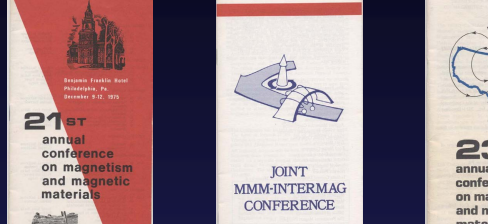
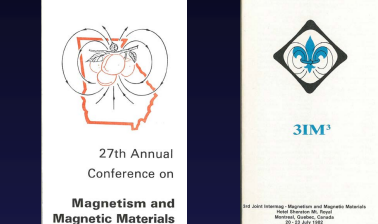
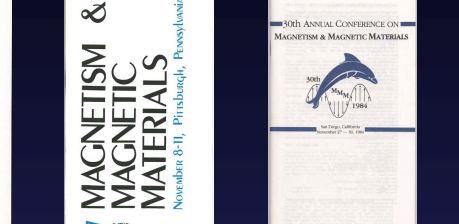
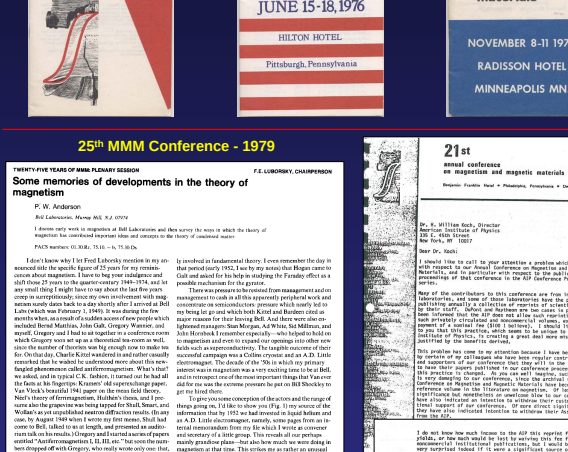
MMM – the third decade – to 1984







25th MMM Conference - 1979



TWENTY-FIVE YEARS OF MMM: PLENARY SESSION — F.E. LUBORSKY, CHAIRPERSON

Some memories of developments in the theory of magnetism

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I discuss early work in magnetism at Bell Laboratories and then survey the roots in which the theory of magnetism has contributed important ideas and concepts to the theory of condensed matter.

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I don't know why I let Fred Luborsky mention in my announced title the specific figure of 25 years for my reminiscences about magnetism. I have to be long year indulgence and shift those 25 years to the quarter-century 1949–1974, and let any small thing I might have to say about the last five years creep in surreptitiously; since my own involvement with magnetism surely dates back to a day shortly after I arrived at Bell Labs (which was February 1, 1949). It was during the few months when, as a result of a sudden access of new people which included Bernd Matthias, John Galt, Gregory Wannier, and myself, Gregory and I had to sit together in a conference room which Gregory soon set up as a theoretical ten-room as well, since the number of theories was big enough now to make ten for. On that day, Charlie Kittel wandered in and rather casually remarked that he wished he understood more about this newfangled phenomenon called antiferromagnetism. What's that? we asked, and in typical C.K. fashion, it turned out he had all the facts at his fingertips. Kramers' old superexchange paper, Van Vleck's beautiful 1941 paper on the mean field theory, Néel's theory of ferrimagnetism, Halpern's thesis, and I presume also the grapevine was being tapped for Shull, Smart, and Wollan's as yet unpublished neutron diffraction results. (In any case, by August 1949 when I wrote my first memo, Shull had come to Bell, talked to us at length, and presented an audiotorium talk on his results.) Gregory and I started a series of papers entitled "Antiferromagnetism I, II, III, etc." but soon the number jumped off with Gregory, who really wrote only one that, incidentally, is still being quoted in which he presented the rigorous theory of the antiferromagnetic Triangular Ising model, which is of interest now as the major rigorous result on what we now call a "fully frustrated" lattice.

