Physics 1976

BURTON RICHTER and SAMUEL C C TING

for their pioneering work in the discovery of a heavy elementary particle of a new kind

THE NOBEL PRIZE FOR PHYSICS

Speech by professor GÖSTA EKSPONG of the Royal Academy of Sciences
Translation from the Swedish text

Your Majesties, Your Royal Highnesses, Ladies and Gentlemen,
By decision of the Royal Swedish **Academy of Sciences, this year's Nobel** Prize for physics has been awarded to Professor Burton Richter and to Professor Samuel Ting for their pioneering work in the discovery of a heavy elementary particle of a new kind.

This discovery has opened new vistas and given rise to great activity in all laboratories around the world where resources are available. It brings with it the promise of a deeper understanding of all matter and of several of its fundamental forces.

Elementary particles are very small compared to our human dimensions. They are smaller than viruses and molecules and atoms, even smaller than the tiny nucleus of most atoms. They are of great importance when it comes to understanding the basic structure and the basic forces of the material world. In some cases they can even be of importance to society. A basic philosophy is that the material units on any level of subdivision derive their properties from the levels below.

Seventy years ago the first elementary particle was involved in a Nobel Prize. This was at a time when no valid picture of atoms had been formulated. In his Nobel lecture in 1906, J. J. Thompson spoke about his discovery of the electron as one of the bricks of which atoms are built up. Today we know that the electron plays a decisive role in many sciences and technologies and through them in many walks of life-it binds together the molecules of our own bodies, it carries the electricity which makes our lamps shine and it literally draws up the pictures on the TV-screens.

Forty years ago Carl David Andersson was awarded a Nobel Prize for the discovery of the positron-which is the antiparticle to the electron. In the presentation of the award in 1936, it was mentioned that twins of one electron and one positron could be born out of the energy coming from radiation. The reverse can also happen. If the two opposite types of particle meet they can disappear and the energy, which can never be destroyed, shows up as radiation. Only in recent years has this description been enriched through experiments at higher energies, where, among many researchers, both Richter and Ting have contributed.

It is with these two particles that the Nobel laureates Ting and Richter have again experimented in most successful ways. Ting discovered the new particle when he investigated how twins of one electron and one positron are born at very high energies. Richter arranged for electrons and positrons to meet in head-on collisions and the new particle appeared when conditions

were exactly right. Both have carried out their researches at laboratories with large particle accelerators and other heavy equipment, which take the **place** of microscopes when it comes to investigating the smallest structures of matter, Ting and his team of researchers from Massachusetts Institute of Technology set up their cleverly designed apparatus at the Brookhaven National Laboratory on Long Island. Richter and his teams from Stanford and Berkeley built their sophisticated instrumentation complex at the Stanford Linear Accelerator Center in California. In the two different laboratories and with very different methods both found almost simultaneously a clear signal that a new, heavy particle was involved-born in violent collisions and dying shortly afterwards. The letter J was chosen as name at Brookhaven, the greek letter ψ (psi) at Stanford.

The multitude of elementary particles can be beautifully grouped together in families with well-defined boundaries. Missing members have been found in many cases, in some cases they still remain to be found. All seem to derive their properties from a deeper level of subdivision where only a few building bricks, called quarks, are required.

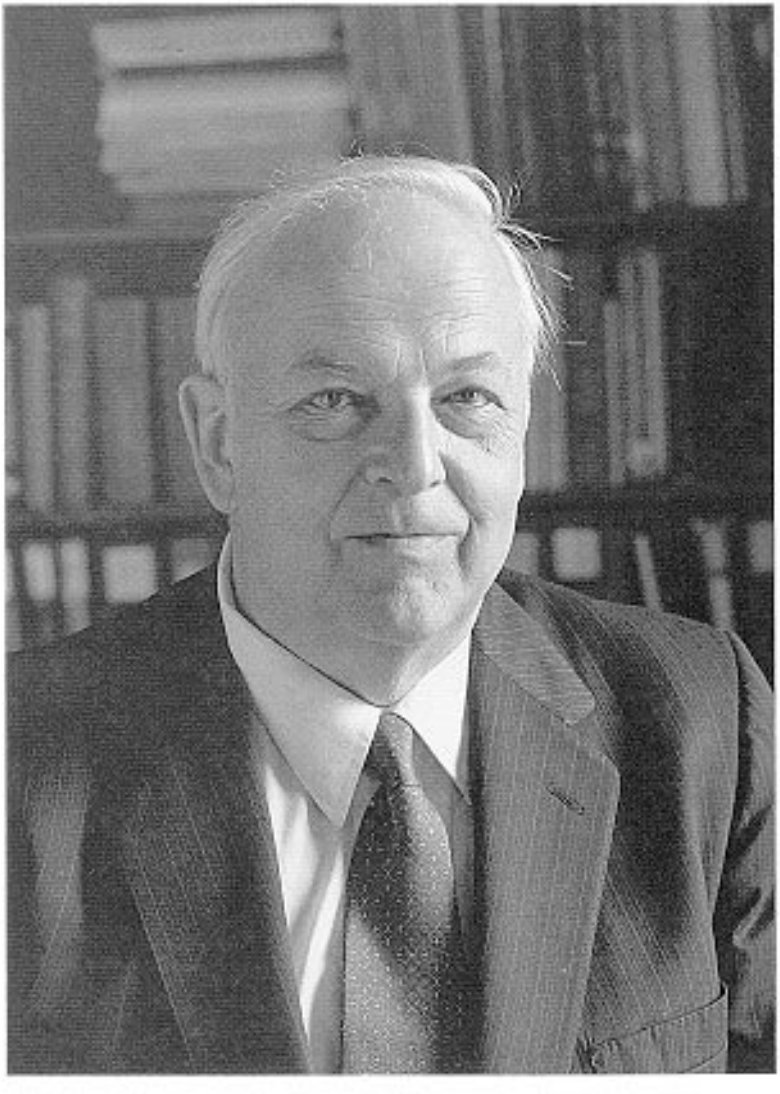
The unique thing about the J-ψ particle is that it does not belong to any of the families as they were known before 1974. Further particles have been discovered resembling the J-ψ one. The reappraisal of particle family structures now required has already begun in terms of a new dimension, corresponding to the new fourth quark already suggested in other contexts.

Most of the recently found particles of normal type can be described as hills of varying height and width in the energy landscape of the physicists, not too unlike pictures of the mounds, barrows and pyramids which the archeologists take an interest in. In the landscape of particles the new J-ψ surprised physicists by being more than twice as heavy as any comparable particle and yet a thousand times more narrow. One can perhaps better imagine the surprise of an explorer in the jungle if he suddenly were to discover a new pyramid, twice as heavy as the largest one in Tikal and yet a thousand times narrower and thus higher. After checking and rechecking that he is not the victim of an optical illusion he would certainly claim that such a remarkable mausoleum must entail the existence of a hidden culture.

Professor Richter, Professor Ting,

I have compared you to explorers of almost unknown territory in which you have discovered new startling structures. Like many great explorers you have had with you teams of skilful people. I would like you to convey to them our congratulations upon these admirable achievements. Your own unrelenting efforts in the field of electron-positron research over a large number of years and your visions have been of outstanding importance and have now culminated in the dramatic discovery of the J-ψ particle. You have greatly influenced and enriched your research field: the physics of elementary particles after November 1974 is recognized to be different from what it was before.

I have the pleasure and the honour on behalf of the Academy to extend to you our warmest congratulations and I now invite you to receive your prizes from the hands of His Majesty the King.



Burton Richter

BURTON RICHTER

I was born on 22 March 1931 in New York, the elder child of Abraham and Fanny Richter. In 1948 I entered the Massachusetts Institute of Technology, undecided between studies of chemistry and physics, but my first year convinced me that physics was more interesting to me. The most influential teacher in my undergraduate years was Professor Francis Friedman, who opened my eyes to the beauty of physics.

In the summer following my junior year, I began work with Professor Francis Bitter in MIT's magnet laboratory. During that summer I had my introduction to the electron-positron system, working part-time with Professor Martin Deutsch, who was conducting his classical positronium experiments using a large magnet in Bitter's laboratory. Under Bitter's direction, I completed my senior thesis on the quadratic Zeeman effect in hydrogen.

I entered graduate school at MIT in 1952, continuing to work with Bitter and his group. During my first year as a graduate student, we worked on a measurement of the isotope shift and hyperfine structure of mercury isotopes. My job was to make the relatively short-lived mercury-197 isotope by using the MIT cyclotron to bombard gold with a deuteron beam. By the end of the year I found myself more interested in the nuclear- and particle-physics problems to which I had been exposed and in the accelerator I had used, than in the main theme of the experiment. I arranged to spend six months at the Brookhaven National Laboratory's 3-GeV proton accelerator to see if particle physics was really what I wanted to do. It was, and I returned to the MIT synchrotron laboratory. This small machine was a magnificent training ground for students, for not only did we have to design and build the apparatus required for our experiments, but we also had to help maintain and operate the accelerator. My Ph.D. thesis was completed on the photoproduction of pi-mesons from hydrogen, under the direction of Dr. L. S. Osborne, in 1956.

During my years at the synchrotron laboratory, I had become interested in the theory of quantum electrodynamics and had decided that what I would most like to do after completing my dissertation work was to probe the short-distance behavior of the electromagnetic interaction. So I sought a job at Stanford's High-Energy Physics Laboratory where there was a 700 MeV electron linear accelerator. My first experiment there, the study of electron-positron pairs by gamma-rays, established that quantum electrodynamics was correct to distances as small as about 10^{-13} cm.

In 1960, I married Laurose Becker. We have two children, Elizabeth, born in 1961, and Matthew, born in 1963.

In 1957, G. K. O'Neill of Princeton had proposed building a colliding beam machine that would use the HEPL linac as an injector, and allow electron-electron scattering to be studied at a center-of-mass energy ten times larger than my pair experiment. I joined O'Neill and with W. C. Barber and B. Gittelman we began to build the first colliding beam device. It took us about six years to make the beams behave properly. This device was the ancestor of all of the colliding beam storage rings to follow. The technique has been so productive that all high-energy physics accelerators now being developed are colliding beam devices.

In 1965, after we had finally made a very complicated accelerator work and had built the needed experimental apparatus, the experiment was carried out, with the result that the validity of quantum electrodynamics was extended down to less than 10^{-14} cm.

Even before the ring at HEPL was operating, I had begun to think about a high-energy electron-positron colliding-beam machine and what one could do with it. In particular, I wanted to study the structure of the strongly interacting particles. I had moved to SLAC in 1963, and with the encouragement of W. K. H. Panofsky, the SLAC Director, I set up a group to make a final design of a high-energy electron-positron machine. We completed a preliminary design in 1964 and in 1965 submitted a request for funds to the Atomic Energy Commission. That was the beginning of a long struggle to obtain funding for the device, during which I made some excursions into other experiments. My group designed and built part of the large magnetic spectrometer complex at SLAC and used it to do a series of pi- and K-meson photoproduction experiments. Throughout this time, however, I kept pushing for the storage ring and kept the design group alive. Finally, in 1970, we received funds to begin building the storage ring (now called SPEAR) as well as a large magnetic detector that we had designed for the first set of experiments. In 1973 the experiments finally began, and the results were all that I had hoped for. The discovery for which I have been honored with the Nobel Prize and the experiments that elucidated exactly what that discovery implied are described in the accompanying lecture. Much more has been done with the SPEAR storage ring, but that is another story.

I spent the academic year, 1975-76, on sabbatical leave at CERN, Geneva. During that year I began an experiment on the ISR, the CERN 30 by 30 GeV proton storage rings, and worked out the general energy scaling laws for high-energy electron-positron colliding-beam storage rings. My motive for this last work was two-fold - to solve the general problems and to look specifically at the parameters of a collider in the 100-200 GeV c.m. energy range that would, I thought, be required to better understand the weak interaction and its relation to the electromagnetic interaction. That study turned into the first-order design of the 27 km circumference LEP project at CERN that was so brilliantly brought into being by the CERN staff in the 1980's.

An interesting sidelight to the LEP story is the attempt by Professor Guy von Dardel of Lund and Chairman of the European Committee for Future Accelerators and I to turn LEP into an inter-regional project. We failed because we couldn't interest either the American or European high-energy physics communities in a collaboration even on as large a scale as LEP. The time was not right, but it surely must be sooner or later.

The general scaling laws for storage rings showed that the size and cost of such machines increased as the square of the energy. LEP, though very large, was financially feasible, but a machine of ten times the energy of LEP would not be. I began to think about alternative approaches with more favorable scaling laws and soon focused on the idea of the linear collider where electron and positron beams from separate linear accelerators were fired at each other to produce the high-energy interactions. The key to achieving sufficient reaction rate to allow interesting physics studies at high energies was to make the beam extremely small at the interaction point, many orders of magnitude less in area than the colliding beams in the storage rings.

In 1978 I met A. N. Skrinsky of Novosibirsk and Maury Tigner of Cornell at a workshop we were attending on future possibilities for high energy machines. We discovered that we had all been thinking along the same general lines and at that workshop we derived, with the help of others present, the critical equations for the design of linear colliders. On returning from the workshop I got a group of people together at the Stanford Linear Accelerator Center and we began to investigate the possibility of turning the two-mile-long SLAC linac into a linear collider. It would be a hybrid kind of machine, with both electrons and positrons accelerated in the same linear accelerator, and with an array of magnets at the end to separate the two beams and then bring them back into head-on collisions. The beams had to have a radius of approximately two microns at the collision point to get enough events to be interesting as a physics research tool, roughly a factor of 1000 less in area than the colliding beams in a storage ring. Construction of SLAC Linear Collider began in 1983, and was finished in late 1987. The first physics experiments began in 1990. Probably the most lasting contribution that this facility makes to particle physics will be the work on accelerator physics and beam dynamics that has been done with the machine and which forms the basis of very active R&D programs aimed at TeV-scale linear colliders for the future. The R&D program is being pursued in the U.S., Europe, the Soviet Union and Japan. Perhaps this will be the inter-regional machine that von Dardel and I tried to make of LEP in the later 1970's.

Along the way I succumbed to temptation and became a scientific administrator first as Technical Director of the Stanford Linear Accelerator Center from 1982 to 1984, and then Director from 1984 to the present. The job of a laboratory director is much different from the job of a physicist, particularly in a time of tight budgets. It is much easier to do physics when

someone else gets the funds than it is to get the funds for others to do the research.

Writing this brief biography had made me realize what a long love affair I have had with the electron. Like most love affairs, it has had its ups and downs, but for me the joys have far outweighed the frustrations.

FROM THE PSI TO CHARM-THE EXPERIMENTS OF 1975 AND 1976

Nobel Lecture, December 11, 1976

by
BURTON RICHTER
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1. INTRODUCTION

Exactly 25 months ago the announcement of the ψ/J particle by Professor Ting's and my groups [1, 2] burst on the community of particle physicists. Nothing so strange and completely unexpected had happened in particle physics for many years. Ten days later my group found the second of the ψ's, [3] and the sense of excitement in the community intensified, The long awaited discovery of *anything* which would give a clue to the proper direction in which to move in understanding the elementary particles loosed a flood of theoretical papers that washed over the journals in the next year.

The experiments that I and my colleagues carried through in the two years after the discovery of the ψ have, I believe, selected from all the competing explanations the one that is probably correct. It is these experiments that I wish to describe. The rapid progress is a consequence of the power of the electron-positron colliding-beam technique, and so I also want to describe this technique and tell something of my involvement in it.

2. COLLIDING BEAMS

I completed my graduate studies at M.I.T. in 1956, and in the Fall of that year I took a position at the High-Energy Physics Laboratory (HEPL) at Stanford University. My main research interest at that time was in exploring the high momentum-transfer or short-distance behavior of quantum electrodynamics (QED). My original plan for a QED experiment had been *to use* the 700-MeV electron linac at HEPL in a study of electron-electron scattering. Within a short time, however, I came to realize that a different experiment would be both technically simpler to carry out and would also probe QED more deeply (though somewhat differently). During my first year at HEPL I did this latter experiment, which involved the photoproduction of electron-positron pairs in which one of the members of the pair emerged at a large angle. This experiment succeeded in establishing the validity of QED down to distances of about 10^{-13} cm.

2.1 The Stanford-Princeton Electron-Electron Storage Rings

In 1957 the idea of an electron-electron scattering experiment came alive again, although in a much different form. This happened when G. K. O'Neill of Princeton University informally proposed the construction at HEPL of a figure 8-shaped set of rings capable of storing counter-rotating beams of electrons at *energies* up to 500 meV for each beam. *In* this plan the HEPL

linac was to act as the injector for the rings, and the circulating electron beams would collide in the common straight section between the two rings. O'Neill's aim was not only to demonstrate the feasibility of colliding electron beams, but also to carry out electron-electron scattering at an energy that could significantly extend the range of validity of QED.

The potential of such an e-e-, colliding-beam experiment, with its total center-of-mass energy of 1000 MeV, was much greater than the ~ 50 MeV that would have been available to test QED in my original e-e- scattering idea. Thus when O'Neill asked me to join in this work, I accepted enthusiastically and became an accelerator builder as well as an experimenter. With two other collaborators, W. C. Barber and B. Gittelman, we set out in 1958 to build the first large storage ring, and we hoped to have our first experimental results in perhaps three years. These results were not in fact forthcoming until seven years later, for there was much to learn about the behavior of beams in storage rings; but what we learned during that long and often frustrating time opened up a new field of particle physics research. [4]

2.2 A Moment of Realization

Let me digress here for a moment to recount a formative experience. In 1959, as the work on the HEPL rings progressed, I was also trying to learn something about how to calculate cross sections in QED under the tutelage of Stanford theorist J. D. Bjorken. One of the problems Bjorken gave me was to calculate the cross section for the production of a pair of point-like particles having zero spin (bosons) in electron-positron annihilation. I carried out this calculation, but I was troubled by the fact that no point-like bosons were known to exist. The only spin-zero bosons I knew about were pions, and the strong interactions to which these particles were subject gave them a finite size. I realized that the structure function of the particle would have to enter into the cross section to account for this finite size. The structure function for the pion could be measured in an experiment in which e^+e^- annihilation resulted in the production of pion pairs. Further, the structures of any of the family of strongly interacting particles (hadrons) could be determined by measuring their production cross sections in e^+e^- annihilation. It's certain that many people had realized all this before, but it came as a revelation to me at that time, and it headed me firmly on the course that eventually led to this platform.

2.3 The Electron-Positron Annihilation Process

This connection between e^+e^- annihilation and hadrons is worth a brief elaboration here, since it is central to the experimental results I shall describe later. The method by which new particles are created in electron-positron collisions is a particularly simple one that I have always naively pictured in the following way. The unique annihilation process can occur only in the collision between a particle and its antiparticle. The process proceeds in two steps:

1. The particle and antiparticle coalesce, and all the attributes that give them their identities cancel. For a brief instant there is created a tiny

electromagnetic fireball of enormous energy density and precisely defined quantum numbers: $J^{PC} = 1^{--}$; all others cancel out to zero.

2. The energy within the fireball then rematerializes into *any* combination of newly created particles that satisfies two criteria: (a) the total mass of the created particles is less than or equal to the total energy of the fireball; (b) the overall quantum numbers of the created particles are the same as those of the fireball. There is no restriction on the individual particles that comprise the final state, only on their sum.

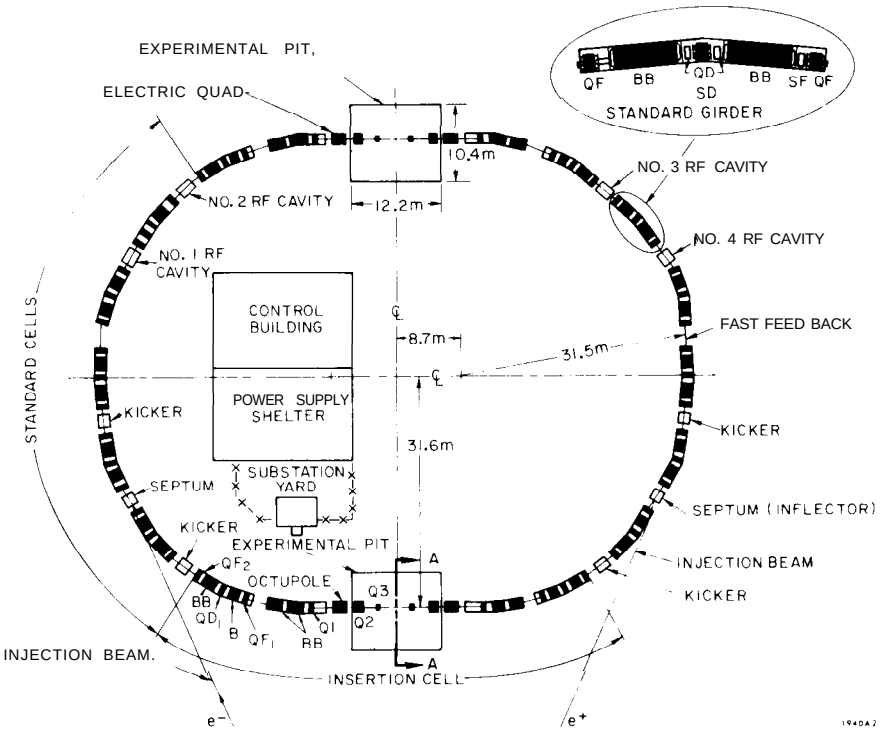
The formation of the fireball or virtual-photon intermediate state in e^+e^- annihilation is described in QED, a theory whose predictions have so far been confirmed by every experimental test. Since we therefore understand Step 1, the creation of the fireball, we are in a sense using the known e^+e^- annihilation process to probe the unknown hadrons that are produced in Step 2 of the process. Our ignorance is thus limited to the structure of the final-state hadrons and to the final-state interactions that occur when particles are created close together. And while that is a great deal of ignorance, it is much less than that of any other particle-production process. In addition, the quantum numbers of the final state in e^+e^- annihilation are simple enough so that we can hope to calculate them from our theoretical models. This is in sharp contrast, for example, to high-energy hadron-hadron collisions, in which very many different angular-momentum states may be involved and thus must be calculated.

2.4 The SPEAR Electron-Positron Storage Ring

In 1961, while work on the e^-e^- rings at HEPL continued, I began with D. Ritson of Stanford some preliminary design on a larger e^+e^- storage ring. In 1963 I moved from HEPL to the Stanford Linear Accelerator Center (SLAC), and set up a small group to carry out the final design of the e^+e^- ring. The design energy chosen was 3 GeV (each beam). A preliminary proposal for this colliding-beam machine was completed in 1964, and in 1965 a full, formal proposal was submitted to the U.S. Atomic Energy Commission (now ERDA).

There followed a period of about five years before any funding for this proposed project could be obtained. During this time, other groups became convinced of the research potential of the e^+e^- colliding-beam technique, and several other projects began construction. We watched this other activity enviously, worked at refining our own design, and tried to appropriate any good ideas the others had come up with. Finally, in 1970, funds were made available for a reduced version of our project, now called “SPEAR”, and we all fell to and managed to get it built in record time-some 21 months from the start of construction to the first beam collisions [5].

The SPEAR storage ring is located in a part of the large experimental area at the end of the 3-kilometer-long SLAC linac. The facility is shown schematically in Fig. 1. Short pulses of positrons, then electrons, are injected from the SLAC accelerator through alternate legs of the Y-shaped magnetic injection channel into the SPEAR ring. The stored beams actually consist of only a single short bunch of each kind of particle, and the bunches collide



1. Schematic of the SPEAR storage ring.

only at the mid-points of the two straight interaction areas on opposite sides of the machine. Special focusing magnets are used to give the beams a small cross-sectional area at these two interaction points. The time required to fill the ring with electrons and positrons is typically 15-30 minutes, while the data-taking periods between successive fillings are about 2 hours. To achieve this long lifetime, it is necessary to hold a pressure of about 5×10^{-9} torr in the vacuum chamber. The center-of-mass (c.m.) energy of the colliding e^+e^- system can be varied from 2.6 to 8 GeV. The radiofrequency power required to compensate for synchrotron radiation losses rises to 300 kilowatts at the maximum operating energy. The volume within which the e^+e^- collisions occur is small and well-defined ($\sigma_x \times \sigma_y \times \sigma_z = 0.1 \times 0.01 \times 5$ cm^3), which is a great convenience for detection.

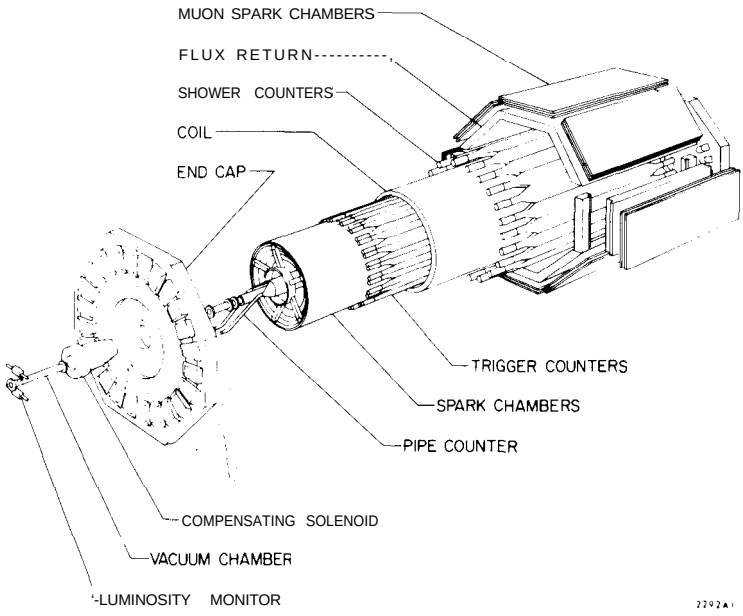
2.5 The Mark I Magnetic Detector

While SPEAR was being designed, we were also thinking about the kind of experimental apparatus that would be needed to carry out the physics. In the 1965 SPEAR proposal, we had described two different kinds of detectors: the first, a non-magnetic detector that would have looked only at particle multiplicities and angular distributions, with some rather crude particle-identification capability; the second, a magnetic detector that could add accurate momentum measurement to these other capabilities. When the early results in 1969, from the ADONE storage ring at Frascati, Italy, indicated

that hadrons were being produced more copiously than expected, I decided that it would be very important to learn more about the final states than could be done with the non-magnetic detector.

