated for us.

Progress from 1958 to 1978 was slow; even my interest in localization was only kept alive for the first decade by Nevill Mott’s persistence and his encouragement of the experimental groups of Ted Davis and Helmut Fritzsche to keep after the original impurity band system, where they confirmed that localization was not merely a Mott phenomenon in that case. I think the valuable outcome for me was that I began to realize that Mottness and localization were not inimical but friendly: my worries in the 1957 days, that interactions would spoil everything, had been unnecessary.

By 1971, when Mott published his book on “Electronic Processes in Non-Crystalline Materials”, I had apparently begun to take an interest again. At least, in 1970 I had begun to answer<sup>1</sup> some of the many doubters of the correctness of my ideas. The masterly first chapter of that book, in which Mott makes the connection between localization and the Joffe–Regel idea of breakdown of transport theory when  $k\ell \sim 1$ , and between this limit and his “minimum metallic conductivity” (MMC) now gradually began to seep into my consciousness; but it was only much later that the third relevant connection, to Landauer’s discussion of the connection between conductivity and the transmission of a single lossless channel, became clear to me. I felt, I think correctly, that Landauer’s conclusion that 1D always localizes was a trivial corollary of my 1958 discussion; a great deal of further thought has to be put into the Landauer formalism before it is a theory of localization proper (see for instance, but not only, my paper of 1981<sup>2</sup>).

After finally understanding Mott’s ideas I became, for nearly a decade, a staunch advocate of Mott’s MMC. I remember with some embarrassment defending it strongly to Wegner just a few days before the Gang of Four (G-IV) paper broke into our consciousness. I liked quoting, in talks during that period, an antique 1914 article about Bi films by W. F. G. Swann, later to be Director of the Bartol Research Foundation, and E. O. Lawrence’s thesis professor, which confirmed the 2D version of MMC experimentally without having, of course, the faintest idea that the number he measured was  $e^2/h$ . But as often is the case, I was holding two incompatible points of view in my

head simultaneously, because I was very impressed by David Thouless's first tentative steps towards a scaling theory and, in fact, we hired at Princeton his collaborator in that work, Don Licciardello.

Again, the story, such as it was, of the genesis of the G-IV paper has been repeatedly narrated. More obscure are the origins of the understanding of conductivity fluctuations and of the relationships between Landauer's formula, localization and conductance quantization. Here, at least for me, Mark Az'bel played an important role. He was the first person to explain to me that a localized state could be part of a transmission channel which, at a sufficiently carefully chosen energy, would necessarily have transmission nearly unity. To my knowledge, he never published any way of deriving the universality and the divergent magnitude of conductivity fluctuations, but he had, or at least communicated to me, the crucial insight, quite early. (In the only paper on the subject I published, his contributions were referred to as "unpublished".) In any case, in the end I came to believe that the real nature of the localization phenomenon could be understood best, by me at least, by Landauer's formula

$$G = \frac{e^2}{h} \text{Tr}[tt^*], \quad (1.1)$$

where  $t_{\alpha\beta}$  is the transmissivity between incoming channel  $\alpha$  and outgoing channel  $\beta$ , on the energy shell. As I showed in that paper, the statistics of conductance fluctuations is innate in this formula; also conductance quantization in a single channel. (Though there are subtle corrections to the simple theory I gave there because of the statistics of eigenvalue spacing, as Muttalib and, using a different formalism, Altshuler showed.)

But what might be of modern interest is the "channel" concept which is so important in localization theory. The transport properties at low frequencies can be reduced to a sum over one-dimensional "channels". What this is reminiscent of is Haldane and Luther's tomographic bosonization of the Fermi system, where we see an analogy between the Fermi surface "patches" of Haldane and the channels of localization theory. Is it possible that a truly general bosonization of the Fermion system is possible in terms of density operators in a manifold of channels<sup>3</sup>?

One motivation for pursuing the consequences of such a bosonization is the failures — this is not too strong a word — of standard quantum computational methods to deal properly with the sign problem of strongly interacting Fermion systems. Quantum Monte Carlo and even very sophisticated generalizations of QMC fail completely to identify the Fermi surface

singularities which are inevitable in such systems.<sup>4</sup> Perhaps a “bosons in channels” reformulation is called for.

## References

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