So I have to confess that as a student of Van Vleck, I nonetheless entered the field of magnetism at Bell Labs under the direction of Charlie Kittel, having been wholly absorbed in my thesis work on pressure broadening at Harvard. (That is not quite true: I had listened at Harvard to my own on a fascinating series of lectures by Cor Gorter, brought there by Van Vleck, on the Dutch magnetic relaxation work, which was a very good way for a beginner to learn the ropes of magnetic resonance and relaxation.) It is remarkable to realize that during those few years of the early '50s during which Bell still had Kittel, Wannier, Bardeen, and Schockley as well as the rest of us, it was not only bringing the world into the transistor age but also making a major fraction of the basic discoveries in magnetism and other branches of solid state physics. Shockley, Bozorth, Williams, and Galt were demonstrating the basic mechanisms of domain wall motion and inventing the powder pattern method; Kittel, Yager, Holden, Gall, and later Dillon and Bond were making a considerable number of the most important magnetic resonance observations; and Herring, Kittel, and myself were deeply involved in fundamental theory. I even remember the day in that period (early 1952, I was by my notes) that Hagen came to Galt and asked for his help in studying the Faraday effect as a possible mechanism for the garnet.

There was pressure to be interested from management and on management to cash in all this apparently peripheral work and concentrate on semiconductors; pressure which surely led to my being let go and which both Kittel and Bardeen cited as major reasons for their leaving Bell. And there were also enlightened managers: Stan Morgan, Ad White, Sid Millman, and John Horsfall. I remember regular chats with Morgan about work in magnetism and even to expand our openings into other new fields such as superconductivity. The tragedy was that one of their successful campaign was a Celine cryostat and an A.D. Little electromagnet. The decade of the '50s in which my primary interest was in magnetism was a very exciting time to be at Bell, and in retrospect one of the most important things that Van Vleck did for me was the extreme pressure he put on Bill Shockley to get me hired there.

To give you some conception of the actions and the range of things going on, I'd like to show you (Fig. 1) my source of the information that by 1952 we had invested in liquid helium and an A.D. Little electromagnet, namely, some pages from an internal memorandum from my file which I write as convener and secretary of a little group. This reveals all our perhaps mainly grandiose plans—but also how much we were doing in magnetism at that time. This strikes me as rather an unusual and interesting historical document.

Now let me talk a little about what was going on for the theory of magnetism and for me at that time.

It has seemed to me that over the years magnetism has made extremely important contributions to the general theory of condensed matter. The reason is that magnetism very often gives us relatively simple and in some sense weakly interacting systems, which therefore are conceptually somewhat easier to deal with than the harder and more complex problems in other fields. Therefore developments in condensed matter and particularly in many-body theory often have appeared first in the theory of magnetism. Among the things that were going on at that time one which seemed to me very important was the theory presented an enormously important direction for the future of many-body physics was the work that was being done by Conyers Herring, partly in collaboration with Charles Kittel. This work explored the general question of spin waves in ferromagnetic materials. It consisted first of a general, somewhat phenomenological, paper by Herring and Kittel in which they spelled out the broad outlines of what really is a very fundamental and important point of view in many-body physics. This is the point of view that the metallic electron gas has certain medium-like responses, in this case a response to twisting the magnetization, and that these medium-like responses determine the excitation spectrum—in the case spin waves—of the system. Conyer's two papers which follow this are rather deep and thoughtful discussions and interpretations of how one would go about calculating the stiffness of the spin waves. We should recognize that these papers almost stand at the very beginning of the subject of modern many-body physics. Along with perhaps John Slater's earlier work on spin waves in insulators, which unfortunately John never generalized, and with some of the early work by the Russians, specifically Landau and Bogoliubov, these papers launch the whole world on the paradigm of how to deal with large systems in a rigorous and fundamental way by means of the Quantum Theory. This was the very next step after the fundamental work by Wigner and his students on the correlation energy problem, in the sense that one began to abandon the simple chemical question of calculating energies using quasi-heuristic orbitals, and instead tries to make precise statements about the excitation spectrum of the complicated system. It is interesting to realize that Herring and Conyers produce the famous work of Bohm and Pines where they brought out the nature of another fundamental excitation of a many-body system, namely the plasma oscillation. For some reason, perhaps the connection with the antiferromagnetic problem, the plasmon work has been more prominent historically, but I personally think they are equally important. Certainly this work, and particularly Conyer Herring's wise and profound insights into the nature of large systems, were a very large part of my own personal background in many-body physics.