Confronted thus with the enlarged task of building not only the SPEAR facility itself but also a large and complex magnetic detector, I began to face up to the fact that my group at SLAC had bitten off more than it could reasonably chew, and began to search out possible collaborators. We were soon joined by the groups of M. Perl, of SLAC; and W. Chinowsky, G. Goldhaber and G. Trilling of the University of California's Lawrence Radiation Laboratory (LBL). This added manpower included physicists, graduate students, engineers, programmers and technicians. My group was responsible for the construction of SPEAR and for the inner core of the magnetic detector, while our collaborators built much of the particle-identification apparatus and also did most of the programming work that was necessary to find tracks and reconstruct events.

This collaborative effort results in the Mark 1 magnetic detector, shown schematically in Fig. 2. The Mark I magnet produces a solenoidal field, coaxial with the beams, of about 4 kilogauss throughout a field volume of about 20 cubic meters. Particles moving radially outward from the beam-interaction point pass successively through the following elements: the beam vacuum pipe; a trigger counter; 16 concentric cylinders of magnetostrictive wire spark chambers that provide tracking information for momentum measurements; a cylindrical array of 48 scintillators that act as both trigger and time-of-flight counters; the one-radiation-length thick aluminium magnet coil; a cylindrical array of 24 lead-scintillator shower counters that provide



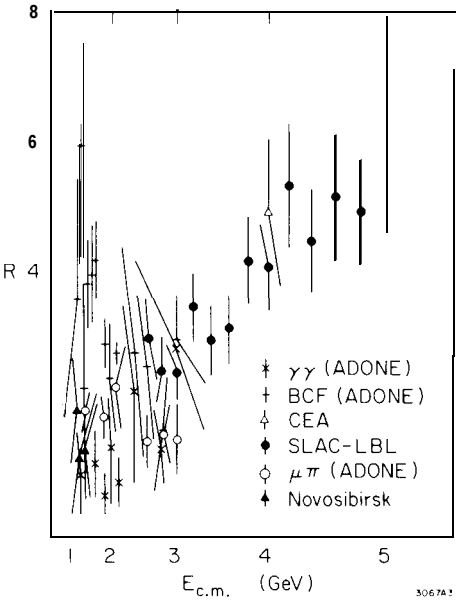
2. An exploded view of the SLAG-LBL magnetic detector.

electron identification; the 20-cm-thick iron flux-return plates of the magnet; and finally an additional array of plane spark chambers used to separate muons from hadrons.

The Mark I magnetic detector was ready to begin taking data in February 1973. During the fall of 1977 it will be replaced at SPEAR by a generally similar device, the Mark II, that will incorporate a number of important improvements. During its career, however, the Mark I has produced a remarkable amount of spectacular physics [6].

3. EARLY EXPERIMENTAL RESULTS

I would like to set the stage for the description of the journey from the ψ's to charm by briefly reviewing here the situation that existed just before the discovery of the new particles. The main international conference in high-energy physics during 1974 was held in July in London. I presented a talk at the London Conference [7] in which I tried to summarize what had been learned up until that time about the production of hadrons in e^+e^- annihilation. This information, shown in Fig. 3, will require a little bit of explanation.



3. The ratio *R as of* July 1974.

3.1. The Hadron/Muon-Pair Ratio

Measurements of the process e+e-+hadrons can be presented straightforwardly in a graph which plots the hadron-production cross section against the c.m. energy of the colliding e^+e^- system. For reasons that I shall explain later, it has become common practice to replace the hadron-production cross section in such graphs by the following ratio:

$$R = \frac{\text{cross section for } e^+e^- \to \text{hadrons}}{\text{cross section for } e^+e^- \to \mu^+\mu^-} \qquad (1)$$

It is that ratio R that is plotted vs. c.m. energy in Fig. 3. Historically, the earliest measurements of R were made at the ADONE ring at Frascati; these occupy the lower-energy region of the graph, and they indicate values of R ranging from less than 1 to about 6. These were followed by two important measurements of R made at the storage ring that had been created by rebuilding the Cambridge Electron Accelerator (CEA) at Harvard; the CEA measurements gave an R value of about 5 at $E_{c.}$ of 4 GeV, and R fr *6* at 5 GeV. The early experimental results from the SLAC-LBL experiment at SPEAR filled in some of the gap between the ADONE and CEA results, and between the two CEA points, in a consistent manner; that is, the SPEAR data appear to join smoothly onto both the lower and higher energy data from ADONE and from CEA. With the exception of the experimental points at the very lowest energies, the general picture conveyed by Fig. 3 is that the value of R seems to rise smoothly from perhaps 2 to 6 as $E_{c.m.}$ increases from about 2 to 5 GeV.

3.2. The Theoretical Predictions

During the same London Conference in 1974, J. Ellis of CERN [8] undertook the complementary task of summarizing the process $e^+e^- \rightarrow$hadrons from a theoretical point of view. Once again, the predictions of many different theories could most conveniently be expressed in terms of the hadron/muon-pair ratio R rather than directly as hadron-production cross sections. The most widely accepted theory of the hadrons at that time gave the prediction that $R = 2$; but there were many theories. Let me illustrate this by reproducing here, as Table I, the compilation of R predictions that Ellis included in his London Talk. As this table shows, these predictions of the hadron/muon-pair ratio ranged upward from 0.36 to ∞, with many a stop along the way.

I included this table to emphasize the situation that prevailed in the Summer of 1974-vast confusion. The cause of the confusion lay in the paucity of e^+e^- data and the lack of experimental clues to the proper direction from elsewhere in particle physics. The clue lay just around the next corner, but that corner itself appeared as a totally unexpected turn in the road.

Table I. Table of Values of R from the Talk by J. Ellis at the 1974 London Conference [8] (references in table from Ellis's talk)

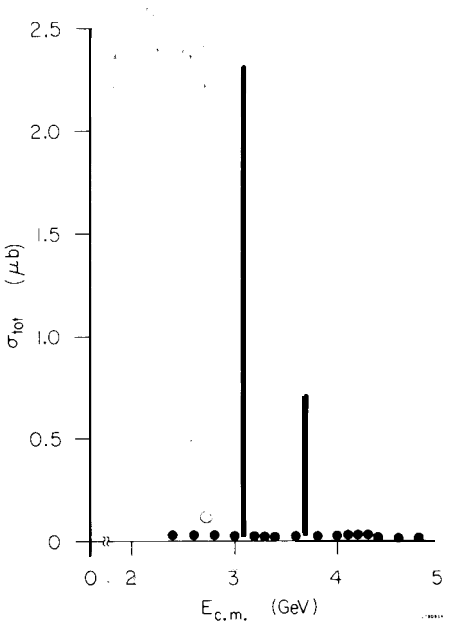
Value	*Model*	
0.36	Bethe-Salpeter bound quarks	Bohm et al., Ref .42
2/3	Gell-Mann-Zweig quarks	
0.69	Generalized vector meson dominance	Renard, Ref. 49
~ 1	Composite quarks	Raitio, Ref. 43
10/9	Gell-Mann-Zweig with charm	Glashow et al., Ref. 31
2	Colored quarks	
2.5 to 3	Generalized vector meson dominance	Greco, Ref. 30
2 to 5	Generalized vector meson dominance	Sakurai, Gounaris, Ref, *47*

3-1/3	Colored charmed quarks	Glashow et al., Ref. 31
4	Han-Nambu quarks	Han and Nambu. Ref. 32
5.7±0.9	Trace anomaly and p dominance	Terazawa, Ref. 27
5.8+3.2 -3.5	Trace anomaly and ε dominance	Orito et al., Ref. 25
6	Han-Nambu with charm	Han and Nambu, Ref. 32
6.69 to 7.77	Broken scale invariance	Choudhury, Ref. 18
8	Tati quarks	Han and Nambu, Ref. 32
8 ± 2	Trace anomaly, and ε dominance	Eliezer, Ref. 26
9	Gravitational cut-off, Universality	Parisi, Ref. 40
9	Broken scale invariance	Yachtmann. Ref. 39
16	$SU_{12} \times SU_2$) gauge models	Fritzsch & Minkowski. Ref. 34
35-1/3	**$su_{16} \times SU_6$)**	
~ 5000	High $\mathcal{Z}$ quarks	Yock. Ref. 73
70,383	Schwinger's quarks)	
∞	∞ of partons	Cabibbo and Karl, Ref. 9 Matveev and Tolkachev, Ref. 35 Rozenblit. Ref. 36

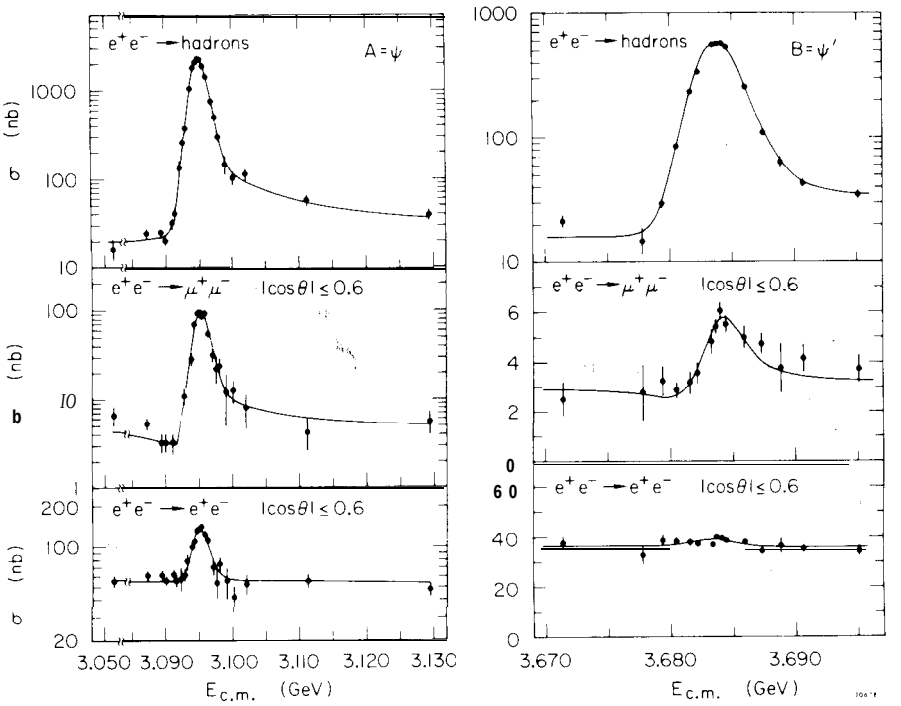
4. THE PSI PARTICLES

4.1. Widths of the Psi Resonances

Figure 4 shows the cross section for hadron production at SPEAR on a scale where all of the data can be plotted on a single graph. This figure is clearly dominated by the giant resonance peaks of the ψ and the ψ'.The extreme



4. The total cross section of hadron production vs. center-of-mass energy.



5. Hadron. $\mu^+\mu^-$ and e^+e^- pair production cross section in the regions of the ψ and ψ'. The curves are fits to the data using the energy spread in the colliding beams as the determinant of the widths.

narrowness of the peaks implies that these two states are very long-lived, which is the principal reason why they could not be accounted for by the previously successful model of hadronic structure. In Fig. 5 we show the ψ and ψ' peaks on a greatly expanded energy scale, and also as they are measured for three different decay modes: $\psi, \psi' \rightarrow$ hadrons; $\psi, \psi' \rightarrow \mu^+\mu^-$; and $\psi, \psi' \rightarrow e^+e^-$. In this figure the ψ and ψ' peaks can be seen to have experimental widths of about 2 MeV and 3 MeV, respectively. These observed widths are just about what would be expected from the intrinsic spread in energies that exists within the positron and electron beams alone, which means that the true widths of the two states must be very much narrower. The true widths can be determined accurately from the areas that are included under the peaks in Fig. 5 and are given by the following expression :

$$\int \sigma_i \, dE = \frac{6\pi^2}{M^2} B_e B_i \, \Gamma \tag{2}$$

where σ_i is the cross section to produce final state i, B_i is the branching fraction to that state, B_e is the branching fraction to e^+e^-, M is the mass of the state, and Γ is its total width. The analysis is somewhat complicated by radiative corrections but can be done, with the result that [9]

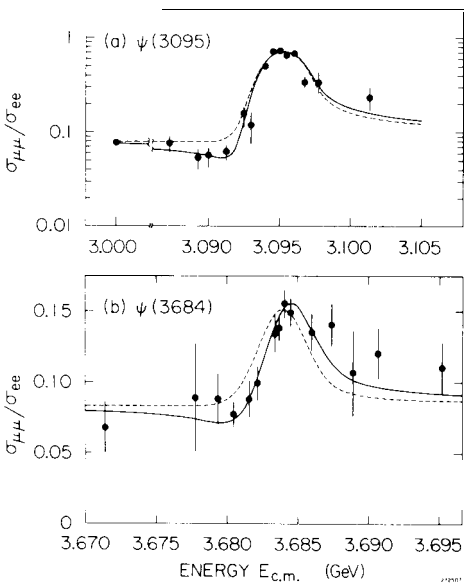
$$\begin{aligned} \Gamma\psi &= 69 \pm 13 \text{ keV} \\ \Gamma\psi' &= 225 \pm 56 \text{ keV} \end{aligned} \tag{3}$$

The widths that would be expected if the psi particles were conventional hadrons are about 20% (of their masses. Thus the new states are several thousand times narrower than those expected on the basis of the conventional model.

4.2. Psi Quantum Numbers

The quantum numbers of the new psi states were expected to be $J^{PC} = 1^{--}$ because of their direct production in e^+e^- annihilation and also because of the equal decay rates to e^+e^- and $\mu^+\mu^-$. In so new a phenomenon, however, anything can go, and so that assumption needed to be confirmed. In particular, one of the tentative explanations of the psi particles was that they might be related to the hypothetical intermediate vector boson, a particle that had long been posited as the carrier of the weak force. Such an identification would permit the psi's to be a mixture of $J^{PC} = 1^-$ and 1^{+-}. These quantum numbers can be studied by looking for an interference effect between on- and off-peak production of muon pairs, since the latter is known to be pure 1^{--}. If the new particles were also 1^{--}, then an interference should occur and produce two recognizable effects: a small dip in the cross section below the peak, and an apparent shift in the position of the peak relative to that observed in the hadron channels. In addition, any admixture of 1^{+-} could be expected to show up as a forward/backward asymmetry in the observed angular distribution.

This analysis was carried out as soon as there were sufficient data available for the purpose. The postulated interference effect was in fact observed, as shown in Fig. 6, while no angular asymmetry was seen [8, 9]. Thus both of the psi states were firmly established as $J^{PC} = 1^{--}$.

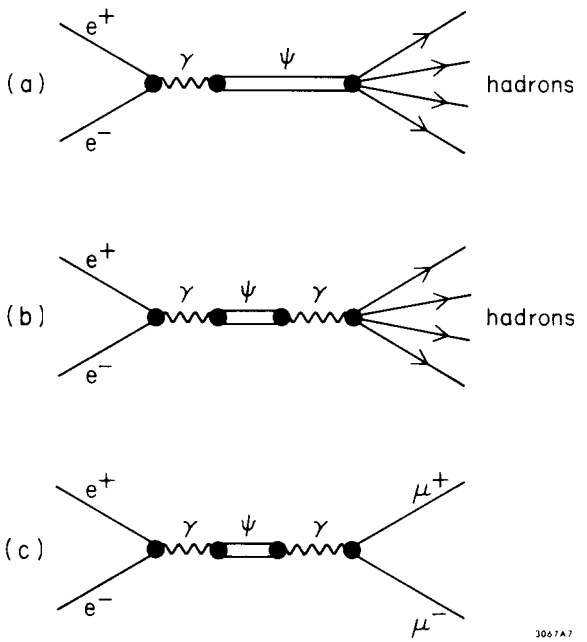


6. The $\mu^+\mu^-$ cross sections at the ψ and the ψ'. The solid curves show the results expected if both states are $J^{PC} = 1^{--}$ and hence interfere with the non-resonant $\mu^+\mu^-$ production. The dashed curves assume no interference.

4.3. Psi Decay Modes

We also studied the many decay modes of the ψ and ψ'. In these studies it was important to distinguish between direct and "second-order" decay processes, a point that is illustrated in Fig. 7. This figure shows the following processes:

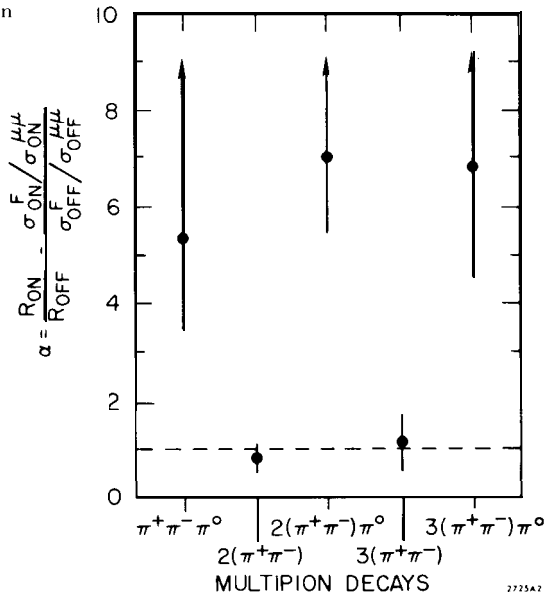
$$\begin{array}{ll} \text{(a)}\ e^+e^- \to \gamma \to \psi \to \text{hadrons} & \text{(direct decay)} \\ \text{(b)}\ e^+e^- \to \gamma \to \psi \to \gamma \to \text{hadrons} & \left.\begin{array}{l}\\ \end{array}\right\} \text{(second-order electro-} \\ \text{(c)}\ e^+e^- \to \gamma \to \psi \to \gamma \to \mu^+\mu^- & \text{magnetic decay)} \end{array} \qquad (4)$$



7. Feynmnn diagrams for ψ production and (a) direct decay to hadrons, (b) second-order electromagnetic decay to hadrons. and (c) second-order electromagnetic decay to $\mu^+\mu^-$.

In processes (b) and (c), hadrons and muon-pairs are produced by virtual photons in exactly the same way that they are produced at off-resonance energies. If the observed hadrons were produced only through second-order electromagnetic decay, then the hadron/muon-pair production ratio, R, would be the same on-resonance as off. This is decidedly not the case. Since R is much larger on-resonance than off, both ψ and ψ' do have direct hadronic decays.

More branching fractions for specific hadronic channels have been measured for the ψ and ψ' than for any other particles. Most of these are of interest only to the specialist, but a few have told us a good deal about the psi particles. Since the second-order electromagnetic decays also complicate these analyses, we must again make on- and off-resonance comparisons between muon-pair production and the production of specific hadronic final states. In Fig. 8 we show such a comparison plotted against the number of pions observed in the final state [10]. Even numbers of pions observed are consistent with what is



8. The ratio of the ratios of hadron to $\mu^+\mu^-$ production on and off the ψ' resonance *vs.* the number of π mesons in the final state.

expected from second-order electromagnetic decays, while the observed odd-pion decays are much enhanced. The ψ decays appear, from these data, to be governed by a certain selection rule (G-parity conservation) that is known to govern only the behavior of hadrons, thus indicating that the ψ itself is a hadron.

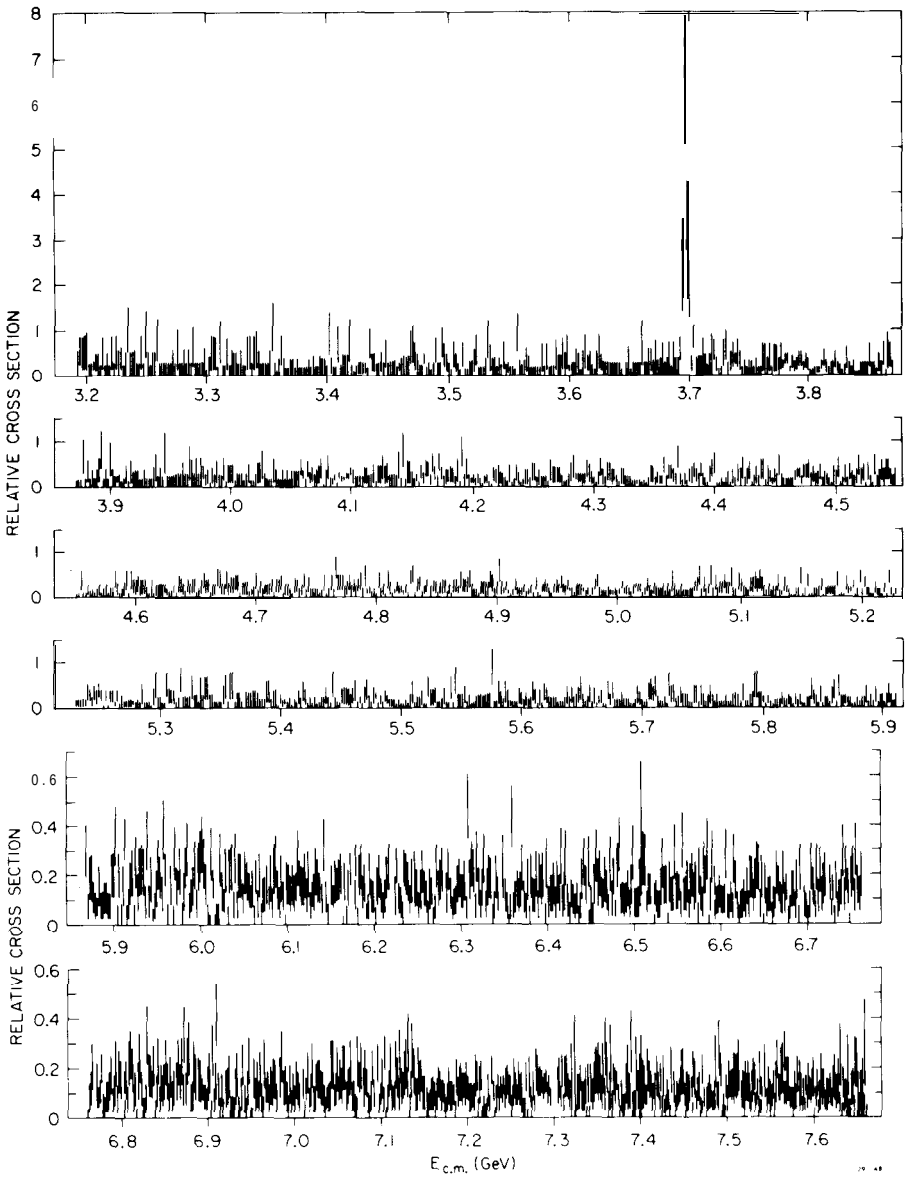
There are certain specific decay modes that, if observed, provide definite evidence on the isospin of the psi particles. Such modes are

$$\psi \text{ or } \psi' \to \pi^+\pi^-\pi^\circ\, \Lambda\bar{\Lambda},\ \bar{p}p. \tag{5}$$

Each of these decay modes has in fact been seen, thus establishing $I^G J^{PC} = 0^- 1^{--}$ for both particles.

4.4. Search for Other Narrow Resonances

By operating the SPEAR storage ring in a "scanning" mode, we have been able to carry out a systematic search for any other very narrow, psi-like resonances that may exist. In this scanning mode, the ring is filled and set to the initial energy for the scan; data are taken for a minute or two; the ring energy is increased by about an MeV; data are taken again; and so forth. Figure 9 shows these scan data from c.m. energies of about 3.2 to 8 GeV [11, 12]. No statistically significant peaks (other than the gf that was found in our first scan) were observed in this search, but this needs two qualifications. The first is that the sensitivity of the search extends down to a limit on possible resonances that have a cross section *x* width of about 5% to 10%, of that of the ψ. The second qualification is that the particular method of search is sensitive only to extremely narrow resonances like the ψ and ψ'; other, much broader resonances have been found at SPEAR, and we shall soon see how these apparently much different states fit into the picture.



9. The fine-scan data from our search for other narrow ψ-like states. The signal near 3.7 GeV is the ψ'.

5. THE INTERMEDIATE STATES

5.1. Radiative Transitions

There are other new states, related to the ψ and ψ' but not directly produced in e^+e^- annihilation, which are observed among the decay products of the two psi particles. More specifically, these new states are produced when either ψ or ψ' decays through the emission of a gamma-ray:

$$\psi \text{ or } \psi' \rightarrow \gamma + \text{intermediate state} \tag{6}$$

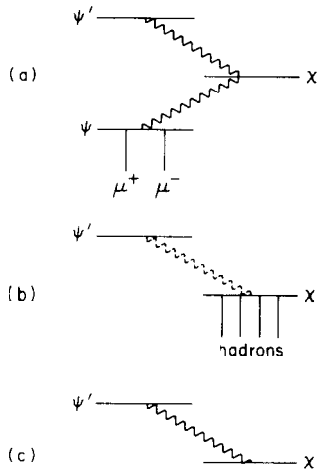
At least four (perhaps five) distinct intermediate states produced in this way have been observed experimentally.

The first such observation was made by an international collaboration working at the DORIS e^+e^- storage ring at the DESY laboratory in Hamburg [13]. This state was named P_c, and its mass was found to be about 3500 MeV. This same group [14] in collaboration with another group working at DESY later found some evidence for another possible state, which they called X, at about 1800 MeV [15]. At SPEAR, the SLAC-LBL group has identified states with masses of about 3415, 3450 and 3550 MeV, and has also confirmed the existence of the DESY 3500-MeV state. We have used the name x to distinguish the state intermediate in mass between the $\psi(3095)$ and the $\psi'(3684)$. To summarize these new states :

$$\begin{aligned}
&\psi'(3684) \to \gamma + \chi(3550)\\
&\psi'(3684) \to \gamma + \chi(3500) \text{ or } P_c\\
&\psi'(3684) \to \gamma + \chi(3555)\\
&\psi'(3684) \to \gamma + \chi(3415)\\
&\psi\,(3095) \to \gamma + X(2800) \text{ (not yet firmly established)}
\end{aligned} \tag{7}$$

5.2. *Three Methods of Search*

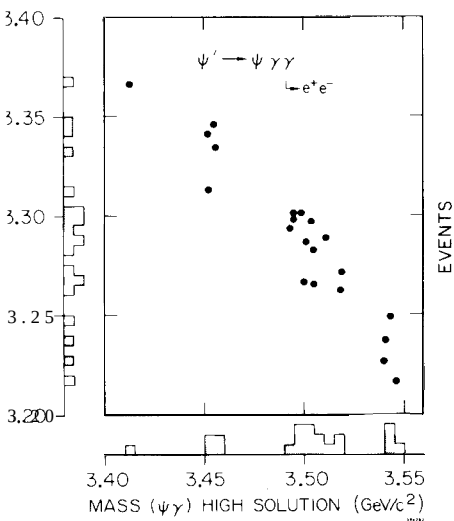
The three methods we have used at SPEAR to search for these intermediate states are indicated schematically in Fig. 10. To begin with, the storage ring is operated at the center-of-mass energy of 3684 MeV that is required for resonant production of the ψ'. In the first search method, Fig. 10(a), ψ' decays to the intermediate state then decays to the ψ through γ-ray emission; and finally the ψ decays, for example, into $\mu^-\mu^-$. The muon-pair is detected along with one or both of the y-ray photons. This was the method used at DESY to find the 3500-MeV state and also by our group at SLAC to confirm this state [16]. In our apparatus at SPEAR, it will occasionally happen that one of the two γ-ray photons converts into an e^+e^- pair before entering the tracking region of the detector. This allows the energy of the converting γ-ray to be



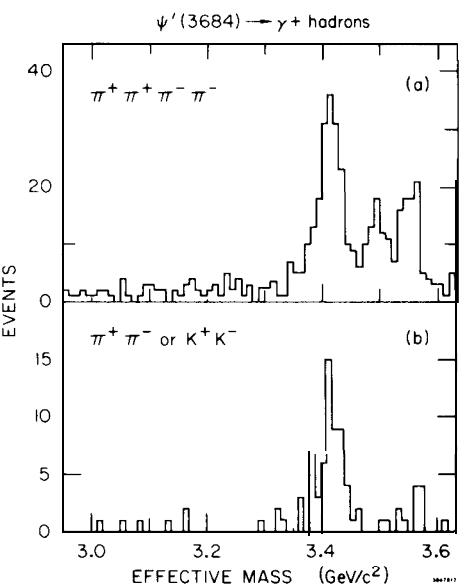
10. Schematics of the three methods of searching for narrow intermediate states.

measured very accurately, and this information can be combined with the measured momenta of the final $\mu^+\mu^-$ pair to make a two-fold ambiguous determination of the mass of the intermediate state. The ambiguity arises from the uncertainty in knowing whether the first or the second gamma-rays in the decay cascade have been detected. It can be resolved by accumulating enough events; to determine which assumption results in the narrower mass peak. The peak associated with the second γ-rays will be Doppler broadened because these photons are emitted from moving sources.) Figure 11 shows the alternate low- and high-mass solutions for a sample of our data [17]. There appears to be clear evidence for states at about 3.45, 3.5 and 3.55 GeV.

The second search method we have used, Fig. 10(b), involves measuring the momenta of the final-state hadrons and reconstructing the mass of the intermediate state [18]. Figure 12 shows two cases in which the effective mass of the final-state hadrons recoils against a missing mass of zero (that is, a γ-ray). In the case where 4 pions are detected, peaks are seen at about 3.4, 3.5 and 3.55 GeV. In contrast, the 2-pion or 2-kaon case shows only one clear peak at 3.4 GeV, with perhaps a hint of something at 3.55 GeV. The appearance of the 2-pion or 2-kaon decay modes indicates that the quantum numbers of the states in question must be either 0^{++} or 2^{++}.



The high-resolution ψ'—γ mass data. clustering indicates at least 3 intermediate s.



12. The invariant mass of the indicated hadron final states that appear with a γ-ray in ψ' decay. The data show three distinct intermediate states, one of which is not seen in the previous figure.

In the third method of search, Fig. 10(c), only a single y-ray is detected. The presence of a monoenergetic y-ray line would signal a radiative transition directly to a specific intermediate state. In our apparatus, this method is difficult to apply because of the severe background problems, but we were able to identify the direct y-ray transition to the 3.4 GeV state [17]. A different experimental group working at SPEAR (a collaboration among the Universities of Maryland, Princeton, Pavia, Stanford and UC-San Diego) was able to make use of a more refined detection system to observe several of these radiative transitions and to measure the ψ' branching franctions of those states [19]

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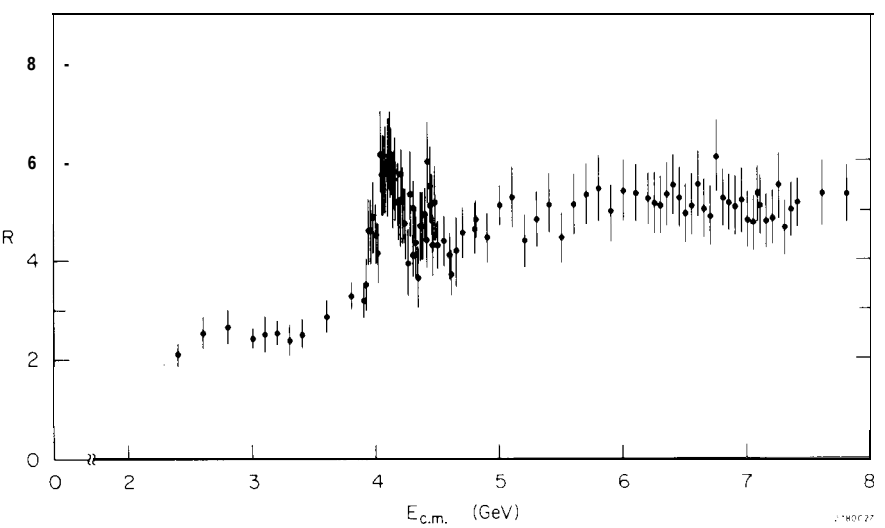
To summarize, these studies have led to the addition of four (the 2800-MeV state is still marginal) new intermediate state, all with charge-conjugation C = + 1, to the original ψ and ψ' particles.

6. TOTAL CROSS SECTION AND BROADER STATES

6.1. Total Cross Section

So far our discussion of the process e^+e^-hadrons has been concerned largely with the two psi particles, which are created directly in e^+e^-annihilation, and with the intermediate states, which are not directly created but rather appear only in the decay products of the ψ and ψ'. It is now time to turn our attention to the larger picture of hadron production to see what else can be learned.

Figure 4 presented the total cross section for e+e-hadrons over the full range of c.m. energies accessible to SPEAR. This figure was dominated by the ψ and ψ' resonance peaks, and very little else about the possible structure of the cross section outside of these peaks was observable. We now remedy this situation in Fig. 13, which shows the hadron/muon-pair ratio R, with the dominating ψ and ψ' resonance peaks removed, including their radiative tails.

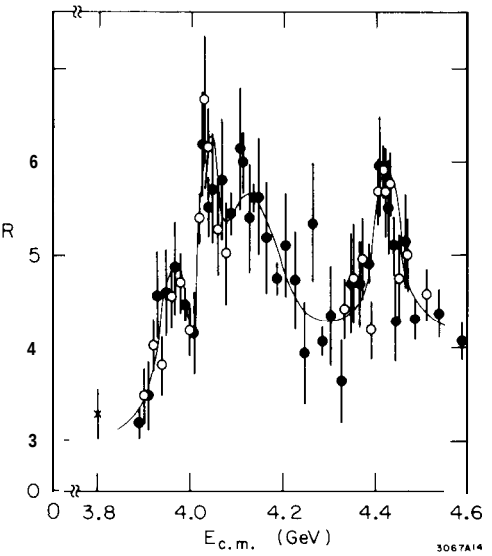


13. The ratio R with the ψ and ψ' deleted (including their radiative tails).

We can characterize the data in the following way. Below about 3.8 GeV, R lies on a roughly constant plateau at a value of $\simeq 2.5$; there is a complex transition region between about 3.8 and perhaps 5 GeV in which there is considerable structure; and above about 5.5 GeV, R once again lies on a roughly constant plateau at a value of $\simeq 5.2$ GeV.

6.2. Broader (Psi?) States

The transition region is shown on a much expanded energy scale in Fig. 14. This figure clearly shows that there seem to be several individual resonant states superposed on the rising background curve that connects the lower and upper plateau regions [20]. One state stands out quite clearly at a mass of 3.95 GeV, and another at about 4.4 GeV. The region near 4.1 GeV is remarkably complex and is probably composed of two or more overlapping states; more data will certainly be required to try to sort this out.



14. An expanded view of R in the transition region around 4 GeV.

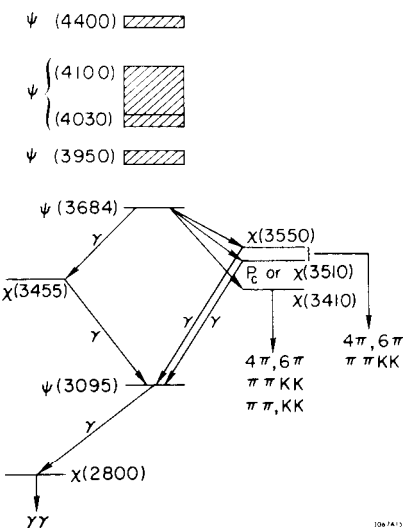
The properties of the several states within the transition region are very difficult to determine with any precision. One obvious problem is that these resonances sit on a rapidly rising background whose exact shape is presently neither clear experimentally nor calculable theoretically. Since these new states are, like the ψ's, produced directly in e^+e^- annihilation, they all have $J^{PC} = 1^{--}$ and can therefore interfere with each other, thus distorting the classical resonance shape that would normally be expected from a new particle. Additional shape-distortion might be expected because new particle-production thresholds are almost certainly opening up in the transition region between the lower and upper plateaus. While precise properties can't be given for the new states, we can get some rough numbers from the data. The 3.95-GeV state (ψ'') has a width of about 40-50 MeV. The 4.4-GeV state (ψ'''') seems to be about 30-MeV wide. The 4.1-GeV region (temporarily called ψ''') seems to consist of at least two peaks: one at 4.03 GeV, which is 10-20 MeV wide, and a broad enhancement at 4.1 GeV, about 100-MeV wide.

The widths of all of these states are much greater than the intrinsic energy spread in the e^+e^- beams, and very much greater than the widths of the ψ and ψ'. The suspicion remains, however, that they may still be correctly identified as members of the psi sequence, and that the vast apparent differences between their widths and those of the ψ and ψ' may result simply from the fact that the higher mass states can undergo rapid hadronic decay through new channels that have opened up above the 3684-MeV mass of the ψ'. As with most of the questions in the transition region, this matter will require a good deal more experimental study before it is resolved. In the meantime, however, we shall tentatively add the three or four new psi-like states shown above to the growing list of members of the "psion" family.

7. AN EXCURSION INTO THEORY

Up to this point, we have been cataloguing new particles without much worrying about what it all means. Granting full status to even the several doubtful states, we have a total of 11 new particles. These are grouped together in Fig. 15 in a kind of energy-level diagram, which also includes principal decay modes.

The system shown in Fig. 15, with its radiative transitions, looks remarkably like the energy-level diagram of a simple atom, in fact like the simplest of all "atoms"-positronium, the bound state of an electron and a positron. Although the mass scale for this new positronium is much larger than that of the old, the observed states of the new system can be placed in a one-to-one correspondence with the levels expected for a bound fermion-antifermion system such as e^+e^-. Table II shows these predicted levels together with the most probable assignments of the new particles to the appropriate levels. To gain some insight into the origins of the new positronium system, let's now turn to some specific theoretical models.



15. An energy-level diagram of the new particles. The many observed decay modes of the psi family have been omitted.

Table II. Some of the low lying bound states of a fermion-antifermion system together with an assignment of the new particle to states with appropriate quantum numbers.

State	L	S	J^{PC}	Particle
1^3S_1	0	1	1^{--}	ψ'
2^3S_1	0	1	1^{--}	ψ''
3^3S_1	0	1	1^{--}	ψ'''
1^3D_1	2	1	1^{--}	ψ''
2^3D_1	2	1	1^{--}	ψ''''
1^1S_0	0	0	0^{-+}	X
2^1S_0	0	0	0^{-+}	$\chi(3.45)$
1^3P_0	1	1	0^{+-}	$\chi(3.4)$
1^3P_1	1		1^{+-}	$\chi(3.5)$
1^3P_2	1		2^{++}	$\chi(3.55)$

7.1. The 3-Quark Model

Some 25 years ago, when only three kinds of hadrons were known (proton, neutron and pi-meson), these particles were universally regarded as simple, indivisible, *elementary* objects. In those days the central task in hadron physics was the effort to understand the strong nuclear force between protons and neutrons in terms of pi-meson exchange. But as the family of hadrons grew steadily larger (they are now numbered in the hundreds), it became increasingly difficult to conceive of them *all* as elementary. In 1963, M. Gell-Mann and G. Zweig independently proposed a solution to this dilema-that *none* of the hadrons was elementary, but rather that all were complex structures in themselves and were built up from different combinations of only three fundamental entities called quarks. These quarks were assumed to carry the familiar 1/2 unit of spin of fermions, but also to have such unfamiliar properties as fractional electric charge and baryon number. A brief listing of the 3 quarks and 3 antiquarks and their properties is given in Table III.

Table III. Properties of the 3 Quarks and 3 Antiquarks

Quarks				Antiquarks			
Symbol	Charge	Baryon Number	Strangeness	Symbol	Charge	Baryon Number	Strangeness
u	2/3	1/3	0	$\bar{u}$	−2/3	−1/3	0
d	−1/3	1/3	0	$\bar{d}$	1/3	−1/3	0
s	−1/3	1/3	1	$\bar{s}$	1/3	1/3	−1

According to this 3-quark model, all mesons were made up of one quark and one antiquark; ail baryons, of three quarks; and all antibaryons, of three antiquarks. The quark compositions of some of the better known hadrons are shown here as examples:

$$\pi^+ = u\bar{d},\ K^+ = u\bar{s},\ p = uud,\ \bar{n} = \bar{d}\bar{d}\bar{u}. \tag{8}$$

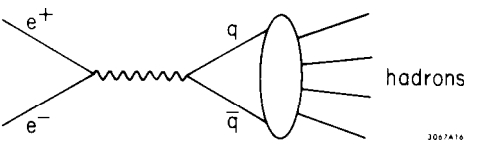
Prior to 1974, all of the known hadrons could be accommodated within this basic scheme. Three of the possible meson combinations of quark-antiquark ($u\bar{u}$, dd, $s\bar{s}$) could have the same quantum numbers as the photon, and hence could be produced abundantly in e^+e^- annihilation. These three predicted states had all infact been found; they were the familiar $\varrho(760), \omega(780)$ and $\varphi(1005)$ vector mesons.

7.2. R in the Quark Model

The quark model postulated a somewhat different mechanism for the process e+-e--+hadrons than that previously described. For comparison,

Customary View	Quark Model Hypothesis
$e^+e^- \rightarrow \gamma \rightarrow$ hadrons	$e^+e^- \rightarrow \gamma \rightarrow q\bar{q} \rightarrow$ hadrons

where $q\bar{q}$ means any quark-antiquark pair. The quark-model hypothesis is shown schematically in Fig. 16. In this picture the virtual photon intermediate state creates a $q\bar{q}$ pair, which then in turn "clothe" themselves with additional $q\bar{q}$ pairs to form the hadrons that are observed in the final state.



16. Hadron production in the quark model.

Since the quarks are assumed to be elementary, point-like fermions and thus similar to electrons and muons in their electromagnetic properties, it was possible to predict the ratio that should exist between the producton cross sections for quark pairs and muon pairs:

$$\frac{\sigma_{q\bar{q}}}{\sigma_{\mu^+\mu^-}} = q_i^2 \tag{10}$$

where q_i is simply the quark's electric charge. Of course, quarks were supposed to have half-integral spin and fractional charge in the final state, while all hadrons have integral charge and some hadrons have integral spin. In a breathtaking bit of daring it was assumed that the "final-state" interactions between quarks that were necessary to eliminate fractional charge and half-integral spin would have no effect on the basic production cross section. With this assumption the ratio of hadron production to muon-pair production becomes simply

$$R = \sum_{u,d,s} q_i^2. \tag{11}$$

As developed up to 1974, the quark model actually included 3 triplets of quarks, rather than simply 3 quarks, so that with this 3 x 3 model the hadron/muon-pair ration, R, would be

$$R = (3[2/3)^2 + (-1/3)^2 + (-1/3)^2]) = 2. \tag{12}$$

This beautiful model had great simplicity and explanatory power, but it could not accommodate the ψ and ψ' particles. Nor could it account for the two plateaus that were observed in the measured values of R. The model allowed for excited states of $u\bar{u}, d\bar{d}$ and $s\bar{s}$, but the required widths were typically some 20% of the mass of the excited state-more than 1000 times broader than the observed widths of the ψ and ψ'. Before that time there had been a number of suggested modifications or additions to the basic 3-quark scheme. I shall not describe these proposed revisions here except for the one specific model which seems now to best fit the experimental facts.

7.3. A Fourth Quark

The first publications of a theory based on 4 rather than 3 basic quarks go all the way back to 1964 [21], only a year or so after the original Gell- Mann/ Zweig 3-quark scheme. The motivation at that time was more esthetic than practical, and these models gradually expired for want of an experimental fact that called for more than a S-quark explanation. In 1970, Glashow, Iliopolous and Maiani [22] breathed life back into the 4-quark model in an elegant paper that dealt with the *weak* rather than the strong interactions. In this work the fourth quark-which had earlier been christened by Glashow the "charmed" quark (c)-- was used to explain the non-occurrence of certain weak decays of strange particles in a very simple and straight-forward way. The new c quark was assumed to have a charge of +2/3, like the u quark, and also to carry + 1 unit of a previously unknown quantum number called charm, which was conserved in both the strong and electromagnetic interactions but not in the weak interactions. The c and $\bar{c}$ quarks were also required to have masses somewhat larger than the effective mass of the 3 original quarks, and it was clear that they should be able to combine with the older quarks and antiquarks to form many new kinds of "charmed" hadrons [23].

7.4. "Charmonium"

The 4-quark theoretical model became much more compelling with the discovery of the psi particles. This model postulates that the ψ is the lowest mass $c\bar{c}$ system which has the quantum numbers of the photon. The ψ's long life is explained by the fact that the decay of the ψ into ordinary hadrons requires the conversion of *both* c and $\bar{c}$ into other quarks and antiquarks. The positronium-like energy-level states of the psions discussed earlier are also well accounted for by the $c\bar{c}$ system; indeed, 5 specific intermediate states were predicted by Applequist *et al. [24],* and by Eichten *et al.* [25], before they were actually discovered. It was the close analogy with positronium that led Applequist and Politzer to christen the new $c\bar{c}$ system *charmonium,* a name that has caught on.

The 4-quark model also requires two plateaus on R. Above the threshold for charmed-hadron production, the $R = 2$ calculation made above must be modified by the addition of the fourth quark's charge, which results in a prediction of $R = 10/3$ (not enough, but in the right direction). The broad psi-like states at 3.95, 4.1, and 4.4 GeV are accounted for by postulating that

the mass of the lightest charmed particle is less than half the mass of the ψ'' (3950) but more than half the mass of the very narrow $\psi'(3684)$, which means that ψ'' can decay strongly to charmed-particle pairs, but ψ' cannot.

To summarize briefly, the 4-quark model of the hadrons seemed to account in at least a qualitative fashion for all of the main experimental information that had been gathered about the psions, and by the early part of 1976 the consensus for charm had become quite strong. The $c\bar{c}$ system of charmonium had provided indirect but persuasive evidence for a fourth, charmed quark, but there remained one very obvious and critically important open question. The particles formed by the $c\bar{c}$ system are not in themselves charmed particles, since charm and anticharm cancel out to zero. But it is necessary to the theory that particles which exhibit charm exist ($c\bar{u}$, cd, etc.). What was needed, then, was simply the direct experimental observation of charmed particles, and the question was: Where were they [26]?

8. THE DISCOVERY OF CHARM

8.1. *What are We Looking For?*

By early 1976 a great deal had been learned about the properties that the sought-after charmed particles must have. As an example, it was clear that the mass of the lightest of these particles, the charmed D meson, had to fall within the range

$$1843 < m_{\mathrm{D}} < 1900 \text{ MeV}. \tag{13}$$

The lower limit was arrived at by noting once again that the $\psi'(3684)$ was very narrow and therefore could not decay into charmed particles, and also that the upper limit had to be consistent with the beginning of the rise of R from its lower to its upper plateau. Since the principal decay product of the c quark was assumed for compelling reasons to be the s quark, then the decay products of charmed particles must preferentially contain strange particlcs such as the K mesons. The charmed D mesons, for example, could confidently be expected to have the following identifiable decay modes:

$$\begin{aligned} &\mathrm{D}^{\circ} \rightarrow \mathrm{K}^{-}\pi^{+} \\ &\mathrm{D}^{\circ} \rightarrow \mathrm{K}^{-}\pi^{+}\pi^{-}\pi^{+} \\ &\mathrm{D}^{+} \rightarrow \mathrm{K}^{-}\pi^{+}\pi^{+} \end{aligned} \tag{14}$$

A further point was that, since the charmed quark would decay only through the weak interactions, one might reasonably expect to see evidence of parity violation in the decays of the D mesons.

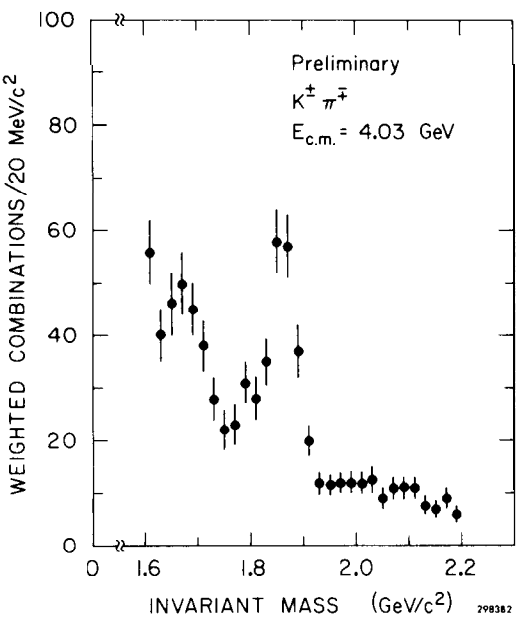
At SPEAR our collaboration had looked for such charm signatures in the limited data taken before the psi discoveries, but without success. As the post-psi data accumulated throughout 1975, it was evident that we should have another go at it, with particular emphasis on the results obtained at energies close to the expected charm threshold, where the simplest charmed mesons would be produced without serious masking effects from extraneous background. Since I spent the academic year 1975-76 on sabbatical leave at CERN, this chapter of the charmed-particle story belongs to my collaborators.

8.2. The Charmed Meson

With the advantages of a much larger data sample and an improvement in the method of distinguishing between pi- and K-mesons in the Mark I detector, a renewed search for charmed particles was begun in 1976. Positive results were not long in coming. The first resonance to turn up in the analysis was one in the mass distribution of the twoparticle system $K^+\pi^+$ in multiparticle events [27]. The evidence for this is shown in Fig. 17. This was the first direct indication of what might be the D meson, for the mass of 1865 MeV was in just the right region. If it was the D°, then presumably the production process was :

$$e^+e^- \rightarrow D^\circ \bar{D}^\circ + X \qquad (15)$$

where X represents any other particles. The D° or $\bar{D}^\circ$ would subsequently decay into the observed $K^+\pi^-$ or $K^-\pi^+$ some fraction of the time—the data indicated a branching fraction of about 2% for this charged two-body mode.



17. The invariant mass distribution of the $K^-\pi^+$ system in multiparticle final states. The peak at a mass of 1865 MeV is the D° meson.

The branching fraction was a little low compared to the charm-model predictions, but not alarmingly so. The measured width of the resonance was consistent with the resolution of our apparatus, which in this case was determined by the momentum resolution of the detector rather than by the more precise energy resolution of the circulating beams. The measured upper bound on the full width was about 40 MeV; the actual value could well be much smaller, as a weak-interaction decay of the D meson would require.

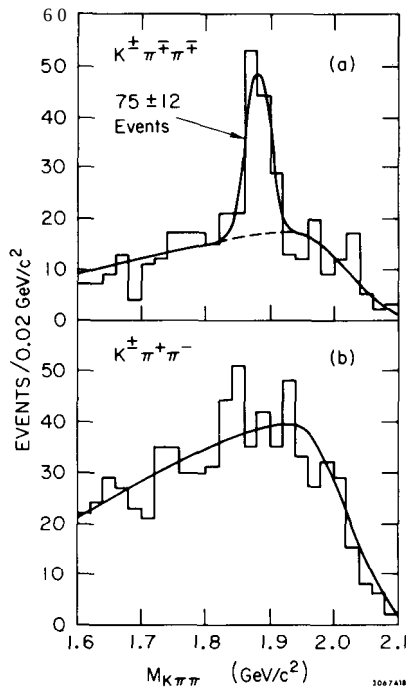
Continuing analysis of the data yielded two more persuasive findings. The first was a resonance in $K^+\pi^-\pi^+\pi^-$ or $K^-\pi^+\pi^-\pi^+$, which appears to be an alternate decay mode of the D° since the mass is also 1865 MeV. The second

was the discovery of the charged companions [28] of the D^0, which were observed at the slightly larger mass of 1875 MeV in the following decay channels :

$$D^+ \to K^- \pi^+ \pi^+ \qquad (16)$$
$$D^+ \to K^+ \pi^- \pi^-$$

The data for the charged D states are shown in Fig. 18. It is important to note that these states are *not* observed in three-body decay when the pions are oppositely charged :

$$D^+ \to K^+ \pi^- \pi^+ \qquad (17)$$
$$D^- \to K^- \pi^+ \pi^-$$



18. The invariant mass distribution of the $K\pi\pi$ system. The $D^{\pm}$ appears in the plot (a) with same-sign pions and not in the plot (b) with opposite-sign pions.