While I am on this subject let me go on to follow it out in its consequences for future developments in magnetism. Shortly after this work was done I remember having from Cliff Shull during the course of a journey somewhere (unfortunately, where is now erased from my memory) the idea that the spin waves and other magnetic phenomena were beginning to be observed in magnetic metals above the ferromagnetic Curie point. It may have been in fact on our way to that famous 1952 conference in Maryland at which so many stories in magnetism began. I think I was already aware and said that there was no difficulty in understanding such phenomena in metals in principle theory of magnetism, but that one would have to modify the naive Stoner-Slater mean field theory. This was done first, as I recall, by the Herring and Kittel work, in order to accommodate such things. This is of course the point of view that we now hold. But I think it is true that scientists, like everyone else, relish a good shooting match, and it was to be 20 years before the last gun shot of the itinerant versus local magnetism arguments were to begin to die down. But it was not just controversy the latter controversy is that quite a number of useful and interesting ideas accompanied the attempt to demonstrate the inadequacy of the two ways of looking at magnetism were not incompatible. The most important of these perhaps was the idea of virtual states introduced by Friedel in the late 1950s. I picked this up and made a pretty picture out of it in my local account paper which, slightly to my embarrassment, remains by far my most widely cited paper. The reason it is slightly to my embarrassment is that I was simply providing a formal framework for the ideas of which Friedel was the predominant originator. Perhaps I can be given a little credit for adding to Friedel's ideas the general concept of the magnetic state which I had been developing over the years, primarily in order to understand the other branch of magnetism, namely antiferromagnetism and magnetism in insulators because magnetic systems have served as models as well as "test beds" for each of these advances.

II. ONE-ION MECHANISM OF ANISOTROPY: AN EVOLUTIONARY ADVANCE

A. Background

To appreciate the significance of the advance in magnetocrystalline anisotropy energy (hereinafter called anisotropy) which I propose, it is first necessary to see the state of affairs in the mid-1950s. Some of these advances are (a) the concept of crystalline field) mechanism of magnetocrystalline anisotropy energy which was synthesized from ingredients in semiconductor physics, and developed into a practical tool for "designing" ferrimagnets (Sec. II); (b) the recognition of the degeneracy of the spin spectrum in ferromagnetic insulators and its contribution to the establishment of the previously mysterious origin of relaxation in ferromagnetic resonance; and (c) the concept of magnetic symmetry and its role in "predicting" ferroelectric (and nonferroelectric) effect, piezomagnetism, and other phenomena involving combinations of time reversal. Some of these advances seem trivial in hindsight, but at the time they were a very important step in the development of the theory of magnetism. It is interesting, therefore, to quote the concluding sentiments of Yoshiro Tsuchiya's review of ferromagnetic theory presented by Van Vleck at the second ISMMT.

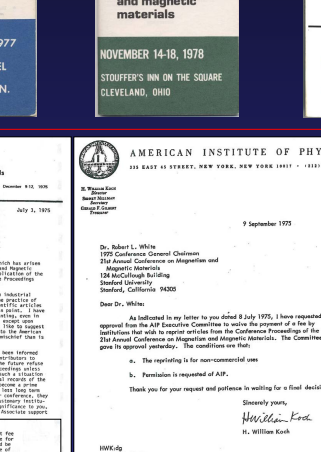
"The theory is unsatisfactory to the extent of showing that there is nothing mysterious in the existence of ferromagnetic theory, but at present it falls far short of the aspiration of a good theory of the anisotropy and magnetostriction constants."