This is precisely what is required by the charmed-quark model. In addition to the clear identification of both neutral and charged D mesons, an excited state [29] of this meson (D*) has also turned up and has been seen to decay to the ground state by both strong and electromagnetic interactions:

$$D^* \to D + \pi \qquad (18)$$
$$D^* \to D + \gamma$$

Since we have several times mentioned the possibility that the psi-like states having masses above that of the $\psi'(3684)$ may be much broader than ψ and ψ' because they are able to decay strongly into charmed-particle pairs, it is interesting to note that this speculation has now been confirmed in the case of the $\psi'''(4030)$. It now appears, in fact, that the following are the principal decay modes of this particle:

$$\begin{aligned}\psi'''(4030) &\rightarrow D^{\circ}\bar{D}^{*} \\ &\rightarrow D^{*}\bar{D}^{\circ} \\ &\rightarrow D^{*}\bar{D}^{*}\end{aligned} \tag{19}$$

As a final bit of evidence in support of the charmed-meson interpretation of the experimental data, the predicted parity violation in D decay has also been observed. In the decay process $D^{\circ}\rightarrow K^{+}\pi^{-}$, the K and π each have spin-0 and odd intrinsic parity. This means that any spin possessed by the D° must show up as orbital angular momentum in the $K\pi$ system, and thus that the parity of the D° must be given by

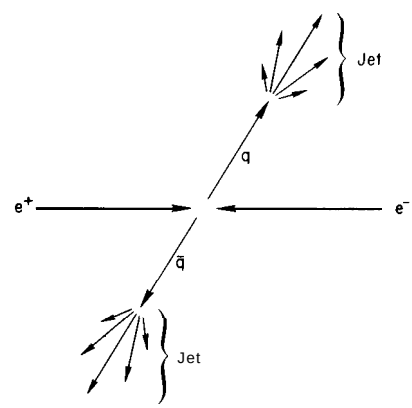
$$P = (-1)^{J} \tag{20}$$

where J is the spin of the D°. An analysis of the 3-body decay data, $D^{\pm}\rightarrow$ $\rightarrow K^{-}\pi^{+}\pi^{+}$ or $K^{+}\pi^{-}\pi^{-}$, showed that the parity cannot be the same as that given above, and therefore that parity must be violated in D-meson decay [30].

The experimental data that have been described here arc strikingly consistent with the predictions of the 4-quark or charm theory of the hadrons, and there is little doubt that charmed particles have now in fact been found. In addition to these charmed mesons uncovered at SPEAR, there has been recent information from Fermilab that a collaborative group working there under Wonyong Lee has now discovered the first of the charmed baryons [31] actually an antibaryon designed Λ_c to identify it as the charmed counterpart of the il.

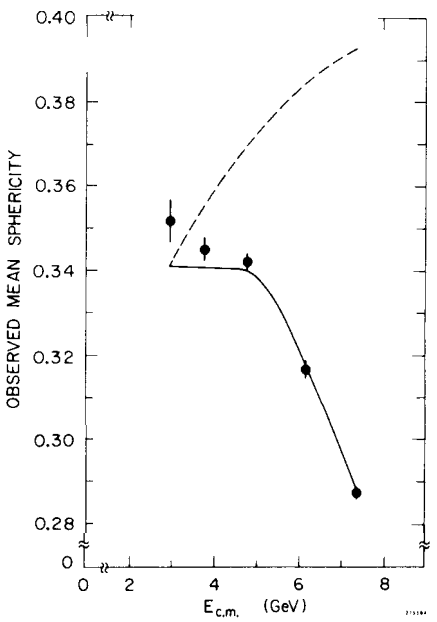
9. OBSERVATION OF <JETS

While this topic is not directly connected with the new particles, it does have a direct bearing on the validity of the quark model. As I noted earlier, the picture of e-e- annihilation that is derived from the quark model indicates that the final-state hadrons do not come directly from the virtual-photon intermediate state, but rather from the quark-antiquark pair that is first created from the electromagnetic fireball and subsequently forms the final hadrons. These hadrons are produced with low transverse momenta with respect to the $q\bar{q}$ direction, and as illustrated in Fig. 19, if the energy is sufficiently high, form two collimated jets of particles whose axes lie along the original $q\bar{q}$ direction.



19. Jet production in the quark model l.

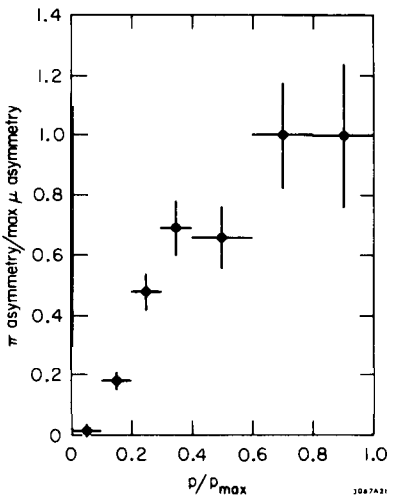
At SPEAR we have analyzed our highest-energy data [32] by determining for each event those particular axes that minimize the transverse momentum relative to those axes for all of the observed particles. This method of analysis leads to the definition of a quantity we have called "sphericity," which is related to the quadrupole moment of the particle distribution in momentum space. The more jet-like event, the lower the sphericity. Figure 20 shows the data compared to the jet model and to an "isotropic" model with no jet-like characteristics. As the energy increases, the events do become more jet-like as required. The result was excellent agreement, not only in the general sense but also in the finding that the angular distribution of the jet axes was consistent with the $1 + \cos^2\vartheta$ distribution that is expected if the jets originate from parent particles of spin-1/2.



20. The mean sphericity of multihadron events *vs.* center-of-mass energy. The solid curve is that expected of the jet model, while the dashed curve is that expected from an isotropic phase-space model.

In addition, under certain operating conditions the beams in the SPEAR storage ring become polarized, with the electron spin parallel and the positron spin antiparallel to the ring's magnetic bending field. In this polarized condition an azimuthal asymmetry in particle production can appear with respect to the direction of the beams. Jets measured under these conditions also displayed the azimuthal asymmetry that is expected of spin-l/2 particles.

Further, the individual hadrons within the jets also displayed this asymmetry [33]. It will be evident that the greater the momentum of a single hadron, the closer that hadron must lie to the original direction defined by the quark. By looking at pion production in detail, we were able to determine that as the pion momentum approached the maximum value possible for the particular machine energy, so did the azimuthal asymmetry approach the maximum possible asymmetry expected for spin-l/2 particles. This point is illustrated in Fig. 21.



2 21. The azimuthal asymmetry parameter for pions normalized to the asymmetry in μ-pair production vs. the fractional pion momentum.

I find it quite remarkable that a collection of hadrons, each of which has integral spin, should display all of the angular-distribution characteristics that are expected for the production of a pair of spin- 1/2 particles. Such behavior is possible without assuming the existence of quarks (the final-state helicity must be one along the direction of the particle or jet), but any other explanation seems difficult and cumbersome. In my view the observations of these jet phenomena in e^+e^- annihilation constitute one of the very strongest pieces of evidence for believing that there really is a substructure to the hadrons.

10. CONCLUSIONS AND QUESTIONS

The electron-positron colliding-beam experiments of the past two years have, I believe, settled the question of the significance of the psi particles. The charmonium family, the two plateaus in R, the wide resonances above charm threshold, the charmed particles themselves, the evidence for the weak decays of the charmed particles and the existence of jets-all these support most strongly the ideas of the quark model of hadron substructure and the 4-quark version of that model. To me, one of the most remarkable features of the quark model is that it correctly explains a great deal of data on strongly interacting particles with the most simple-minded of calculations. The charmonium spectrum, for example, is calculated with the nonrelativistic Schrödinger equation using a simple potential. The two plateaus in R and jet structure are explained by assuming that the final-state interactions of strongly interacting particles can be ignored. Why it is all so simple, while at the same time the quarks themselves appear confined to hadrons and are never seen in the free state, is one of the central questions of strong-interaction physics.

We already know, however, that the 4-quark model cannot be the complete story. The colliding-beam experiments are not entirely consistent with this model. The high energy plateau value of R is about 5.1 rather than 3-1/3 as demanded by the charm model. While R = 3-1/3 is only reached in the theory at very high energies, the difference between 3-1/3 and 5.1 arc too

large to be explained easily. At the same time, there is evidence in our data for a class of events (the μ-e events) which are not easily explained within the framework of 4-quarks and 4 leptons (e-, ν_e, μ^-, ν_μ) and which may require an expansion of the lepton family and/or the quark family. These inconsistencies immediately bring up the question of how many quarks and leptons there are.

There are two schools of thought on this question. One school says that the quark system is complete or nearly complete-while there may be a few more quarks to be found, there are a small number of indivisible elements, among which are the present four, and all of the strongly interacting particles are built out of these elementary and indivisible components. The other school says that the quarks themselves are probably built from something still smaller, and that we shall go on forever finding smaller and smaller entities each inside the next larger group.

These and other questions on particle structure may be answered by the next generation of e^+e^- colliding-beam machines now being built at DESY and SLAC which will reach 35 to 40 GeV in the center-of-mass system. Experiments on these machines will begin in 4 to 5 years and should tell us promptly about the existence of new plateaus in R, new "oniums", or new leptons.

An even more fundamental set of questions, which I find more interesting than the number of quarks, will probably not be answered by experiments at any accelerator now in construction. These questions have to do with the possibility of a unified picture of the forces of nature: gravity, the weak interaction, the electromagnetic interaction, and the strong interaction. Weinberg [34] and Salam [35] have made the first models of a unified weak and electromagnetic interaction theory. Attempts have been made at a unified picture of the weak, electromagnetic and strong interactions-more primitive than the Weinberg/Salam model, for the problem is more difficult, but still a beginning. The experimental information required to establish these unified pictures will almost certainly require still higher energies: several hundred GeV in the center-of-mass and again, I believe, in the e^+e^- system. If any of these unified pictures is correct at very high energies, then our only correct field theory, quantum electrondynamics, will necessarily have to break down, and I will have come full circle back to the first experiment I wanted to do as an independent researcher [36].

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Samuel C. C. Ting

SAMUEL CHAO CHUNG TING

I was born on 27 January 1936 in Ann Arbor, Michigan, the first of three children of Kuan Hai Ting, a professor of engineering, and Tsun-Ying Wang, a professor of psychology. My parents had hoped that I would be born in China, but as I was born prematurely while they were visiting the United States, by accident of birth I became an American citizen. Two months after my birth we returned to China. Owing to wartime conditions I did not have a traditional education until I was twelve. Nevertheless, my parents were always associated with universities, and I thus had the opportunity of meeting the many accomplished scholars who often visited us. Perhaps because of this early influence I have always had the desire to be associated with university life.

Since both my parents were working, I was brought up by my maternal grandmother. My maternal grandfather lost his life during the first Chinese Revolution. After that, at the age of thirty-three, my grandmother decided to go to school, became a teacher, and brought my mother up alone. When I was young I often heard stories from my mother and grandmother recalling the difficult lives they had during that turbulent period and the efforts they made to provide my mother with a good education. Both of them were daring, original, and determined people, and they have left an indelible impression on me.

When I was twenty years old I decided to return to the United States for a better education. My parents' friend, G. G. Brown, Dean of the School of Engineering, University of Michigan, told my parents I would be welcome to stay with him and his family. At that time I knew very little English and had no idea of the cost of living in the United States. In China, I had read that many American students go through college on their own resources. I informed my parents that I would do likewise. I arrived at the Detroit airport on 6 September 1956 with $100, which at the time seemed more than adequate. I was somewhat frightened, did not know anyone, and communication was difficult.

Since I depended on scholarships for my education, I had to work very hard to keep them. Somehow, I managed to obtain degrees in both mathematics and physics from the University of Michigan in three years, and completed my Ph.D. degree in physics under Drs. L. W. Jones and M. L. Perl in 1962.

I went to the European Organization for Nuclear Research (CERN) as a Ford Foundation Fellow. There I had the good fortune to work with Giuseppe Cocconi at the Proton Synchrotron, and I learned a lot of physics from him. He always had a simple way of viewing a complicated problem, did experiments with great care, and impressed me deeply.

In the spring of 1965 I returned to the United States to teach at Columbia University. In those years the Columbia Physics Department was a very stimulating place, and I had the opportunity of watching people such as L. Lederman, T. D. Lee, I. I. Rabi, M. Schwarts, J. Steinberger, C. S. Wu, and others. They all had their own individual style and extremely good taste in physics. I benefitted greatly from my short stay at Columbia.

In my second year at Columbia there was an experiment done at the Cambridge Electron Accelerator on electron-positron pair production by photon collision with a nuclear target. It seemed to show a violation ofquantum electrodynamics. I studied this experiment in detail and decided to duplicate it. I contacted G. Weber and W. Jentschke of the Deutsches Elektronen-Synchrotron (DESY) about the possibility of doing a pair production experiment at Hamburg. They were very enthusiastic and encouraged me to begin right away. In March 1966 I took leave from Columbia University to perform this experiment in Hamburg. Since that time I have devoted all my efforts to the physics of electron or muon pairs, investigating quantum electrodynamics, production and decay of photon-like particles, and searching for new particles which decay to electron or muon pairs. These types of experiments are characterized by the need for a high-intensity incident flux, for high rejection against a large number of unwanted background events, and at the same time the need for a detector with good mass resolution.

In order to search for new particles at a higher mass, I brought my group back to the United States in 1971 and started an experiment at Brookhaven National Laboratory. In the fall of 1974 we found evidence of a new, totally unpredicted, heavy particle-the J particle. Since then a whole family of new particles has been found.

In 1969 I joined the Physics Department of the Massachusetts Institute of Technology (MIT). In 1977, I was appointed as the first Thomas Dudley Cabot Institute Professor of Physics at MIT. In recent years it has been my privilege to be associated with M. Deutsch, A. G. Hill, H. Feshbach, W. Jentschke, H. Schopper and G. Weber. All have strongly supported me. In addition, I have enjoyed working with many very outstanding young physicists such as U. Becker, J. Burger, M. Chen, R. Marshall and A. J. S. Smith.

I married Dr. Susan Marks in 1985. We have one son, Christopher, born in 1986 and I have two daughters, Jeanne and Amy, from an earlier marriage.

I have been awarded the Ernest Orlando Lawrence Award from the US government in 1976 and the DeGasperi Award in Science from the Italian government in 1988. I have also received the Eringen Medal awarded by the Society of Engineering Science in 1977, the Golden Leopard Award for Excellence from the town of Taormina, Italy in 1988 and the Gold Medal for Science and Peace from the city of Brescia, Italy in 1988. I am a member of the National Academy of Sciences (US) and the American Physical Society, the Italian Physical Society and the European Physical Society. I have also been elected as a foreign member in Academia Sinica, the Pakistan Academy of Science and the Academy of Science of the USSR (now Russian

Academy of Science). I also hold Doctor Honoris Causa degrees from the University of Michigan, The Chinese University of Hong Kong, Columbia University, the University of Bologna, Moscow State University and the University of Science and Technology in China and am an honorary professor at Jiatong University in Shanghai, China.

THE DISCOVERY OF THE J PARTICLE:

A personal recollection

Nobel Lecture, 11 December, 1976

by

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and

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1. PHOTONS AND HEAVY PHOTONS

The study of the interaction of light with matter is one of the earliest known subjects in physics. An example of this can be found in the Mo Tsu [1] (the book of Master MO, Chou Dynasty, China, 4th century B.C.). In the 20th century, many fundamentally important discoveries in physics were made in connection with the study of light rays. The first Nobel Prize in Physics was awarded to W. C. Röntgen in 1901 for his discovery of X-rays.

In modern times, since the work of Dirac, we realized the possibility of the creation of electron-positron pairs by energetic light quanta. The work of W. E. Lamb and R. C. Retherford provided a critical step in the understanding of interactions between photons and electrons. The elegant formulation of quantum electrodynamics by S. Tomonaga, J. Schwinger and R. Feynman, F. J. Dyson, V. F. Weisskopf and others has led to a procedure for calculating observable effects of the proper electromagnetic field of an electron.

In the last decade, with the construction of giant electron accelerators, with the development of sophisticated detectors for distinguishing electrons from other particles, and finally with the building of electron-positron colliding beam storage rings, much has been learnt about the nature of very high energy light quanta in their interactions with elementary particles. The study of interactions between light and light-like particles (the so-called vector mesons, or heavy photons) eventually led to the discovery of a new family of elementary particles-the first of which is the J particle.

My first knowledge of the concept of light quanta and the role they play in atomic physics came from the classical book "The Atomic Spectra" by Herzberg [2], which I picked up in the summer of 1957 when I was working in New York as a summer student. Just before my graduation from college, I received as a Christmas gift from my father the English translation of the book "Quantum Electrodynamics" by Akhiezer and Berestetskii [2]. During my school years at Michigan I managed to go through this book in some detail and worked out some of the formulas in the book myself. Then, during my years as a junior faculty member at Columbia University, I read with great interest a paper by Drell [2], who pointed out the implications of various tests of quantum electrodynamics at short distances using high-energy electron accelerators. I did a theoretical calculation with Brodsky [3] on how to isolate a certain class of Feynman graphs from the muon production of three muons.

There are basically two ways of testing the theory of interactions between photons, electrons, and muons. The low-energy method, like the Lamb shift or (g-2) experiment, tests the theory to high accuracy at a long distance (or small momentum transfer). For example, the most recent experiment done at CERN by Picasso and collaborators [4] to measure the g-factor anomaly of the muon with a muon storage ring, obtained the result:

$(g-2)/2 = 0.001165922 + 0.000000009$ (an accuracy of 10 parts per million).

This result can be compared with calculations of quantum electrodynamics, including corrections from strong and weak interactions. The theoretical number is

$$(g-2)/2 = 0.001165921 \pm 0.000000010,$$

a most fantastic achievement of both experiment and theory.

The other way of testing quantum electrodynamics involves the study of reactions at large momentum transfers. Using the uncertainty principle $\Delta x\ \Delta p \approx h$, this type of experiment, though much less accurate, probes the validity of QED to a large momentum transfer or to a small distance. One such experiment, the process of e^+e^- production by multi-GeV photons in the Coulomb field of the nucleus, has both electromagnetic and strong interaction contributions to the e^+e^- yield. By properly choosing the kinematical conditions we can isolate the contributions from quantum electrodynamics alone and reduce the yield from strong interactions to a few percent level. The momentum transfer to the electron propagator is about 1 GeV; it is related to the effective mass of the e^+e^- pair. The yield of QED pairs is of the order α^3 ($\alpha = 1/137$). Because the yield is third order in α, to obtain a reasonable amount of events the experiment must be able to handle a high intensity of incident flux. A large acceptance detector is necessary not only to collect the events but also to average the steep angular dependence of the yields.

The effective mass of a pair of particles emitted from the same point is obtained by measuring the momentum of each of the particles p_1 and p_2, and the angles θ_1 and θ_2 between their paths and the incident beam direction, and by identifying the two particles simultaneously so that their masses m_1 and m_2 can be determined. The effective mass m of the pair is defined by:

$$m^2 = m_1^2 + m_2^2 + 2[E_1E_2 - p_1p_2 \cos(\theta_1 + \theta_2)],$$

where E_i = total energy of the particle.

A pair spectrometer has two arms, which measure simultaneously the momenta p_1 and p_2 of the particles and the angles θ_1 and θ_2. Owing to the immense size of the equipment required, the physical position of each arm is often preselected. This restricts θ_1 and θ_2 to a relatively narrow band of possible values. Different effective masses may be explored by varying the accepted momentum of the particles p_1 and p_2.

When the two particles are uncorrelated, the distribution of m is normally a smooth function. A 'narrow' resonance will exhibit a sharp peak above this smooth distribution, while a 'wide' resonance will produce a broader bump.

The identification of particles from the spectrometer is done by

i) measuring the charge and momentum of the particle from its trajectory in a magnetic field;

ii) determining for a given trajectory, or a given momentum, the mass of the particle by measuring its velocity and using the relation p = m . v.

The measurement of velocity can be done with Čerenkov counters using the cerenkov effect. For electrons, their additional property of having only. electromagnetic interactions can be used. When an electron enters a dense piece of lead, it loses all its energy by a cascading process which releases photons. The amount of light emitted from a lead-lucite sandwich shower counter (or a lead-glass counter) is thus proportional to the energy of the electron.

In October, 1965, I was invited by W. Jentschke, then Director of the Deutsches-Elektronen Synchrotron (DESY) in Hamburg, Germany, to perform my first experiment on e^+e^- production [5]. The detector we used is shown in Figs. la and lb. It has the following properties that are essential to this type of experiment: i) it can USC an incident photon flux of ~ 10^{11}/s, with a duty cycle of 2 - 3 % ; ii) the acceptance is very large and is not limited by edges of the magnets or by shielding, being defined by scintillation counters alone; iii) all counters are located such that their surfaces are not directly exposed to the target; iv) to reject the hadron pairs, the cerenkov counters are separated by magnets so that knock-on electrons from the pions interacting with gas radiators in the first pair of counters LC, RC are swept away by the magnet MA and do not enter the second pair of counters HL, HR. The low-energy knock-on electrons from HL, HR are rejected by shower counters.

The large number of Čerenkov counters and shower counters enables us to perform redundant checks on hadron rejection. Since each Čerenkov counter is 100% efficient on electrons and not efficient on hadrons, the observation that:

the yield of e^+e^- from 3 cerenkov counters =

the yield of e^+e^- from 4 cerenkov counters,

ensures that we are measuring pure e^+e^- pairs. The combined rejection is $>>10^8$.

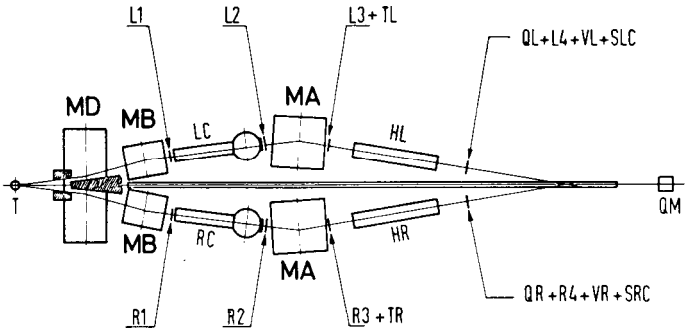


Fig. la. Plan view of the spectrometer. MD. MA. RIB are dipole magnets; L1, ... L4. and R1, ... R4, are triggering counters: LC, RC, and HL, HR are large-aperture threshold Čerenkov counters; SLC. SRC are shower counters; and TL, QL, VL, and TR. QR, VR are hodoscopcs. QM is a quantameter.

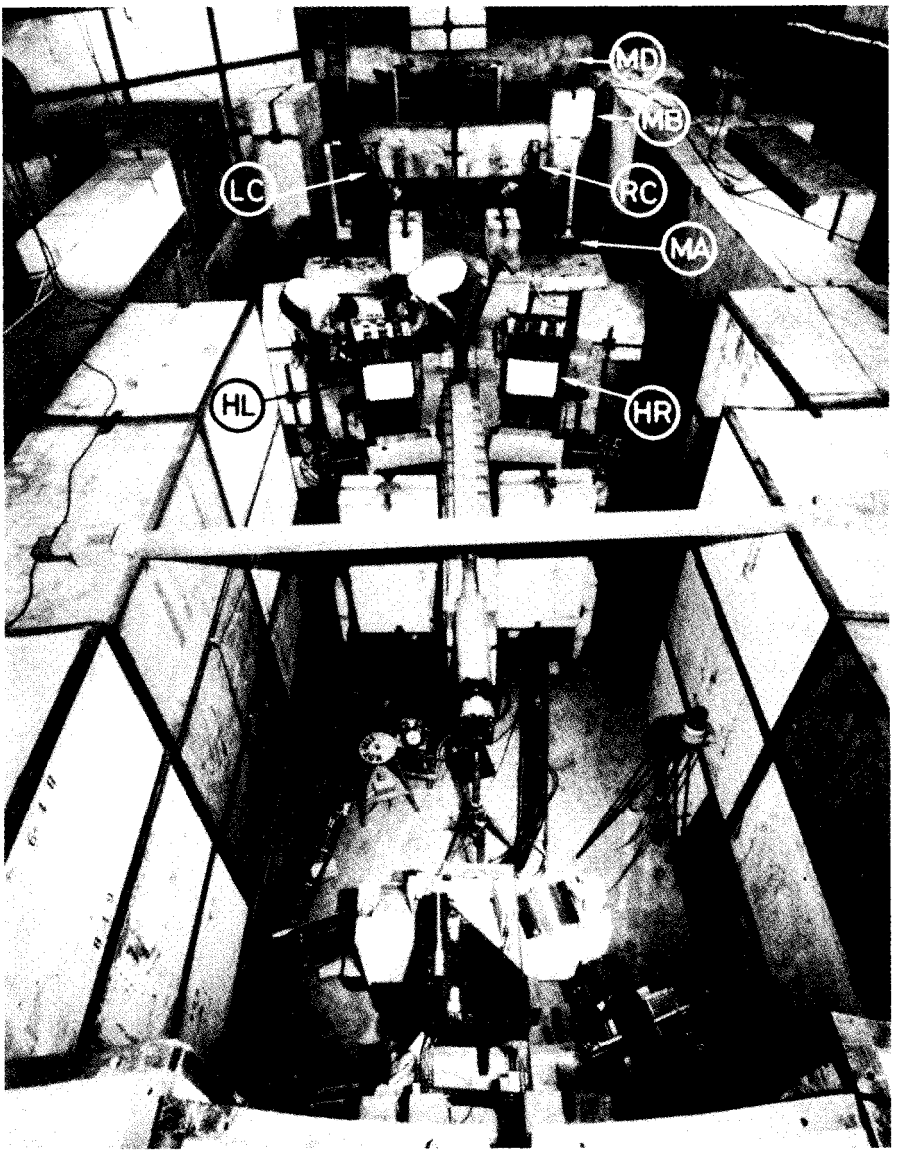


Fig. 1b. Over-all view of my first experiment at DESY. The position of LC, RC, HL, HR, MA, and MD are all marked. The physicist on the left is Dr. A. J. S. Smith; on the right is Dr. C. L. Jordan.

After we had finished this experiment, which showed that quantum electrodynamics correctly describer the pair production process to a distance of $\approx 10^{-14}$ cm, we tuned the spectrometer magnets so that the maximum pair mass acceptance is centred near m $\approx$ 750 MeV. We observed a large increase in the e^+e^- yield and an apparent violation of QED. This deviation is caused by an enhancement of the strong interaction contribution to the e^+e^- yield where the incident photon produces a massive photon-like particle, the ρ meson, which decays into e^+e^- [6-8] with a decay probability of order α^2.

In order to show that this is indeed the case, we made another measurement at a larger e^+e^- opening angle and observed an even larger deviation from QED. This is to be expected since the QED process decreases faster than the strong interaction process when we increase the opening angle of the e^+e^- pair.

The observation of $\rho \to e^+e^-$ decay started a series of experiments by my group on this subject [9-12). Basically the heavy photons $\rho,, \varphi$, are resonance states of $\pi^+\pi^-(\rho), \pi^+\pi^-\pi^0(\omega), K^+K^-$ or $\pi^+\pi^-\pi^0(\varphi)$ with a rather short lifetime of typically $\approx 10^{-23}$ - 10^{-24} s. The widths of these particles are $\Gamma_\rho \approx$ ≈ 100 MeV, $\Gamma_\omega \sim 10$ MeV, and $\Gamma_\varphi \approx 5$ MeV. They are unique in that they all have quantum numbers J (spin) = 1, C (charge conjugation) = - 1, P (parity) = 1. Thus they are exactly like an ordinary light-ray except for their heavy mass. The mass of p is $m_\rho \simeq 760$ MeV, and $m_\omega \simeq 783$ MeV; $m_\varphi \approx 1019.5$ MeV.

The production of heavy photons by photons on nucleon and nuclear targets shows that it is a diffraction process very much like the classical scattering of light from a black disk. The experiments on photoproduction of heavy photons and observation of their e^+e^- decay measure the coupling strength between each heavy photon and the photon. 'The interference between the e^+e^- final state from heavy photon decays and e^+e^- from QED measures the production amplitude of the heavy photon. The interference between these amplitudes can be viewed classically as a simple two-slit experiment, where in front of one of the *slits* WC placed a thin piece of glass (corresponding to $\gamma\gamma \to \rho \to \gamma \to$ $\to e^+e^-$) thus disturbing the interference pattern. The QED pairs alone would correspond to passing of light without the glass in front of the slit. The interference between $\rho(2\pi) \to e^+ + e^-$ and $\omega(3\pi) \to e^+ + e^-$ and the interference between $\rho(2\pi) \to 2\pi$ and $\omega(3\pi) \to 2\pi$ are measurements of strength of isospin non-conservation in electromagnetic interactions [13].

In the course of these experiments;, since the width of ω is ~ 10 MeV and φ is ~ 5 MeV, we developed a detector with a mass resolution of -3 MeV.

Some of the measurements have low. event rates. In one particular experiment where we studied the e^+e^- mass spectra in the mass region above the ρ and ω mesons, the yield of e^+e^- pairs was about one event per day, with the full intensity of the accelerator. This implies that for about half a year the whole laboratory was working on this experiment alone. The rate of one event per day also implies that often there were no events for 2-3 days, and then on other days we had 2 - 3 events. It was during the course of this experiment that we developed the tradition of checking all voltages manually every 30 minutes, and calibrating the spectrometer by measuring the QED yields every 24 hours. To ensure that the detector was stable, we also established the practice of having physicists on shift, even when the accelerator was closed down for maintenance, and never switched off any power supplies. The net effect of this is that for many years our counting room has had a different grounding system from that of the rest of the laboratory. The Control Room for this series of experiments is shown in Fig. 2.

Some of the quantitative results from the above experiments may be explained if we assume that there are three kinds of fundamental building blocks

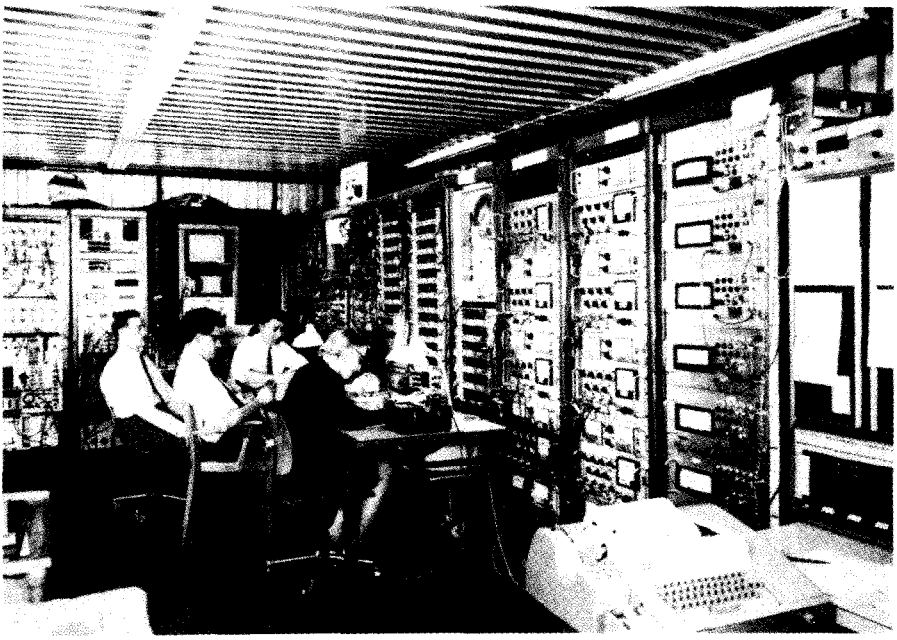


Fig. 2. Earlier Control Room at DESY. The three other people in the picture are Miss I. Schulz, Dr. U. Becker and Dr. M. Rohde. All have worked with me during the last 10 years.

in the world, known as quarks, which combine to form various elementary particles. The interactions between photons, heavy photons, and nuclear matter are results of interactions of the various quarks.

Sakurai [14] was the first to propose that the electromagnetic interaction of elementary particles may be viewed as through the heavy photon (vector meson) intermediate states.

2. NEW PARTICLES

After many years of work, we have learnt how to handle a high intensity beam of $\sim 10^{11}\gamma/s$ with a $2-3\%$ duty cycle, at the same time using a detector that has a large mass acceptance, a good mass resolution of $\Delta M \approx 5$ MeV, and the ability to distinguish $\pi\pi$ from e^+e^- by a factor of $> >10^8$.

We can now ask a simple question: How many heavy photons exist? and what are their properties? It is inconceivable to me that there should be only three of them, and all with a mass around 1 GeV. To answer these questions, I started a series of discussions among members of the group on how to proceed. I finally decided to first perform a large-scale experiment at the 30 GeV proton accelerator at Brookhaven National Laboratory in 1971, to search for more heavy photons by detecting their e^+e^- decay modes up to a mass (m) of 5 GeV. Figure 3 shows the photocopy of one page of the proposal; it gives some of the reasons I presented, in the spring of 1972, for performing an e^+e^- experiment in a proton beam rather than in a photon beam, or at the DESY colliding beam accelerator then being constructed.

The best way to search for vector mesons is through production experiments of the type $p + p \rightarrow V^{o} + X$, $V^{o} \rightarrow e^{+}e^{-}$. The reasons are:

(a) The V^{o} are produced via strong interactions, thus a high production cross section.

(b) One can use a high intensity, high duty cycle extracted beam.

(c) An $e^{+}e^{-}$ enhancement limits the quantum number to 1^{-}, thus enabling us to avoid measurements of angular distribution of decay products.

Contrary to popular belief, the $e^{+}e^{-}$ storage ring is not the best place to look for vector mesons. In the $e^{+}e^{-}$ storage ring. the energy is well-defined. A systematic search for heavier mesons requires a continuous variation and monitoring of the energy of the two colliding beams—a difficult task requiring almost infinite machine time. Storage ring is best suited to perform detailed studies of vector meson parameters once they have been found.

Fig. 3. Page 4 of proposal 598 submitted to Brookhaven National Laboratory early in 1972 and approved in May of the same year, giving some of the reasons for performing this experiment in a slow extracted proton beam.

Historically, to my knowledge, the Zichichi Group was the first one to use hadron-hadron collisions to study $e^{+}e^{-}$ yields from proton accelerators [15]. This group was the first to develop the earlier shower development method so as to greatly increase the e/x rejection [16]. In later years the Lederman Group made a study of the $\mu^{+}\mu^{-}$ yield from proton nuclei collisions [17]. Some of the early theoretical work was done by Preparata [18], Drell and Yan [19], and others.

Let me now go to the J-particle experiment [20 - 22].

I. To perform a high-sensitivity experiment, detecting narrow-width particles over a wide mass range, we make the following four observations.

i) Since the $e^{+}e^{-}$ come from electromagnetic processes, at large mass *m*, the yield of $e^{+}e^{-}$ is lower than that of hadron pairs ($\pi^{+}\pi^{-}, K^{+}K^{-}, \bar{p}p, K^{+}\bar{p}$, etc.) by a factor $< < 10^{-6}$.

ii) Thus, to obtain sufficient $e^{+}e^{-}$ rates, a detector must be able to stand a high flux of protons, typically of 10^{11} - 10^{12} protons/s, and

iii) it must be able to reject hadron pairs by a factor of $> > 10^{8}$.

iv) For a detector with finite acceptance, there is always the question of where is the best place to install it to look for new particles. A *priori we* do not know what to do. But we do know that in reactions where ordinary hadrons are produced, the yield is maximum when they are produced at rest in the centre-of-mass system [23]. If we further restrict ourselves to

the 90° e^+e^- decay of new particles, then we quickly arrive at the conclusion that the decayed e^+ or e^- emerge at an angle of 14.6 in the laboratory system for an incident proton energy of 28.3 GeV, independent of the mass of the decaying particle.

II. Figure 4 shows the layout of the slow-extracted intense proton beam from the Alternating Gradient Synchrotron (AGS) at Brookhaven, during the period 1973-1974. Our experiment (No. 598) was located in a specially designed beam line (the A-line). To design a clean beam with small spot sizes, I remembered having a conversation with Dr. A. N. Diddens of CERN who had used a slow-extracted beam at the CERN Proton Synchrotron. He advised me to focus the beam with magnets alone without using collimators.

The incident beam of intensity up to 2×10^{12} protons per pulse was focused to a spot size of 3×6 mm^2. The position of the beam was monitored by a closed-circuit TV. The stability and the intensity of the beam were monitored by a secondary emission counter and six arrays of scintillation counter telescopes, located at an angle of 75° with respect to the beam, and buried behind 12 feet of concrete shielding. Daily calibrations were made of the secondary emission counter with the Al and C foils.

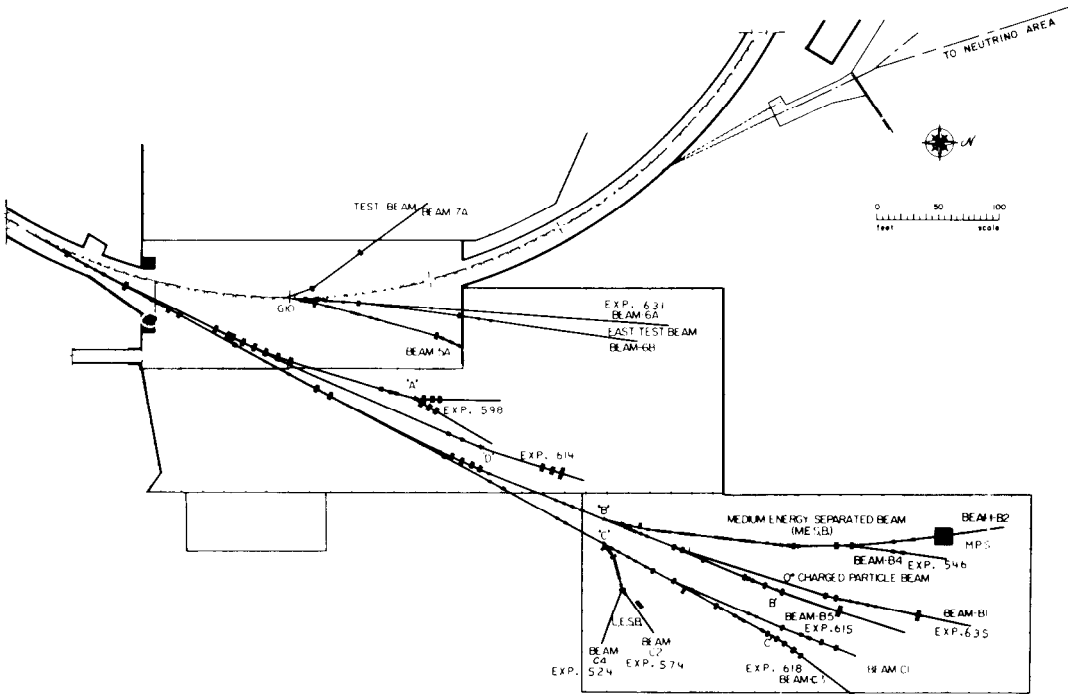


Fig. 4. The AGS East experimental area. The MIT experiment is No. 598 at the end of Station A. Experiment 614 is that of Prof. M. Schwartz (see Ref. 22).

III. From our early experience at DESY, we felt the best way to build an electron-pair detector that could handle high intensities, and at the same time have a large mass acceptance and a good mass resolution, is to design a large double-arm spectrometer and to locate most of the detectors behind the magnets so that they would not "view" the target directly. To simplify analysis and to obtain better mass resolution, we used the "p,θ independent" concept in which the magnets bend the particles vertically to measure their momentum, while the production angles are measured in the horizontal plane. Figures 5a and 5b show the plan and side views of the spectrometer and detectors.

The main features of the spectrometer are the following:

1) *The target:* The target consists of nine pieces of 1.78 mm thick beryllium, each separated by 7.3 cm so that particles produced in one piece and accepted by the spectrometer do not pass through the next piece. This arrangement also helps us to reject pairs of accidentals by requiring two tracks to come from the same origin.

2) *The magnet system:* The bending powers of the dipole magnets M_0, M_1, M_2, are such that none of the counters sees the target directly. The field of the magnets in their final location was measured with a three-dimensional Hall probe at a total of 10^5 points.

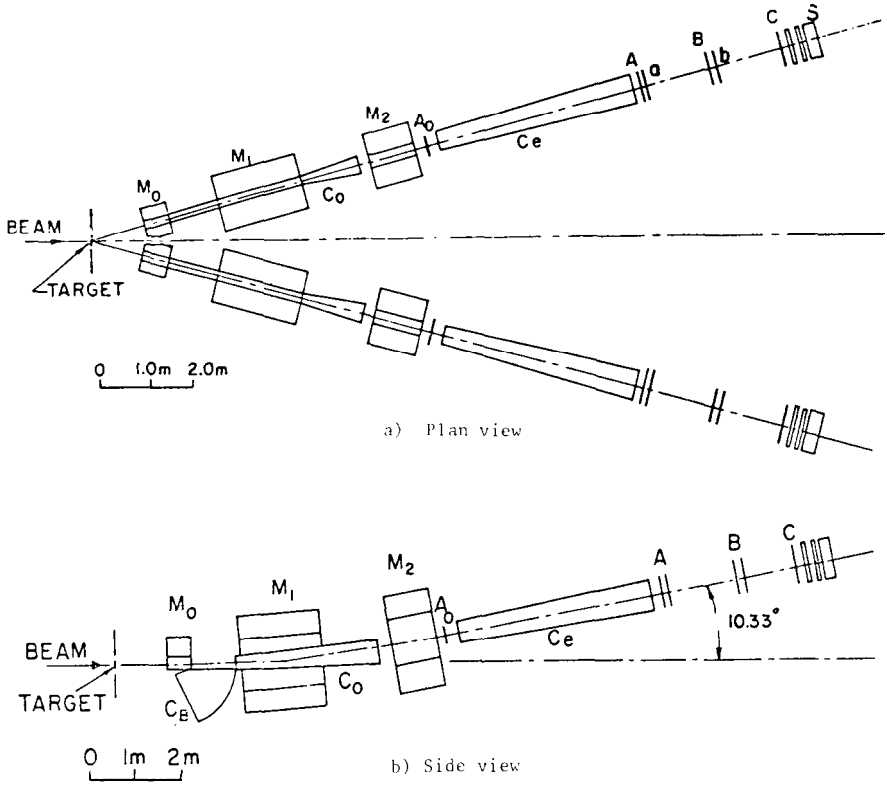


Fig. 5. Schematic diagram of the experimental set-up for the double-arm spectrometer used in our discovery of the J particle. M_0, M_1, and M_2 are dipole magnets; A_0, A, B, and C are 8000-wire proportional chambers; a and b are each 8 × 8 hodoscopes; S designates three banks of lead-glass and shower counters; C_B, C_0, and C_e are gas Čerenkov counters.

3) The chambers: A_0, A, B, and C are multiwire proportional chambers. They consist of more than 8000 very fine, 20μm thick, gold-plated wires, 2 mm apart, each with its own amplifier and encoding system. The wire arrangement is shown in Fig. 6. The 11 planes all have different wire orientation. In each of the last three chambers the wires are rotated 60" with respect to each other, so that for a given hit, the numbers of wires add up LO a constant-a useful feature for sorting out multitracks and rejecting soft neutrons and ;'-rays which do not fire all planes. We developed special gas mixtures to operate the chambers at low voltage and high radiation environment. To help improve the timing resolution, two planes of thin (1.6 mm thick hodoscopes (8 x 8) are situated behind each of the chambers A and B. These chambers are able to operate at a rate of -20 MHz and are also able to sort out as many as eight particles simultaneously in each arm.

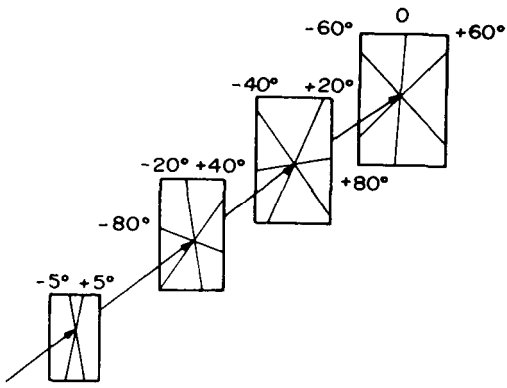


Fig. 6. Relative orientation of the planes of wires in the proportional chambers.

It is essential that all 8000 wires should function properly because to repair a single wire would involve removing close to a thousand tons of concrete.

These chambers and the magnets yield a mass resolution of ± MeV and a mass acceptance of 2 GeV at each magnet current setting. The good mass resolution makes it possible to identify a very narrow resonance. The large mass acceptance is very important when searching over a large mass region for narrow resonances.

4) *Čerenkov counters and shower counters :* The Čerenkov counters marked C_0, and C_e, together with the lead-glass and shower counters marked S, enable one to have a rejection against hadron pairs by a factor of $> > 1 \times 10^8$.

The Čerenkov counter in the magnet (C,, see Fig. 7a) has a large spherical mirror with a diameter of 1 m. This is followed by another Čerenkov counter behind the second magnet with an elliptical mirror of dimensions 1.5×1.0 m^2. The Čerenkov counters are filled with hydrogen gas so that the knock-on electrons are reduced to the minimum. As in our earlier DESY experiments, the separation of the two counters by strong magnetic fields ensures that the

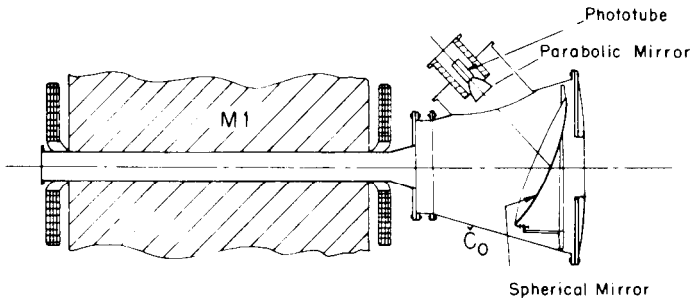


Fig. 7a.. Plan view of the C_0 counter shown in its location in the experiment

small number of knock-on electrons produced in the first counter is swept away and does not enter into the second counter.

To reduce multiple scattering and photon conversion, the material in the beam is reduced to a minimum. The front and rear windows of C_0 are 126 μm and 250 μm thick, respectively. To avoid large-angle Čerenkov light reflection, the mirrors of C_0 and C_e are made of 3 mm thick black lucite, aluminized on the forward (concave) surface only. The mirrors in the experiment were made at the Precision Optical Workshop at CERN. We measured the curvature of the mirrors with a laser gun, and out of the many mirrors that were made a total of24 were used in this experiment (4 in C_0, 4 in C_e, 16 in C_B).

The counters are painted black inside so that only the Čerenkov light from electrons along the beam trajectory will be focused onto the photomultiplier cathode. Special high-gain, high-efficiency phototubes of the type RCA C31000M are used, so that when we fill the counter with He gas as radiator (where we expect, on the average, 2-3 photoelectrons) we are able to locate the single photoelectron peak (see Fig. 7b).

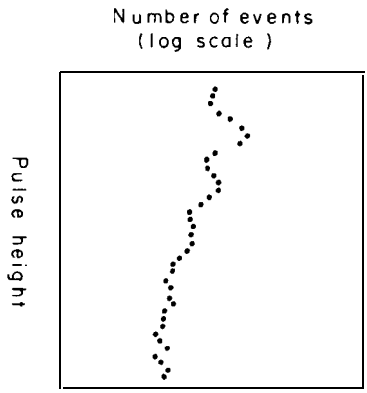


Fig. 7b. Pulse-height spectrum from the phototube (RCA C31000M) of the C_0 Čerenkov counter with He as radiator. Clearly visible are the one, two, and three photoelectron peaks.

The counter C_0 is very close to the target, which is a high-radiation-level area. To reduce random accidentals and dead-time, the excitation voltage on the photomultiplier has to be kept as low as possible. Yet we must still ensure that the counter is efficient. We have to avoid mistakingly setting the

voltage so low that the counter is only efficient on an e^+e^- pair from $\pi^0 \to \gamma + + e^+ + e^-$, which may enter the counter. When C_0 is filled with hydrogen gas, a single electron will yield about eight photoelectrons, a pair will yield about sixteen. The knowledge of the location of one photoelectron peak enables us to distinguish between these two cases. The counters are all calibrated in a test beam to make sure they are 100% efficient in the whole phase space.

At the end of each arm there are two orthogonal banks of lead-glass counters of three radiation lengths each, the first containing twelve elements, the second thirteen, followed by one horizontal bank of seven lead-lucite shower counters, each ten radiation lengths thick, to further reject hadrons from electrons. The subdividing of the lead-glass and lead-lucitc counters into ~ 100 cells also enables us to identify the electron trajectory from spurious tracks.

Figure 8 shows an over-all view of the detector with the roof removed. Figure 9 shows the end section of one arm of the detector, showing part of the Cerenkov counter C_0, the proportional chambers, and counters.

Fig. 8. Over-all view of the detector.

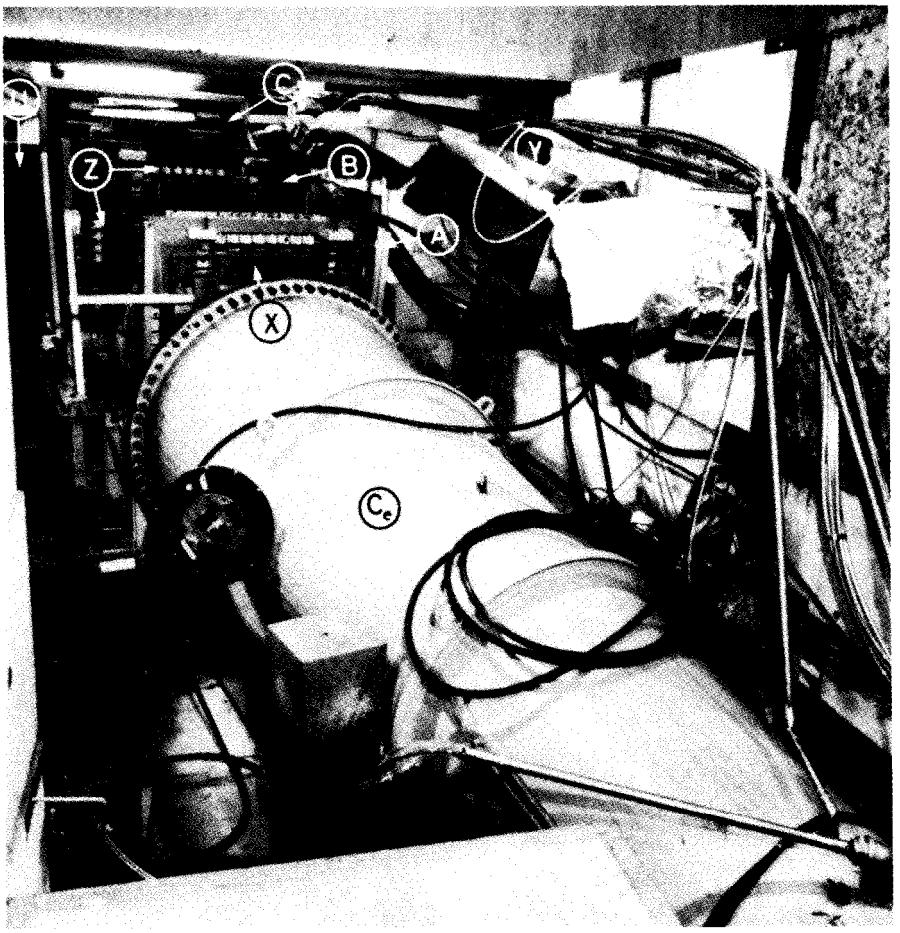
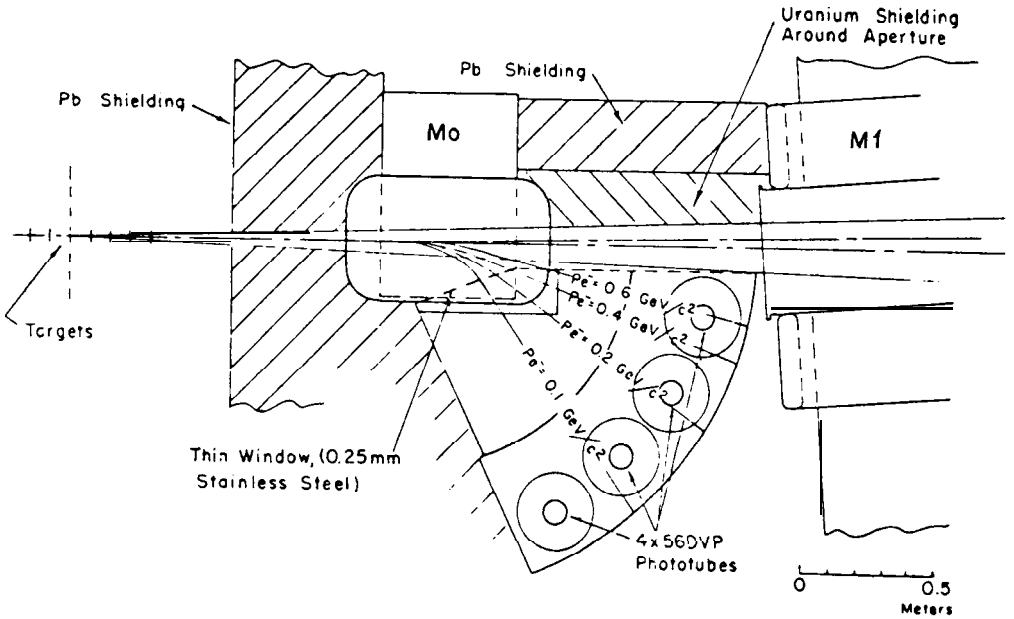
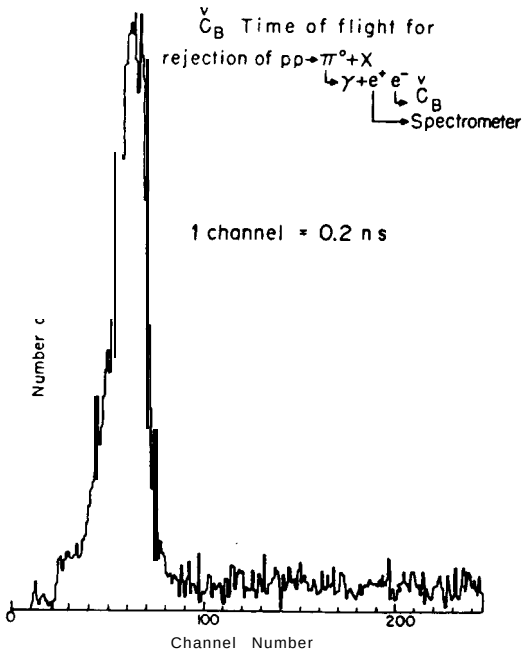


Fig. 9. End view of one arm, showing part of the Čerenkov counter C_e, the chambers A, B, C, with part of the 8000 amplifiers X, cables Y, and hodoscopes Z. The lead-glass counter is at the end of chamber U.