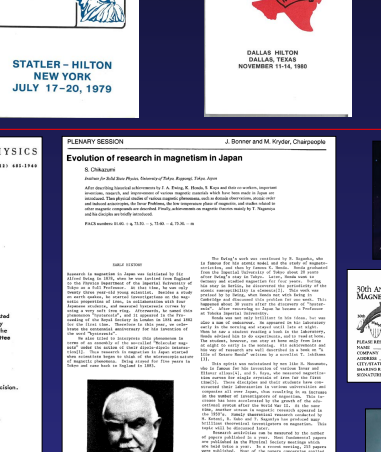
Because of this assessment, it is important to recall that Van Vleck's theory of anisotropy is concerned solely with 3d ferromagnetic metals (e.g., iron, nickel, and cobalt). In these materials even the theory of ferromagnetism itself has been so far difficulties which still persist. It is not surprising, therefore, that the prediction of Van Vleck's anisotropy theory are limited to rough orders of magnitude. Even at present, the status of anisotropy theory for 3d metals is not very much better, and we are still current in anisotropy research has shifted from 3d metals to rower materials.

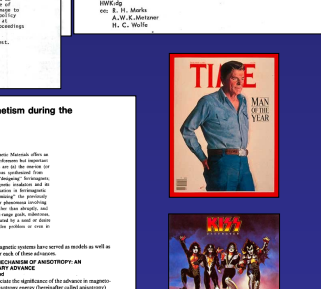
Because of the advent of the one-ion mechanism discussed below, the present status of anisotropy theory for ionic compounds, e.g., fluorides and oxides, is much more advanced than for the 3d metals. Before discussing this advance, which is clearly revolutionary, we recall that in ionic compounds the mechanisms of anisotropy may be divided into two broad categories, namely "two-ion" mechanisms and the "one-ion" mechanism. The former include not only the inter-ionic dipole interaction but also various kinds of anisotropic exchange, i.e., the combined effect of spin-orbit coupling and inter-ionic exchange interaction. Although the anisotropic exchange mechanisms are analogous (except for their antisymmetric counterpropositions to scientific advances even though this very interpretation is well documented in mythology by the birth of Athena, the goddess of wisdom, who was pre-programmed to emerge fully grown from the head of the chief of Olympian Management.

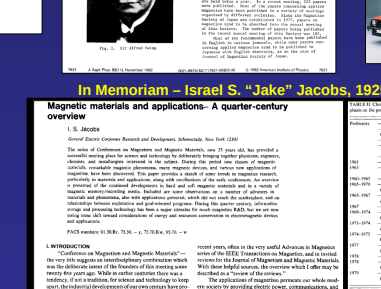
V. ACKNOWLEDGMENTS

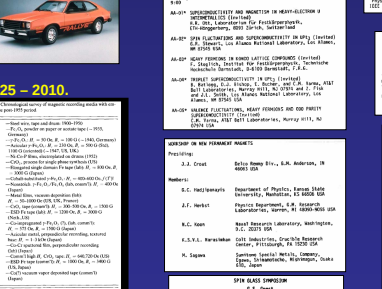
It is a pleasure to thank Y.J. Fokin, J.J. Krebs, and M. Rubinstein for discussions and encouragement.

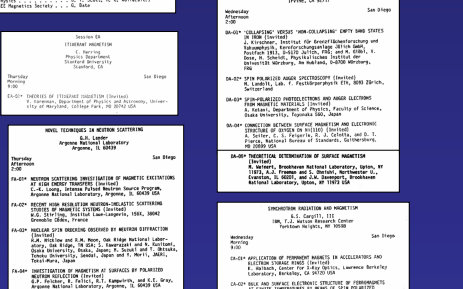












In Memoriam – Israel S. "Jake" Jacobs, 1925 – 2010.

Jeff Childress - 55th MMM Conference 2010