5) A pure electron beam, for calibration: To obtain a high rejection against hadron pairs and to ensure that the detectors are 100% efficient for electrons, we need to calibrate the detectors with a clean electron beam. In an electron accelerator such as DESY we can easily produce a clean electron beam with an energetic photon beam hitting a high-Z target thus creating 0" e^+e^- pairs. In a proton accelerator the best way to create a clean electron beam is to use the reaction $\pi^0 \rightarrow \gamma + e^+ + e^-$, tagging the e^+ in coincidence with the e^-. To accomplish this, the very directional Čerenkov counter C_B is placed close to the target and below a specially constructed magnet M_0 (Fig. 10a). This counter also is painted black inside; it is sensitive to electrons above 10 MeV/c and rejects pions below 2.7 GeV/c. The coincidence between C_B and C_0, C_e, the shower counter, and the hodoscopes, indicates the detection of an e^+e^- pair from the process $\pi^0 \rightarrow \gamma + e^+ e^+ e^-$. A typical plot of the relative timing of this coincidence is shown in Fig. 10b. We can trigger on C_B and provide a pure electron beam to calibrate C_0, C_e, the lead-glass and shower counters.



a. Side view of magnet M_0 which bends the various low-energy trajectories (P_e) of $e^{\pm}$ into C_B.



b. The relative timing between an electron pulse from C_B and a positron trigger from the main spectrometer arm or vice versa.

Fig. 10. Measurement of e^+e^- from $\pi^0 \to \gamma + e^+ + e^-$ decay.

This is another way of setting the voltage of the C_0 counters, since the coincidence between C_e and C_B will ensure that the counter is efficient for a single electron and not a zero degree pair.

6) Shielding: As shown in Fig. 8 the detector is large, and with 10^{12} protons incident on a 10% collision length target there are ~ 10^{12} particles generated around the experimental area. To shield the detector and the physicists, we constructed scaled-down wooden models of the concrete blocks, and soon realized that we would need more shielding than was available at Brookhaven. This problem was solved by obtaining all the shielding blocks from the Cambridge Electron Accelerator, which had just closed down. The total shielding used is approximately a) 10,000 tons of concrete, b) 100 tons of lead, c) 5 tons of uranium, d) 5 tons of soap-placed on top of C_0, between M_1 and M_2, and around the front of C_e to stop soft neutrons. Even with this amount of shielding, the radiation level in the target area, one hour after the shutting down of the proton beam, is still 5 röntgen/hour, a most dangerous level.

During the construction of our spectrometers, and indeed during the entire experiment, I encountered much criticism. The problem was that in order to gain a good mass resolution it was necessary to build a spectrometer that was very expensive. One eminent physicist made the remark that this type of spectrometer is only good for looking for narrow resonances-and there are no narrow resonances. Nevertheless, since I usually do not have much confidence in theoretical arguments, we decided to proceed with our original design.

In April 1974, we finished the set-up of the experiment and started bringing an intense-proton beam into the area. We soon found that the radiation level in our counting room was 0.2 röntgen/hour. This implied that our physicists would receive the maximum allowable yearly dose in 24 hours ! We searched very hard, for a period of two to three weeks, looking for the reason, and became extremely worried whether we could proceed with the experiment at all.

One day, Dr. U. Becker, who has been working with me since 1966, was walking around with a Geiger counter when he suddenly noticed that most of the radiation was coming from one particular place in the mountains of shielding. Upon close investigation we found out that even though we had 10,000 tons of concrete shielding blocks, the most important region-the top of the beam stopper-was not shielded at all! After this correction, radiation levels went down to a safe level and we were able to proceed with the experiment.

From April to August, we did the routine tune-ups and found the detectors performing as designed. We were able to use 10^{12} protons per second. The small pair spectrometer also functioned properly and enabled us to calibrate the detector with a pure electron beam.

IV. Owing to its complexity, the detector required six physicists to operate it. Before taking data, approximately 100 hours were spent ensuring that all the detectors were close to 100% efficient. I list some examples:

i) The efficiency of the Cerenkov counters was measured over the whole phase space, and voltages set so that they were efficient everywhere. A typical result for C_e, is shown in Fig. 1 la.

ii) The voltages and the response of all the lead-glass and shower counters were calibrated to ensure that the response did not change with time.

iii) The efficiency of the hodoscopes at the far end, furthest away from the photomultiplier tube, was checked.

iv) The timing of the hodoscopes was also checked to ensure that signals from each counter generated by particles produced at the target arrived simultaneously. During the experiment, the time-of-flight of each of the hodoscopes and the Čerenkov counters, the pulse heights of the Čerenkov counters and of the lead-glass and shower counters, the single rates of all the counters together with the wire chamber signals, were recorded and continuously displayed on a storage/display scope.

v) To ensure that the proportional wire chambers were efficient over their whole area, a small test counter was placed behind the chambers at various positions over the chambers' area, and voltage excitation curves were made at those positions. A typical set of curves for all the planes is shown in Fig. 11 b.

vi) To check the timing between the two arms, two tests were performed. Firstly, the test counter was physically moved from one arm to the other

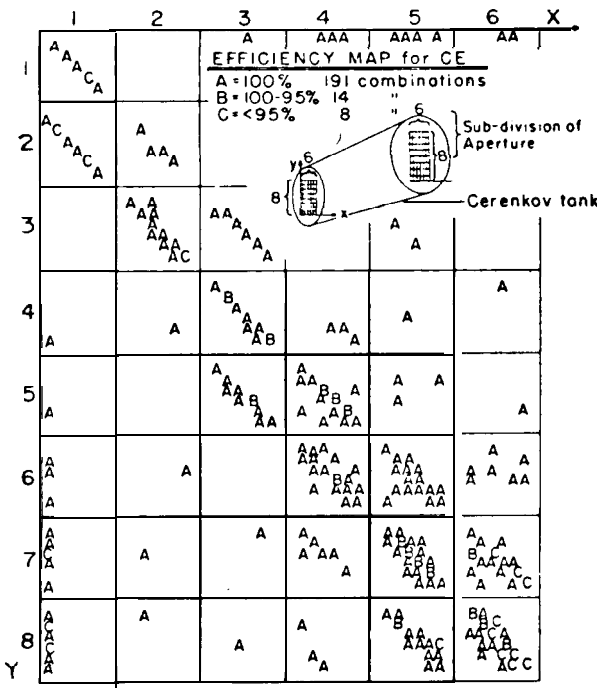


Fig. 11 a. Mapping of the efficiency of the C_e, counter over its whole phase space. The letters on the plot refer to efficiencies measured for trajectories between the corresponding points marked on the grid at each end of the counter.

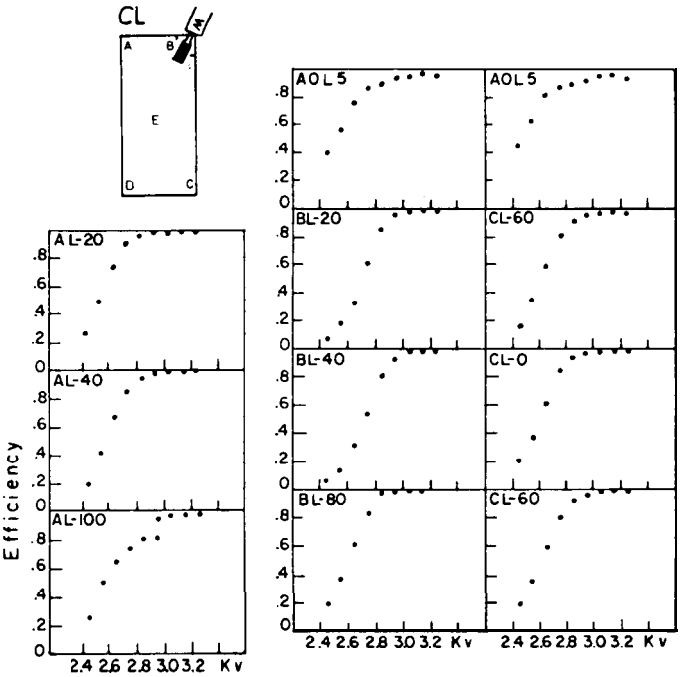


Fig. 11 b. Efficiency of all the wire plant-s as a function of the applied voltage. The measurements were done by placing a small test counter W in various positions. marked A, B, C, D, E. in every chamber.

so that the relative timing could be compared. Secondly, the e^+e^- yield was measured at low mass, $m_{ee} < 2$ GeV/c^2, where there is an abundance of genuine e^+e^- pairs.

In the early summer of 1974 we took some data in the high mass region of 4-5 GeV. However, analysis of the data showed very few electron-positron pairs.

By the end of August we tuned the magnets to accept an effective mass of 2.5-4.0 GeV. Immediately we saw clean, real, electron pairs.

But most surprising of all is that most of the e^+e^- pairs peaked narrowly at 3.1 GeV (Fig. 12a). A more detailed analysis shows that the width is less than 5 MeV! (Fig. 12b).

Throughout the years, I have established certain practices in the group with regard to experimental checks on our data and on the data analysis. I list a few examples:

i) To make sure the peak we observed was a real effect and not due to instrumentation bias or read-out error of the computer, we took another set of data at a lower magnet current. This has the effect of moving the particles into different parts of the detector. The fact that the peak remained fixed at 3.1 GeV (Fig. 12a) showed right away that a real particle had been discovered.

ii) We used two completely different sets of programs to ensure that the analysis was correct. This means that two independent groups of physi-

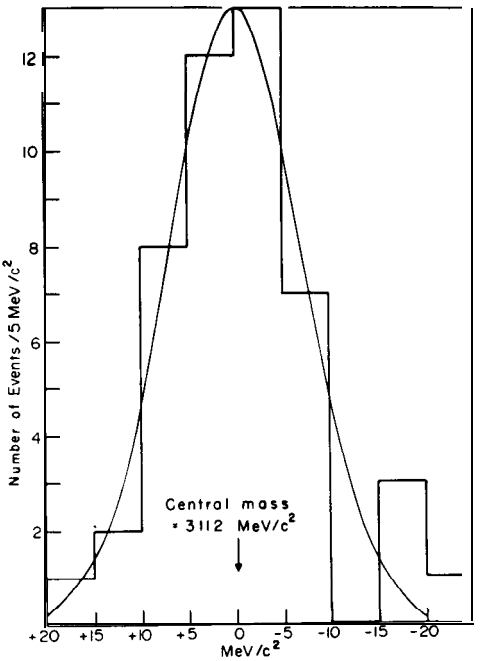


Fig. 12b. The measurement of the width of the J. The width is shown to be less than 5 MeV.

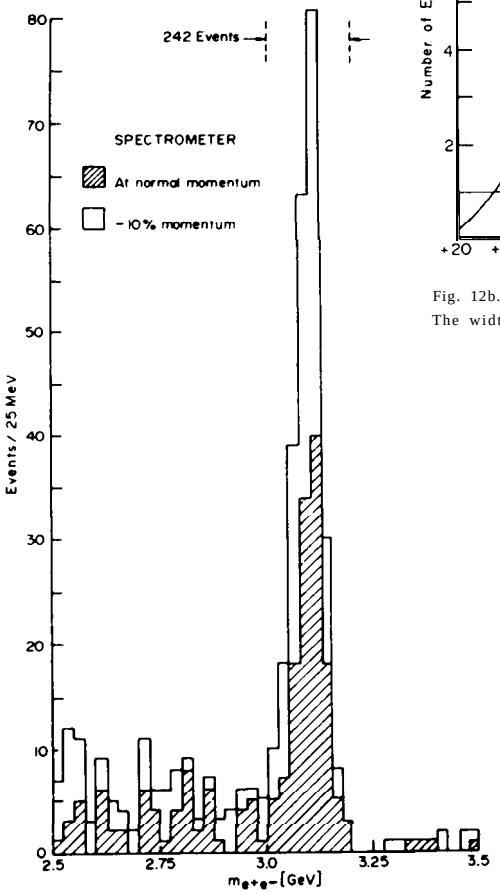


Fig. 12a Mass spectrum for events in the mass range $2.5 < m_{ee} < 3.5$ GeV/c. The shaded events correspond to those taken at the normal magnet setting, while the unshaded ones correspond to the spectrometer magnet setting at - 10% lower than normal value.

cists analysed the data, starting from the reduction of raw data tapes, to form their own data summary tapes, and then performed two sets of Monte Carlo acceptance calculations, two sets of event reconstruction, two sets of data corrections, and finally, two sets of results which must agree with each other. Although this procedure uses twice as much computer time, it provides greater confidence in our results after the two independent approaches have reached the same conclusions.

iii) To understand the nature of various second-order background corrections, we made the following special measurements:
   a) To check the background from pile-up in the lead-glass and shower counters, different runs were made with different voltage settings on the counters. No effect was observed in the yield.
   b) To check the background from scattering from the sides of the magnets, cuts were made in the data to reduce the effective aperture. No significant reduction in the yield was found.
   c) To check the read-out system of the chambers and the triggering system of the hodoscopes, runs were made with a few planes of chambers deleted and with sections of the hodoscopes omitted from the trigger. No unexpected effect was observed on the yield.
   d) Since the true event rate is proportional to incident beam intensity and the accidental backgrounds from the two arms are proportional to the square of the incident intensity, a sensitive way to check the size of the background is to run the experiment again with different intensities. This was done and the background contribution in the peak was found to be unnoticeable.

iv) To understand the nature of production properties of the new peak, we increased the target thickness by a factor of two. The yield increased by a factor of two, not by four.

These and many other checks convinced us that we had observed a real massive particle.

We discussed the name of the new particle for some time. Someone pointed out to me that the really exciting stable particles are designated by Roman characters-like the postulated W^0, the intermediate vector boson, the Z^0, etc.-whereas the "classical" particles have Greek designations like ρ, ω, etc. This, combined with the fact that our work in the last decade had been concentrated on the electromagnetic current $j_\mu(x)$, gave us the idea to call this particle the J particle.

V. I was considering announcing our results during the retirement ceremony for V. F. Weisskopf, who had helped us a great deal during the course of many of our experiments. This ceremony was to be held on 17 and 18 October 1974. I postponed the announcement. for two reasons. First, there were speculations on high mass e^+e^- pair production from proton-proton collisions as coming from a two-step process : $p+N \to \pi+\ldots$, where the pion undergoes a second collision $\pi+N \to e^+ + e^- + \ldots$. This could be checked by a measurement based on target thickness. The yield from a two-step process would

increase quadratically with target thickness, whereas for a one-step process the yield increases linearly. This was quickly done, as described in point (iv) above.

Most important, we realized that there were earlier Brookhaven measurements [24] of direct production of muons and pions in nucleon-nucleon collisions which gave the μ/π ratio as 10^{-4}, a mysterious ratio that seemed not to change from 2000 GeV at the ISR down to 30 GeV. This value was an order of magnitude larger than theoretically expected in terms of the three known vector mesons, p, ω, φ, which at that time were the only possible "intermediaries" between the strong and electromagnetic interactions. We then added the J meson to the three and found that the linear combination of the four vector mesons could not explain the μ^-/π^- ratio either. This I took as an indication that something exciting might be just around the corner, so I decided that we should make a direct measurement of this number. Since we could not measure the μ/π ratio with our spectrometer, we decided to look into the possibility of investigating the e^-/x^- ratio.

We began various test runs to understand the problems involved in doing the e/π experiment. The most important tests were runs of different e- momenta as a function of incident proton intensities to check the single-arm backgrounds and the data-recording capability of the computer.

On Thursday, 7 November, we made a major change in the spectrometer (see Fig. 13) to start the new experiment to search for more particles. We began by measuring the mysterious e/π ourselves. We changed the electronic logic and the target, and reduced the incident proton beam intensity by almost two orders of magnitude. To identify the e^- background due to the decay of π^0 mesons, we inserted thin aluminium converters in front of the spectrometer to increase the $\gamma \to e^+ + e^-$ conversion. This, together with the C_B counter which measures the $\pi \to \gamma + e^+ + e^-$ directly, enabled us to control the major e^- background contribution.

We followed the e/x measurements with another change in the spectrometer by installing new high-pressure Čerenkov counters and systematically measuring hadron pairs ($K^+K^-, \pi^+\pi^-, \bar{p}p$, etc.) to find out how many other particles exist that do not decay into e^+e^- but into hadrons. But, after a long search, none was found.

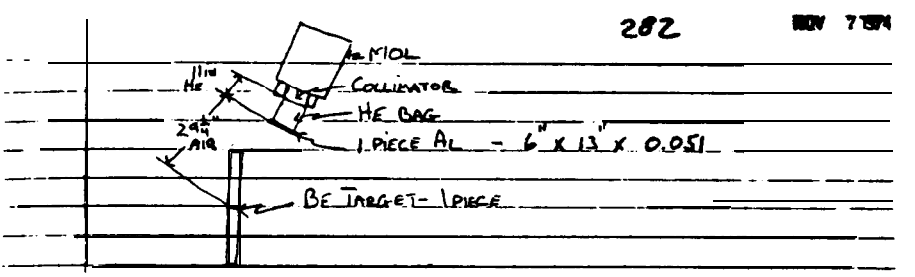
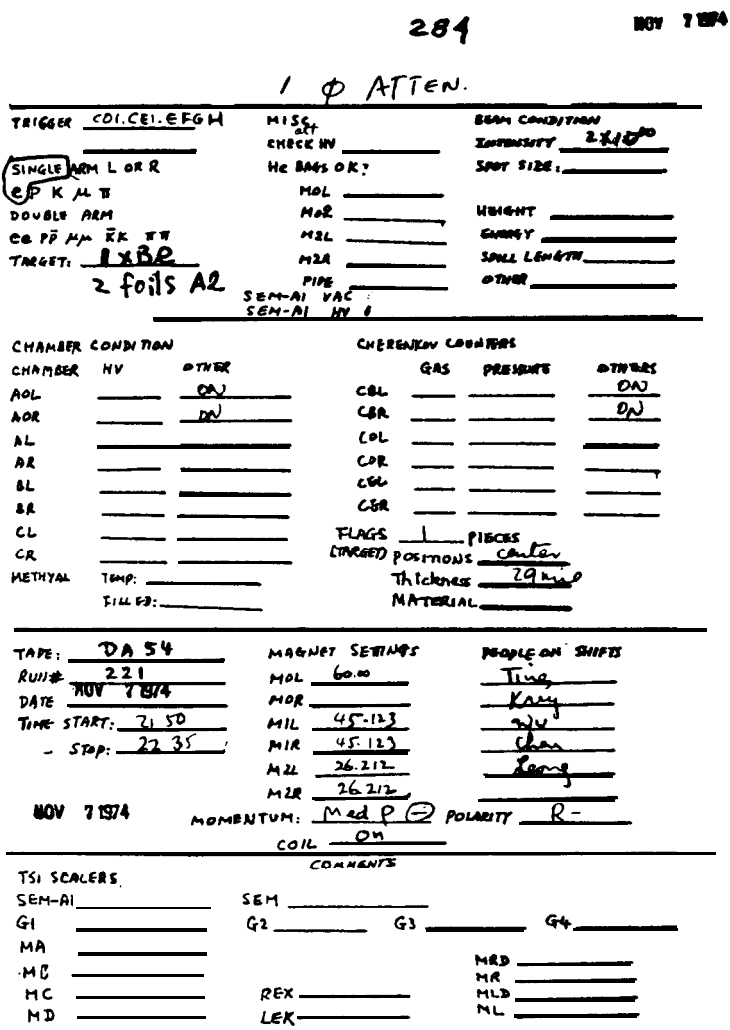


Fig. 13a. Aluminium foil arrangement in front of magnet M_0 in our new experiment to determine the e/π ratio. The converter was used to determine the electron background yield.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SINGLE ARM L OR R
e P K μ π
DOUBLE ARM
ee pp̄ μμ K̄K ππ
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BEAM CONDITION
INTENSITY 2x10^10
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MAGNET SETTINGS
M0L 60.00
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M1L 45-123
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M2L 26.212
M2R 26.212
MOMENTUM: Mod P ⊖ POLARITY R-
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Chen
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COMMENTS

TSI SCALERS
SEM-AI
G1
MA
MB
MC
MD
SEM
G2 G3 G4
REX
LEK
M2D
M2R
M1D
M1L

Fig. 13b. Data sheet for a typical run under the new experimental conditions. Blank spacers imply either data entered in the computer or conditions identical to the prior run. In this run the electrons pass through the right detector arm with a momentum of about 6 GeV. Two pieces of aluminium foil in front of the magnet M_0, serve as converters. [From the group's data book. pp. 282 and 284. 7 November 1974.]

In the meantime, since the end of October, M. Chen and U. Becker and others in the group had been insisting that we publish our results quickly. I was very much puzzled by the $\mu/\pi = 10^{-4}$ ratio and wanted to know how many particles existed. Under pressure, I finally decided to publish our results of J alone.

On 6 November I paid a visit to G. Trigg, Editor of Physical Review Letters, to find out if the rules for publication without refereeing had been changed. Following that visit, I wrote a simple draft in the style of our quantum electrodynamics paper of 1967 (Ref. 5). The paper emphasized only the discovery of J. and the checks we made on the data without mention of our future plans.

On 11 November we telephoned G. Bellettini, the Director of Frascati Laboratory, informing him of our results. At Frascati they started a search on 13 November, and called us back on 15 November to tell us excitedly that they had also seen the J signal and obtained a $\Gamma_{\mu\mu}^2/\Gamma_{\text{total}}$ = 0.8±0.2 keV. Their first spectrum is shown in Fig. 14a. The Frascati Group were able to publish their results in the same issue of Physical Review Letters [25] as ours. Very shortly after, they made a more detailed study of J (Fig. 14b) and also established that its total width is only -60 keV. (It lives ~ 1000 times longer than the ρ meson.) They have since made a systematic search for more particles at lower mass-but have found none [26].

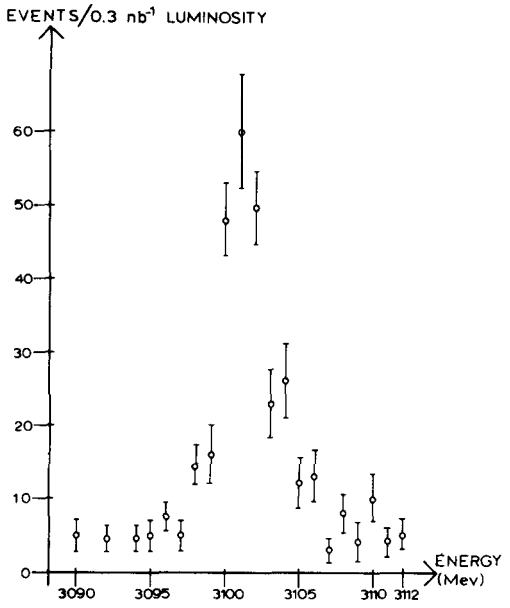

Fig. 14a. Result from one of the Frascati groups on J-particle production. The number of events per 0.3 nb^{-1} luminosity is plotted versus the total c.m. energy of the machine. (From Ref. 25.)

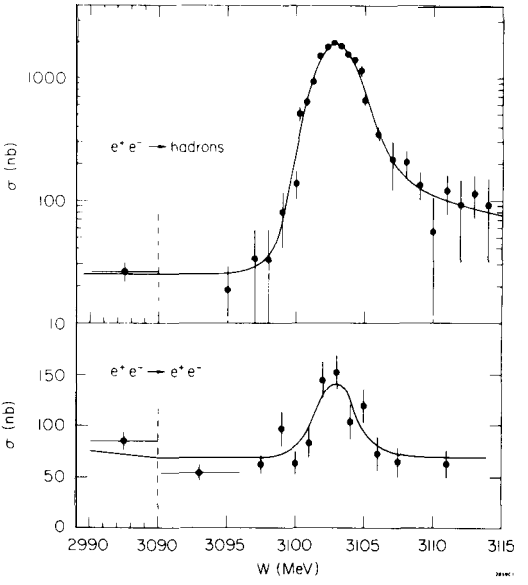

Fig. 14b. Excitation curves for the reactions $e^+ + e^- \to$ hadrons and $e^+ + e^- \to e^+ + e^-$.
The solid line represents the best fit to their data. (From Ref. 26.)

VI. Now, immediately after the discovery of J, because of its heavy mass and unusually long lifetime, there were many speculations as to the nature of this particle. Lee, Peoples, O'Halloran and collaborators [27] were able to photoproduce the J particle coherently from nuclear targets with an ~ 100 GeV photon beam. They showed that the photoproduction of the J is very similar to ρ production and thus were the first to establish that J is a strongly interacting particle.

Pilcher, Smith and collaborators [28] have ingeniously used a large acceptance spectrometer to perform an accurate and systematic study of J production at energies >100 GeV. By using π beams as well as proton beams, and by measuring a wide range of mass and the momentum transfer dependence of $\mu\mu$ production, they were the first to state that the single muon yield which produced the mysterious $\mu/\pi = 10^{-4}$, which had puzzled me for a long time, comes mostly. from the production of muon pairs. The J yield from the π mean seems to be much higher than from the proton.

In Fig. 13 arc listed some of the relative yields of J production from various proton accelerators. It seems that I had chosen the most difficult place to discover the J.

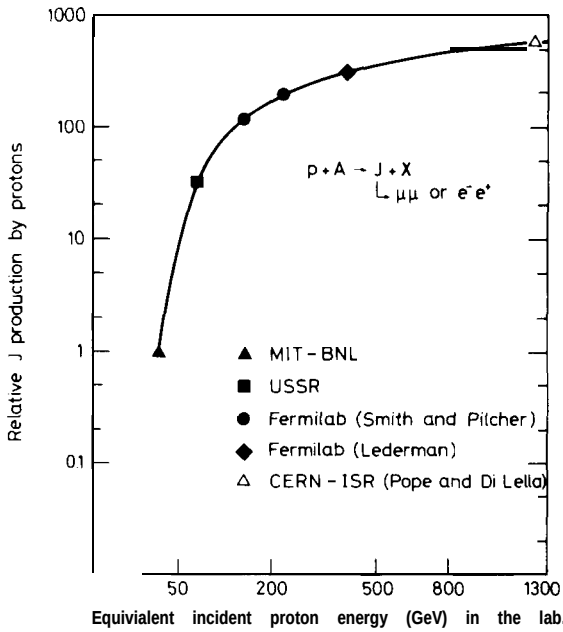

Fig 15. Relative J production, at 90' in the centre of mass. as a function of the energy of the incident proton beam. For experiments using nuclear targets, a linear A-dependence has been used to obtain the yield on a nucleon. Refs: MIT-BNL: J. J. Aubert et al.. Phys. Rev. Letters 33. 1404 (1974) ; CERN-ISR: F. W. Büsser et al., Phys. Letters 56B, 482 (1975); USSR: Yu. M. Antipov et al., Phys. Letters *60B*, 309 (1976) ; Lederman Group: H. D. Snyder et al.. Phys. Rev. Letters 36. 1415 (1976) : Smith-Pilcher Group: K. J. Anderson et al., paper submitted to the 18th Internat. Conf. on High-Energy Physics. Tbilisi. USSR (1976).

3. SOME SUBSEQUENT DEVELOPMENTS

The discovery of the J has triggered off many new discoveries. Some of the most important experimental work was done at SLAC [29] and at DESY [30].

The latest results [31] from the 4π superconducting magnet detector, called "Pluto", measuring the $e^+ + e^- \to$ hadrons near the mass of ψ' (the sister state of J) first discovered at SLAC, are shown in Fig. 16a. The yield of ψ' (and of J) goes up by $>10^2$. It can be seen that an electron-positron storage ring is an ideal machine for studying these new particles. The same group has recently carried out a careful search for new particles at a higher mass region. Their accurate results, shown in Fig. 16b, confirm the indication by SLAC that there may be many more states in this high mass region.

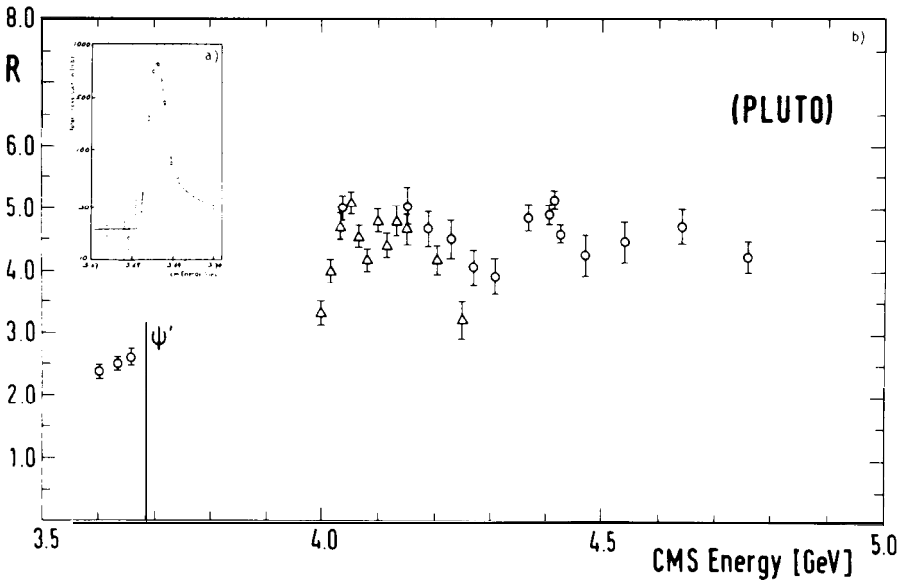

Fig. 16. a) Excitation curve for ψ'. b) Ratio R = $(e^+ + e^- \to \text{hadrons})$ over $(e^+ + e^- \to \mu^+ + + \mu^-)$, measured by the DESY Pluto group. (Ref. 31.)

One of the most important discoveries after that of the J is the observation by the double-arm spectrometer (DASP) Group at DESY [32] of the chain reaction

$$
\begin{aligned}
e^+ + e^- &\to \psi' \\
&\quad \big| \to P_c \to \gamma_1 \\
&\qquad \big| \to \gamma_2 + J \\
&\qquad\quad \big| \to \mu + \mu .
\end{aligned}
$$

By tuning the storage ring so that the electron-positron energy reaches 3.7 GeV to produce the ψ', using the double-arm spectrometer to select the $J \to \mu^+ + \mu^-$ events and detecting both the γ_1 and γ_2 as well, they found that the two photons γ_1 and γ_2 are strongly correlated into two groups. The first group has $E_{\gamma_1} = 169 \pm 7$ MeV and $E_{\gamma_2} = 398 \pm 7$ MeV (or vice versa, since they did not

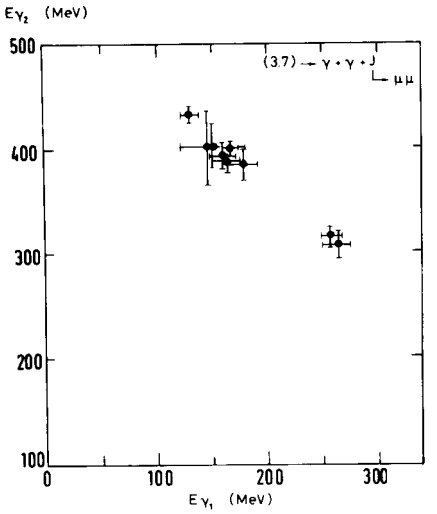

Fig. 17. Scatter plot of the two-photon energies for candidates for the decay $\psi' \to (J \to \mu^+ + \mu^-) + \gamma + \gamma'$. (Ref. 32.)

determine which γ came first), and the second group has $E_{\gamma_1} = 263 \pm 8$ MeV and E_{γ_2} = 315 ± MeV. This correlation, called scatter plot, is shown in Fig. 17. The emission of monochromatic y-rays indicates the existence of intermediate states with even-spin quantum number.

The narrow width of the J and the existence of the P_c and many other states, strongly suggests that the J may be a bound state of two new quarks. The existence of charmed quarks was first proposed by Bjorken and Glashow [33], and Glashow, Iliopoulos and Maiani [34], originally as a cure for certain difficulties in the weak interaction of hadrons. Indeed, the energy levels of the observed states are very similar to the positronium state discovered by Deutsch in 1951 [35].

Recently there are indications from experiments at BNL [36], from DESY [37, 38], from the Fermi Laboratory [39] and from SLAC [40] of the existence of further narrow states, indications which very much follow the general prediction of Glashow.

4. CONCLUSION

In conclusion, we can ask ourselves some further questions:

1) We know that the photon transforms itself into p, ω, and φ with a mass of about 1 GeV. It can transform into J and its various associated states with a mass of about 3 - 5 GeV. What happens when we go to higher and higher energies? It seems very unlikely that there should not be many more new series of photon-like particles.
2) The existence of J implies that we need at least four quarks to explain the phenomena observed so far. How many more quarks will we need if we find a new series of particles in higher energy regions?
3) If we need a large family of quarks, are they the real fundamental blocks of nature? Why has none of them been found?

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Physics 1977

PHILIP W ANDERSON, NEVILL F MOTT and JOHN H VAN VLECK

for their fundamental theoretical investigations of the electronic structure of magnetic and disordered systems

THE NOBEL PRIZE FOR PHYSICS

Speech by Professor PER-OLOV LÖWDIN of the Royal Academy of Sciences
Translation from the Swedish text

Your Majesties, Your Royal Highnesses, Ladies and Gentlemen,

This year's Nobel Prize in physics is shared equally between Philip Anderson, Sir Nevill Mott and John Van Vleck for their fundamental contributions to the theory of the electronic structure of magnetic and disordered systems.

All matter consists of positive and negative electricity: partly heavy positive elementary particles gathered in atomic nuclei, partly light negative elementary particles-electrons-which move in wonderful patterns around the nuclei-always attracted to them but difficult to catch because of their own movement. It is this electron dance which is essentially responsible for the electric, magnetic, and chemical properties of matter.

The 1937 Nobel Prize winner in medicine, Albert Szent-Györgyi, has often compared the chemical process in living cells with a great drama played with the electrons as actors on a stage formed by the biomolecules-with the only difference that the scene as well as the actors may be a thousand billion times smaller than we are accustomed to from the Royal Opera. No scientist has seen the score of this musical of life itself, and no one will probably ever be able to see it in its entirety-only a few have been granted the privilege of seeing small fragments in the form of isolated ballets, often with a hero and sometimes with a ballerina.

In the crystal and ligand field theories developed by Van Vleck, there is always a metal atom playing the role of the hero in the drama. In many of the enzymes fundamental for the life of our body, there is often a metal atom in the active center which regulates the action. The haemoglobin in our red blood cells contains an iron atom which carries the oxygen molecule to its given place in the body-in the same way as the hero carries the ballerina on his strong arms. It is Van Vleck who has developed the basic theory for such processes, which are also of great importance in the chemistry of complex compounds, geology, and laser technology.

The electronic dance is of similar importance also in the solid bodies surrounding us-in the ladies' diamonds, in the every-day rock salt, or in the amorphous glasses. Such materials have characteristic electric and magnetic properties which depend on the motions of the electrons. In the same way as it is easier in an ordinary waltz to waltz forward than backward, there is in the electronic dance a specific spin-orbit coupling between the rotations of the electrons and their translational movements, which is of importance for the magnetic properties. Like the dancers in a ballet are constantly changing place, the electrons have also their own exchange and superexchange phenomena-their own characteristic "pas de deux". Both Van Vleck and Anderson have studied the local magnetic properties of matter,

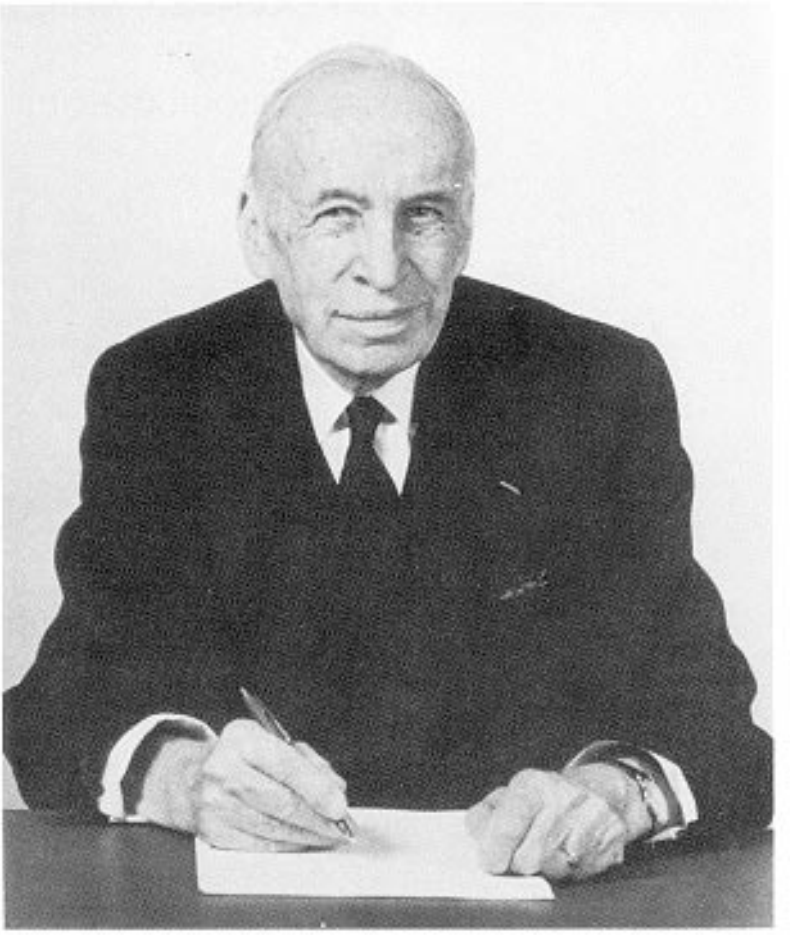
where the hero is a metal atom with strong personal magnetism whose special properties may vary strongly with the environment-a theory basic for the construction of dilute magnetic alloys. Here one dares perhaps to speak about a successful localization policy.

One of the greatest current problems of humanity is the so-called energy problem-it has been said that the modern society uses too much energy. According to the laws of physics, such a statement is quite absurd, since energy can neither be created nor destroyed. The whole thing is instead a problem of order-at the level of the elementary particles. What happens is that energy of higher order is transformed into energy of lower order, that mechanical and electric energy are changed into heat, that the motions of the elementary particles involved will be more and more disordered. It is the merit of Anderson to have shown that even the reverse may sometimes happen: that geometrically disordered materials, as for instance glass, have their own laws, and that the electronic dance in them may lead to localized states with a high form of order, which influence the properties of the material. Perfectly ordered systems are of great importance in electronics, but they are usually very expensive to produce, so disordered systems with similar properties are hence of essential importance.

In some of his work, Sir Nevill Mott has taken up these and similar ideas in order to study the electrical properties of materials and the transition between conductors, semi-conductors and insulators. In this connection, Mott has also investigated the importance of the interaction between the electrons -that the electrons indeed like to dance in pairs, but also that there is a mutual repulsion which sometimes causes them to guard their own domains and stop the hand-in-hand dance which is essential for the electronic conductivity of the material. The theory for Mott-transitions and Mott-Anderson transitions is today of fundamental importance for the understanding of certain materials and for the construction of new ones. Anderson and Mott have shown that properly controlled disorder may be technically as important as perfect order.

This year's Laureates in physics are all three giants within solid-state theory, and it is actually rather remarkable how small a portion of their total work has been considered in connection with this year's Nobel award. Even if these discoveries already now have shown their technical value, it is their fundamental contributions to the free basic research-to the human knowledge of the electronic structure of solids-which has primarily been awarded, with the understanding that it may be even more awarded, with the understanding that it may be even more practically important in the future. Through their work, Anderson, Mott, and Van Vleck have shown that the understanding of the electronic choreography is not only remarkably beautiful from the point of view of science but also of essential importance for the development of the technology of our every-day life.

I have the pleasure and the honour on behalf of the Academy to extend to you our warmest congratulations and I now invite you to receive your prizes from the hands of His Majesty the King.



John H Van Vleck

JOHN HASBROUCK VAN VLECK

I was born in Middletown, Connecticut, March 13, 1899 where my father and grandfather were respectively professors of mathematics and of astronomy at Wesleyan University. However, when I was seven years old father accepted a professorship at the University of Wisconsin, so I grew up in Madison, Wisconsin, where I attended the public schools, and graduated from the University of Wisconsin in 1920. As a sort of revolt against having two generations of academic forbears, I vowed as a child that I would not be a college professor, but after a semester of graduate work at Harvard, I outgrew my childish prejudices, and realized that the life work for which I was best qualified was that of a physicist, not of the experimental variety, but in an academic environment.

I have been lucky in a number of respects. Coming from an academic family, I had invaluable parental guidance or advice at various times. At Harvard I took most of my courses under Professor Bridgman or Professor Kemble. The latter's course on quantum theory fascinated me, so I decided to write my doctor's thesis under Kemble's supervision. He was the one person in America at that time qualified to direct purely theoretical research in quantum atomic physics. My doctor's thesis was the calculation of the binding energy of a certain model of the helium atom, which Kemble and Niels Bohr suggested independently and practically simultaneously, with Kramers making the corresponding calculation in Copenhagen. The results did not agree with experiment for the "old quantum theory" was not the real thing. However, when the true quantum mechanics was discovered by Heisenberg and others in 1926, my background in the old quantum theory and its correspondence principle was a great help in learning the new mechanics, particularly the matrix form which is especially useful in the theory of magnetism.

I was fortunate in being offered an assistant professorship at the University of Minnesota in 1923, a year after my Ph. D. at Harvard, with purely graduate courses to teach. This was an unusual move by that institution, as at that time, posts with this type of teaching were generally reserved for older men, and recent Ph. D.'s were traditionally handicapped by heavy loads of undergraduate teaching which left little time to think about research. Also it was at Minnesota that I met Abigail Pearson, a student there, whom I married June 10, 1927, and on Nobel Day, December 10, 1977 we had been married exactly 50 1/2 years !

I was also lucky in choosing the theory of magnetism as my principal research interest, as this is a field which has continued to be of interest over

the years, with new ramifications continuing to make their appearance (magnetic resonance, relaxation, microwave devices, etc.). So often a particular field loses general interest after a span of time. My last paper dealing with magnetism was published fifty years after my first one.

Besides my work on magnetism, and the closely related subjects of ligand fields and of dielectrics, one of my interests has been molecular spectra. The theoretical problems associated with the fine structures therein appeared rather academic at the time, but recently have burgeoned in interest in connection with radioastronomical investigations, including notably those of the observatory at Gothenburg.

Degrees, positions, award:, etc.
A.B. University of Wisconsin, 1920
Ph. D., Harvard University, 1922 (instructor 1922-3)
Honorary D. Sc. or D. Honoris Causa, Wesleyan U., 1936; U. Wisconsin, 1947; Grenoble U., 1950; U. Maryland, 1955; Oxford U., 1958; U. Paris, 1960; Rockford College, 1961; U. Nancy, 1961; Harvard U., 1966; U. Chicago, 1968; U. Minnesota 1971.
On faculty, University of Minnesota, 1923-28; University of Wisconsin 1928-34 Harvard University 1934-69, emeritus 1969- (Dean of Engineering and Applied Physics 1951-57).
Lorentz (visiting) professor, Leiden, 1960; Eastman Professor, Oxford, 1961-62; Guggenheim Fellow, 1930.
Foreign member, Royal Swedish Academy, Uppsala Academy, Netherlands Academy, Academic des Sciences, Royal Society of London.
National Medal of Science, USA; Lorentz Medal (Netherlands) ; Cresson Medal (Franklin Institute) ; Michelson Prize of Case Institute of Technology; Langmuir Award in Chemical Physics; General Electric Foundation; Chevalier, Legion of Honor.
Member, National Academy of Sciences, American Academy of Arts and Sciences, American Philosophical Society, International Academy of Quantum Molecular Science; Honorary Member, French Physical Society; President, American Physical Society, 1952.

Professor Van Vleck died in 1980.

QUANTUM MECHANICS THE KEY TO UNDERSTANDING MAGNETISM

Nobel Lecture, 8 December, 1977

J.H. VAN VLECK
Harvard University, Cambridge, Massachusetts, USA

The existence of magnetic materials has been known almost since prehistoric times, but only in the 20th century has it been understood how and why the magnetic susceptibility is influenced by chemical composition or crystallographic structure. In the 19th century the pioneer work of Oersted, Ampere, Faraday and Joseph Henry revealed the intimate connection between electricity and magnetism. Maxwell's classical field equations paved the way for the wireless telegraph and the radio. At the turn of the present century Zeeman and Lorentz received the second Nobel Prize in physics for respectively observing and explaining in terms of classical theory the so-called normal Zeeman effect. The other outstanding early attempt to understand magnetism at the atomic level was provided by the semi-empirical theories of Langevin and Weiss. To account for paramagnetism, Langevin (1) in 1905 assumed in a purely ad hoc fashion that an atomic or molecular magnet carried a permanent moment μ, whose spatial distribution was determined by the Boltzmann factor. It seems today almost incredible that this elegantly simple idea had not occurred earlier to some other physicist inasmuch as Boltzmann had developed his celebrated statistics over a quarter of a century earlier. With the Langevin model, the average magnetization resulting from *N* elementary magnetic dipoles of strength μ in a field *His* given by the expression

$$M = \frac{N\mu\int\int\cos\theta e^{\mu H\cos\theta/kT}d\omega}{\int\int e^{\mu H\cos\theta/kT}d\omega} = NL\left(\frac{\mu H}{kT}\right), \text{ where } L(x) = \coth x - \frac{1}{x} \qquad (1)$$

At ordinary temperatures and field strengths, the argument x of the Langevin function can be treated as small compared with unity. Then $L(x) = \frac{1}{3}x$, and Eq. (1) becomes

$$M = N\frac{\mu^2}{3kT}H \qquad (2)$$

so that the magnetic susceptibility $\chi = \frac{M}{H}$ is inversely proportional to the absolute temperature, a relation observed experimentally for oxygen ten years earlier by Pierre Curie (2) and hence termed Curie's law.

To explain diamagnetism, Langevin took into account the Larmor precession of the electrons about the magnetic field, and the resulting formula for the diamagnetic susceptibility is

$$\chi = -\frac{Ne^2}{6mc^2}\sum_i (r_i)^2 \tag{3}$$

where $(r_i)^2$ is the mean square radius of an electron orbit, and the summation extends over all the electrons in the atom. The important thing about (3) is that, in substantial agreement with experiment, it gives a diamagnetic susceptibility independent of temperature, provided the size of the orbits does not change.

Two years later, in 1907, Pierre Weiss (3), another French physicist, took the effective field acting on the atom or molecule to be the applied field augmented by a mysterious internal or molecular field proportional to the intensity of magnetization. The argument of the Langevin function then becomes $\frac{\mu(H+qM)}{kT}$ rather than $\frac{\mu H}{kT}$, and in place of (2) one has

$$\chi = \frac{M}{H} = \frac{N\mu^2}{3k(T-T_c)} \text{ where } T_c = \frac{Nq\mu^2}{3k} \tag{4}$$

Since the right side of (4) becomes infinite for T = T_c, the Weiss model predicts the existence of a Curie point below which ferromagnetism sets in. This model also describes qualitatively quite well many ferromagnetic phenomena. Despite its many successes there was one insuperable difficulty from the standpoint of classical electrodynamics. Namely the coefficient q of the molecular field qM should be of the order $\frac{4\pi}{3}$ whereas it had to be of the order 10^3 to describe the observed values of T_c.

There was, moreover, an even worse difficulty. If one applies classical dynamics and statistical mechanics consistently, a very simple calculation, which can be made in only a few lines but I shall not reproduce it here, shows that the diamagnetic and paramagnetic contributions to the susceptibility exactly cancel. Thus there should be no magnetism at all. This appears to have been first pointed out by Niels Bohr (4) in his doctor's dissertation in 1911, perhaps the most deflationary publication of all time in physics. This may be one reason why Bohr broke with tradition and came forth with his remarkable theory of the hydrogen spectrum in 1913. That year can be regarded as the debut of what is called the old quantum theory of atomic structure, which utilized classical mechanics supplemented by quantum conditions. In particular it quantized angular momentum and hence the magnetic moment of the atom, as was verified experimentally in the molecular beam experiments of Stern and Gerlach (5). Hence there was no longer the statistical continuous distribution of values of the dipole moment which was essential to the proof of zero magnetism in classical theory. When Langevin assumed that the magnetic moment of the atom or molecule had a fixed value μ, he was quantizing the system without realizing it, just as in Moliere's *Bourgeois Gentilhomme*, Monsieur Jourdain had been writing prose all his life, without appreciating it, and was overjoyed to discover he had been doing anything so elevated. Magnetism could be understood qualitatively in terms of in-

complete shells of electron orbits, and a sentence of Bohr which I like to quote reads "In short an examination of the magnetic properties and colors of the long periods gives us a striking illustration of how a wound in the otherwise symmetrical inner structure of the atom is first created and then healed." However, with the passage of time it became increasingly clear that the old quantum theory could give quantitatively correct results for energy levels or spectral frequencies only in hydrogen. One historian of science has referred to the early 1920's as the crisis in quantum theory, but I would characterize this era as one of increasing disillusion and disappointment in contrast to the hopes which were so high in the years immediately following 1913.

The advent of quantum mechanics in 1926 furnished at last the real key to the quantitative understanding of magnetism, I need not elaborate on the miraculous coincidence of three developments, the discovery of the matrix form of quantum mechanics by Heisenberg and Born, the alternative but equivalent wave mechanical form by de Broglie and Schrödinger, and the introduction of electron spin by Uhlenbeck and Goudsmit. A quantum mechanics without spin and the Pauli exclusion principle would not have been enough - one wouldn't have been able to understand even the structure of the periodic table or most magnetic phenomena. Originally spin was a sort of appendage to the mathematical framework, but in Dirac 1928 synthesized everything in his remarkable four first order simultaneous equations. To stress the importance of the quantum mechanical revolution, I cannot do better than to quote an often-mentioned sentence from one of Dirac's early papers, which reads *"The general theory of quantum mechanics is now almost complete. The underlying physical laws necessary for the mathematical theory of a large part of physics and all of chemistry are thus completely known".*

With at last the key available for the proper analysis of what was going on inside the atom, it was natural that more than one physicist would try applying it to a particular problem. So it is not surprising that four different researchers independently calculated and reported in practically simultaneous publications (6) the susceptibility of a rotating diatomic molecule carrying a permanent dipole moment, which could be either electric or magnetic depending on whether one was interested in an electric or magnetic susceptibility. (I was one of the four. The others were Kronig, Manneback, and Miss Mensing working in collaboration with Pauli. The new mechanics happily restored the factor $\frac{1}{3}$ in the Langevin formula) (or the corresponding Debye expression in the electric case), as shown in Table I. Thus was ended the confusion of the old quantum theory, where half quanta worked better in band spectra even though whole integers were required with rational application of Bohr's 1913 ideas.

There are three common paramagnetic gases, viz. O_2, NO_2, and NO. I shall discuss NO first as its behavior is the most interesting of the three. In 1926 Robert Mulliken, who has a sixth sense for deducing molecular energy levels from band spectra, had decided that the ground state of the NO molecule was a $^2\Pi$ state, whose two components were separated by about 122 cm^{-1} but he wasn't sure whether the doublet was regular rather than inverted. I tried

Table 1. Value of C in Relation $\chi = CN\mu^2/kT$

Value of C	Form and Year of Theory
$\frac{1}{3}$	Classical, 1905
1.54	Whole quanta, 1921
4.57	Half Quanta, 1924
$\frac{1}{3}$	Quantum mechanics, 1926

calculating the susceptibility of NO on the basis of Mulliken's energy levels and found (7) that the observed susceptibility at room temperatures could be explained on the basis that the doublet was regular, i.e. the $^2\Pi_{1/2}$ component lower than the $^2\Pi_{3/2}$. I wasn't entirely convinced that the agreement was real rather than spurious, as molecular quantum mechanics was then in its infancy. If the theory was correct there should be deviations from Curie's law, and so measurements on the susceptibility as a function of temperature should be decisive. To my surprise, experiments to test this prediction were performed in 1929 at three different laboratories in different parts of the world, with each going to a lower temperature than the preceding (8). As shown in Fig. 1, the agreement with theory was gratifying. The ordinate in Fig. 1 is not the susceptibility itself, but rather the effective magneton number μ_{eff} defined by $\chi = \mathcal{N}\mu^2_{\text{eff}}\beta^2/3kT$, where β is the Bohr magneton $he/4\pi mc$. The non-constancy of μ_{eff} is a measure of the deviation from Curie's law.

My calculations on NO started me thinking on the general conditions under which Curie's law should be valid or non-valid. I noted the fact, often over-

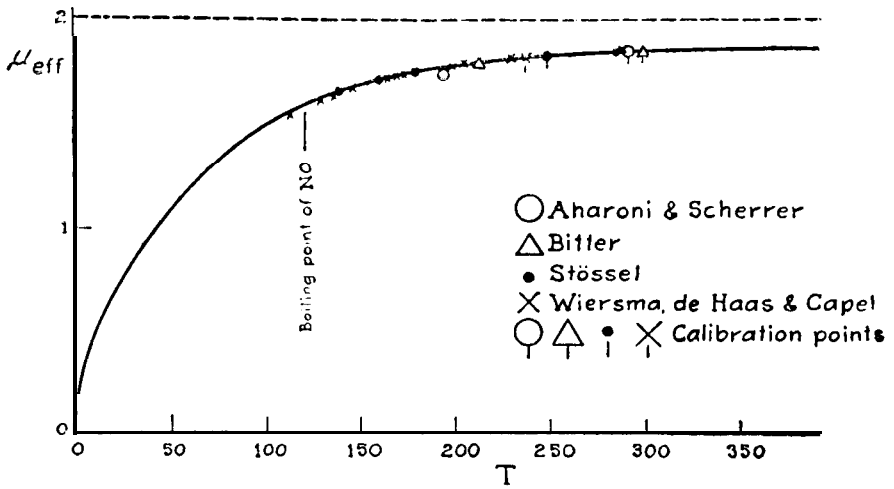


Fig. 1. The effective magneton number (measured in multiples of the Bohr unit $\beta = he/4\pi mc$) of nitric oxide as a function of temperature. Were Curies law valid, the curve would be a horizontal straight line.

looked in those early days, that to make a proper computation of the susceptibility even in weak fields, it is necessary to know the energy of the stationary states, or alternatively the partition function, to the second order in the field strength *H*., corresponding to including the second as well as first order Zeeman effect. If the energy of a stationary state is

$$E_i = E_i^{(0)} + E_i^{(1)}H + E_i^{(2)}H^2 + \ldots$$

the correct formula for the susceptibility is

$$\chi = \frac{\mathcal{N}}{\sum p_i} \sum_i \left(\frac{E_i^{(1)\,2}}{kT} - 2E_i^{(2)} \right) p_i \text{ with } p_i = \exp\left(\frac{-E_i^{(0)}}{kT} \right)$$

Perturbation theory tells us that

$$E_i^{(1)} = <i \mid \mu_H \mid i>,\ E_i^{(2)} = \sum_j \frac{\mid <i \mid \mu_H \mid j> \mid^2}{h\nu_{ij}} \quad (j \neq i) \tag{6}$$

where $h\nu_{ij}$ is the energy interval $E_i^{(0)} - E_j^{(0)}$ spanned by the matrix element $<i|\mu_H|j>$ of the magnetic moment in the direction of the field H. From (5) and (6) one derives (7) the results presented in Table II.

Table II. Behavior of the Susceptibility in Various Situations

(a) x is proportional to $1/T$ if all $|h\nu_{ij}|$ are $<<kT$.

(b) χ is independent of T if all $|h\nu_{ij}|$ are $>>kT$.

(c) χ = A + B/T if all $|h\nu_{ij}|$ are either $>>kT$ or $<<kT$.

(d) no simple dependence of χ on T if $|h\nu_{ij}|$ is comparable with kT.

In connection with the above it is to be understood that the relevant $h\nu ij$ are only those which relate to the energy intervals spanned by $<i|\mu|j>$, which because of selection principles can often be less than the total spread in the populated energy levels.

From too cursory examination of Eq. (5) one might conclude that case (a) could never arise when there is a second order Zeeman effect, but this is not so. Since $h\nu_{ji} = -h\nu_{ij}, |<i|\mu_H|j>|^2 = |<j|\mu_H|i>|^2$ the various terms in (4) can be so paired as to involve a factor $(p_j - p_i)/h\nu_{ij})$ which is approximately $\frac{1}{2}(p_i + p_j)/kT$ if $|h\nu_{ij}| << kT$. The fact that the factor $h\nu_{ij}$ has thereby disappeared shows that there is no catastrophe in the expression for the susceptibility even when the denominators in the expression (6) for the second order perturbed energy are very small.

The NO molecule, as we have seen, is an illustration of the situation (d). On the other hand, the O_2 and NO, molecules are examples of (a) and hence obey Curie's law. The oxygen molecule exhibits the same susceptibility as though its spin of unity (S = 1) were completely uncoupled from the molecule. Actually the spin is coupled to the molecule so that most of the Zeeman energy becomes of the second rather than first order, but this complication is immaterial as regards the susceptibility since the binding energy is only of the order 2 cm^{-1}, small compared to *kT*. The third common paramagnetic gas NO, should have a susceptibility corresponding to a free spin $\frac{1}{2}$, as it is an odd molecule. Existing data were in disagreement with this prediction when I

made it, but new magnetic measurements made by Havens at Wisconsin at my suggestion removed this discrepancy (9).

In 1925 Hund (10) wrote a paper on the magnetic susceptibilities of rare earth compounds which was the crowning achievement of the empiricism of the old quantum theory. He utilized Landé's then phenomenological g-factor and the Hund rule that the state of lowest energy is that of maximum spin, and of maximum *L* compatible with this S. At the time this rule was an inspired conjecture, but today physicists justify it by examining nodes in the wave function. He thus obtained the formula

$$\chi = \frac{N\beta^2 J(J+1)g_J^2}{3kT}$$

for the susceptibility, and found that this expression agreed remarkably well with experiments for all the trivalent rare earths compounds except those containing Sm or Eu. In 1928 Laporte (11) pointed out that for these particular two ions, the multiplet structure was such that the interval separating the lowest multiplet component from the one next above it is not large compared to *kT*. So he summed Hund's expression for x over the multiplet's various values ofK weighted in accordance with the Boltzmann factor. Even so, he was not able to raise the susceptibility to the values found experimentally. When I read his paper it occurred to me that probably the cause for the discrepancy was that the second order energy had been omitted. So Miss Frank and I made the relevant calculations (12), and then there was agreement with experiment, as shown in Fig. 2. The reason that Hund was able to obtain agreement with experiment for other rare earths was that his

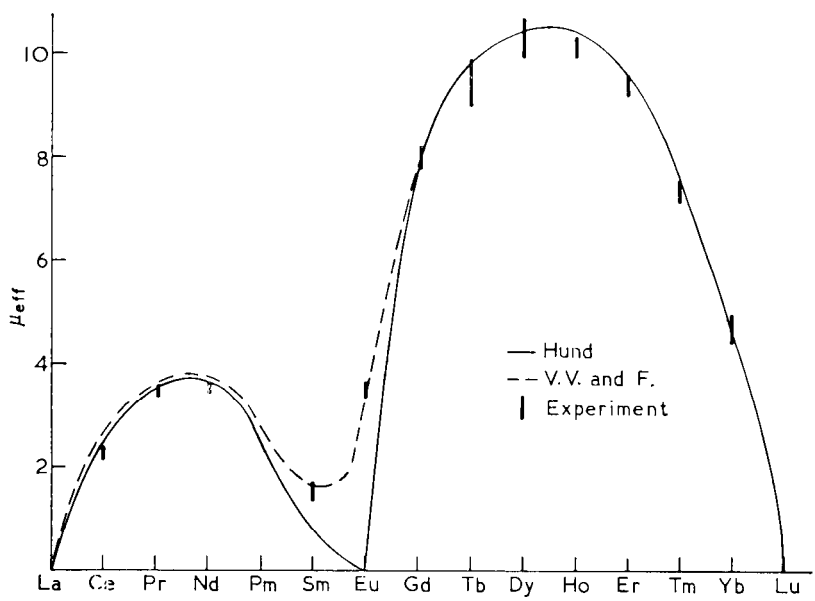


Fig. 2. The effective magneton number (in multiples of β) at room temperature for the sequence of trivalent ions in the configurations $4f^0, 4f, 4f^2, ., 4f^{14}$.

empirical expression for the first order energy was the same as the true quantum-mechanical one, and that the second order energy could be omitted without too much error. The latter was the case because the interval separating the lowest multiplet component from the next one above is large except for Sm^{3+}, Eu^{3+}, and the second order energy involves this interval in the denominator. Since Sm^{3+} and Eu^{3+}, unlike the other rare earth ions, correspond to case (d) of table II, deviations from Curie's law are to be expected for salts containing these ions. This was indeed confirmed by the limited amount of experimental data available at the time.

In 1930 and 1931 a great deal of my time went into writing my book on the Theory of Electric and Magnetic Susceptibilities, which appeared in 1932 (13). In this volume I aimed to include the major theoretical developments which had taken place up to the time of writing. Besides the things which I have already mentioned, there were other major developments in the theory of magnetism in the early days of quantum mechanics. Heisenberg (14) took the mystery out of the then twenty year old Weiss molecular field. He showed that it arose from exchange effects connecting the different magnetic atoms, which had the effect of introducing the needed strong coupling between the spins. Other notable theoretical developments prior to 1932 included Landau's paper (15) on the diamagnetism of free electrons, in which he showed that spinless free electrons had a small susceptibility of diamagnetic sign, in contrast to the zero result of classical mechanics. Pauli (18) showed that the spin moment of conduction electrons gives rise to only a small paramagnetic susceptibility practically independent of temperature. This paper was notable because it was the first application of Fermi-Dirac statistics to the solid state. If one used the Boltzmann statistics one would have a large susceptibility obeying Curie's law.

On the other hand, there were some important development which arrived just a little too late for me to include them in my volume. Néel's first paper on antiferromagnetism appeared in 1932, and in later years he introduced an important variant called ferri-magnetism, in which the anti-parallel dipoles are of unequal strength, so that they do not compensate and the resulting behavior can be ferromagnetic (17). There was also Peirls' (18) theoretical explanation of the de Haas-van-Alphen effect, and Bloch's 1932 paper (19) on the width of the boundaries (now called Bloch walls) separating the elementary domains in ferromagnetic materials. The corresponding domain structure was explained and elaborated by Landau and Lifschitz two years later (20).

In 1930 I held a Guggenheim fellowship for study and travel in Europe. I spent most of the time in Germany, but by far the most rewarding part of the trip scientifically was a walk which I took with Kramers along one of the canals near Utrecht. He told me about his own theorem (21) on degeneracy in molecules with an odd number of electrons and also of Bethe's long paper (22) concerned with the application of group theory to the determination of the quantum mechanical energy levels of atoms or ions exposed to a crystalline electric field, and in my book I referred to the role of the crystalline field only

in a qualitative way, stressing the fact that it could largely suppress the orbital part of the magnetic moment in salts of the iron group. In the process of writing I did not have the time or energy to attempt quantitative numerical computations. I was most fortunate when, heginning in the fall of 1931 I had two post-doctoral students from England, namely William (now Lord) Penney, and Robert Schlapp. I suggested to these two men that they make calculations respectively on salts of the rare earth and of the iron group. The basic idea of the crystalline field potential is an extremely simple one, namely that the magnetic ion is exposed not just to the applied magnetic field but experiences in addition a static field which is regarded as an approximate representation of the forces exerted upon it by other atoms in the crystal. The form of the crystalline potential depends on the type of crystalline symmetry. For some of the most common types of symmetry the terms of lowest order in x,y,z are respectively

axial, tetragonal or hexagonal	$A(x^2 + y^2 - 2z^2)$	(7a)
rhombic	$Ax^2 + By^2 - (A + B)z^2$	(7b)
cubic	$D(x^4 + y^4 + z^4 - \frac{3}{5} r^4)$	(7c)

If the potential satisfies Laplace's equation, the factors A, *B, D* are constants, but because of charge overlap they can be functions of the radius.

The 4f electrons responsible for the magnetism of the rare earths are sequestered in the interior of the atom, and so experience only a small crystalline field. The general formalism which I developed in 1927 and which is displayed in table II shows that it is a good approximation to treat the atom as free provided the decomposition of the energy levels caused by the crystalline field is small compared to *kT.* This condition is fulfilled fairly well for the rare earths at room temperatures, and explains the success of Hund's theory. At low temperatures inclusion of the crystalline potential is usually imperative, and so Penney utilized it to interpret the existing experimental data mainly by Cabrera and by Becquerel. Fig, 3 is taken from the original paper of Penney and Schlapp (23). The ordinate is the reciprocal of the susceptibility. Hence for Nd^{3+} one expects it to approach zero as $T \rightarrow 0$ inasmuch as Nd^{3+} is an ion with an odd number of electrons, and even at $T = 0$ there is still the Kramers degeneracy which implies a first order Zeeman effect and a $1/T$ term in the susceptibility. On the other hand for the even ion Pr^{3+} a sufficiently asymmetrical field should completely lift the degeneracy (case (b) of Table II) and the susceptibility should remain finite as one approaches T = 0. This difference is strikingly exhibited in the two sides of Fig. 3.

When applied to the iron group the results of crystal field theory are particularly striking and form the basis of much of what may be called modern magnetochemistry. The crystalline potential is much larger than for the rare earths and is so powerful that it quenches a large part of the orbital part of the magnetic moment even at room temperatures. Schlapp found that the magnetic behavior in the iron group required a large crystalline field of nearly (but usually not entirely) cubic symmetry.

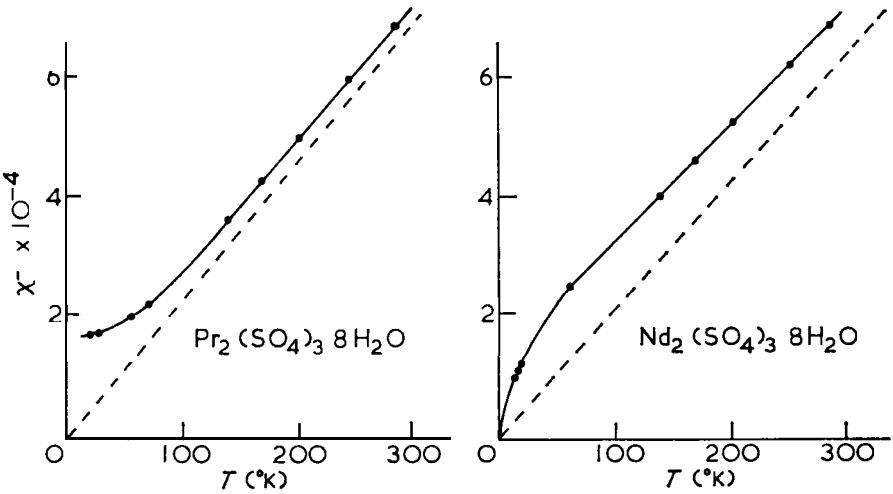


Fig. 3. The reciprocal of the susceptibility as a function of temperature, for two rare earth compounds containing respectively an even and odd number of electrons.

Each time I read the paper of Schapp and Penney (24) I am impressed with how it contains all the essential ingredients of modern crystalline field theory, although there have been changes in the best quantitative estimate of *D* in (7c). For instance it accounted for the fact that most nickel salts are nearly isotropic magnetically and follow Curie's law down to quite low temperatures, whereas the corresponding cobalt salts are highly anisotropic and deviate greatly from Curie's law. However, for a while we thought that there was a difficulty and inconsistency. Let us focus attention on the ions in *F* states; e.g. Ni^{++}, Co++. In a nearly cubic field an F state will decompose in the fashion shown in Figure 4. If a non-degenerate level is deepest, as in Figure 4, then the orbital moment is completely quenched, and there should be almost complete isotropy. On the other hand, if Figure 4 is upside down, and if the components a, b, c of the ground level do not coincide because of deviations

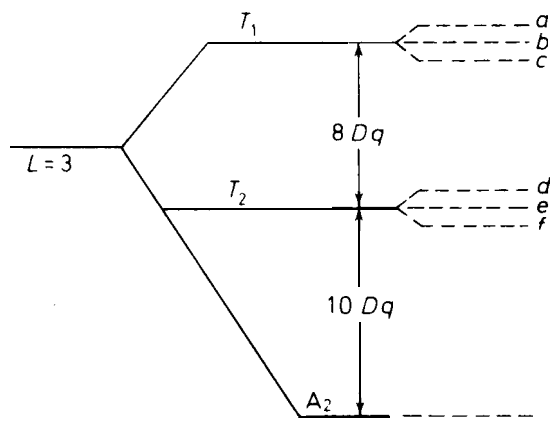


Fig. 4. Orbital energies of an F state in a nearly cubic field The decompositions (a-b-c) and (d-e-f) ensue only because of deviations from cubic symmetry. The quantity Dq is connected with the constant D of (7c) by the relation Dq = 2 D<r^4> /105.

from cubic symmetry, and so have different Boltzmann factors, the anisotropy will be considerable. The very different behavior of nickel and cobalt can thus be explained if it supposed that Figure 4 is rightside up for Ni^{++} but is upside down in Co++. The calculations of Schlapp then worked fine. However, this seemed to us for a while a thoroughly dishonest procedure, as it appeared to require a change in the sign of D

Then one day it dawned on me that a simple calculation based on the invariance of the trace shows that the splitting pattern does indeed invert in going from nickel to cobalt even though the constant D is nearly the same.

The article (25) in which I published this result is my favorite of the various papers I've written as it involved only a rather simple calculation, and yet it gave consistency and rationality to the apparently irregular variations in magnetic behavior from ion to ion.

The iron group salts I have discussed are of the 6-coordinated type, e.g. $Co(NH_4)_2(SO_4)_2 6 H_2O$. A simple electrostatic calculation made by Gorter (26) shows that the constant D in (7c) should change sign when the coordination is 4 rather than 6 fold and then Fig. 4 should be upright in Co++ and inverted in Ni^{++}. Krishnan and Mookherji (2 7) in 1937 verified experimentally this theoretical prediction. They prepared some tetracoordinated cobalt compounds, which are a beautiful cobalt blue in color and found that they indeed show very much less anisotropy than do the pink six-coordinated ones.

In 1935 I published a paper (28) in which I amplified and generalized in two respects the primitive crystal field theory employed a few years previously by Penney, Schlapp, and myself. In the first place I showed that Bethe's grouping of energy levels according to symmetry type was still valid even if one allowed the electrons in the unclosed shells to wander away sometimes from the central paramagnetic ion and take a look at the diamagnetic atoms clustered around it. In more technical language, the wave function of the electron has mixed into it small terms which correspond to such excursions. This generalization corresponds to the use of molecular rather than atomic orbitals. Following Ballhausen (29) it is convenient to designate this more general model as ligand rather than crystal field theory, as chemists sometimes refer to the neighboring atoms clustering about the central ion as ligands. The use of ligand in distinction from crystal field theory can also be characterized as making allowance for incipient covalence.

The other modification I made of the conventional theory was to note that under certain conditions, the levels may be split so much by the crystalline field as to break down the Hund rule that the deepest state is that of maximum multiplicity permitted by the Pauli principle. This situation is shown schematically in Fig. 5, which is drawn for the configuration d^6. According to the Hund rule the deepest state is ^5D (S = 2) and this necessitates all but one of the five Stark components being singly inhabited, as in the left side of Fig. 5. It is obvious that the energy in the crystalline or ligand field is lower if the three deepest Stark components are doubly populated, with antiparallel spin because of the exclusion principle. However, then the resultant spin is

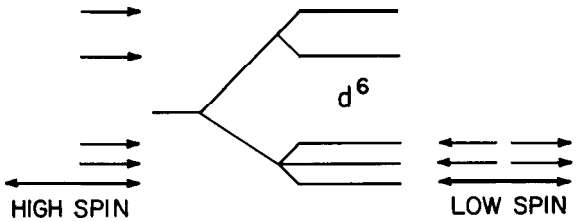


Fig. 5. The central diagram of the figure shows the decomposition of a single 3d level in a field of mainly cubic symmetry. The arrows indicate how the different crystalline field components are filled in case the ion contains six 3d electrons, and also the direction of alignment of each spin. The situation in the left side of the figure represents conformity to the Hund rule, while the right exemplifies what happens when minimization of the energy in the crystalline field is so important as to break down this rule.

only 0, the Russell-Saunders coupling is broken up, and the part of the energy not associated with the crystalline field is raised. The two cases represented by the two sides of Fig. 5 are sometimes referred to as the high and low spin cases. When the susceptibility of a compound is found to conform to the low rather than high spin situation, this is something of interest to chemists. It shows that the inter-atomic bonding is strong, since it is large enough to break down the Hund rule. Beginning with Pauling and Coryell (30) in 1936, this magnetic criterion has even been used to study the chemical behavior of iron in blood. For example, the ferro-haemoglobin ion exhibits high and low spin values 2 and 0 in the presence of H_2O and O_2 molecules respectively. I should by all means mention that prior to my own paper Pauling (31) also stressed the role of covalency effects in magnetism, and the fact that sometimes the low rather than high spin case may be realized. However, in my opinion the method of electron pair bonds which he employed is less flexible and realistic without some modification than is that of molecular orbitals which I used.

On 1937 Jahn and Teller (32) established a remarkable theorem that when in a crystal there is a degeneracy or coincidence of levels for reasons of symmetry, the ligands experience forces which distort the crystalline arrangement, thereby lowering both the symmetry and the energy.

I realized that the Jahn-Teller effect might have an important effect on magnetic susceptibilities, and in 1939 I published a paper on this subject (33). The energetic effect of Jahn-Teller distortions, is very similar to that of molecular vibrations. Consequently I was able to make the calculations which I performed do double duty using them also in connection with the theory of paramagnetic relaxation caused by spin-lattice coupling. The work I have discussed so far all has related mainly to static susceptibilities but when I visited Leiden in 1938, Gorter (34) aroused my interest in the behavior of the susceptibility at radio frequencies and related problems in relaxation. In a landmark pioneer paper written in 1932 Waller (35) showed that there could be a transfer of energy between the magnetic and phonon systems because of the modulation of the dipolar energy by the lattice vibrations, and a little later Heitler and Teller, Fierz, and Kronig (36) showed that there could be a

similar relaxation effect, usually of larger magnitude, because of the vibrational modulations of the energy associated with the crystalline potential. I made a more detailed explicit calculation (37) of the numerical values of the relaxation times to be expected for titanium, chronium and ferric ions. On the whole the agreement with experiment was rather miserable. In an attempt to explain away part of the disgreement, I suggested in another paper (38) that there might be what is usually called a phonon bottleneck. The point is that because of the conservation of energy only a portion of all the phonons, those in a narrow frequency range, can exchange energy with the spin or magnetic system. Because of their limited heat capacity, these phonons are easily saturated and brought to the same temperature as the spin system, except insofar as they exchange energy by anharmonic processes coupling them to other oscillators, or transport the excess energy to a surrounding bath that serves as a thermostat. Consequently the relaxation process may be considerably slower than one would calculate otherwise.

This brings me up to the years of world war II, during which very little was done in the way of pure research. Even before the war, the number of physicists interested in magnetism was limited, both because at that time there were few theoretical physicists in the world, and because there were many different fields in which quantum mechanics could be applied. So I seldom ran into problems of duplicating the work of other physicists, except for the calculations with the rotating dipole I mentioned near the beginning of my talk, and some duplication with Kronig on paramagnetic relaxation. As an example of the rather relaxed rate of development I might mention that while the first successful experiments on adiabatic demagnetization were made by Giauque (39) at California in 1933, the first attempt to interpret these experiments in the light of crystal field theory was not until Hebb and Purcell (40) published an article in 1937 which was essentially a term paper in my course in magnetism which had only two students. Shortly after the war, the whole tempo of research in magnetism changed abruptly. The development of radar in the war created apparatus and instruments for microwave spectroscopy, permitting exploration of a spectral low frequency spectral region previously practically untouched. Also infrared and optical spectroscopy of solids was pursued much more vigorously, with improved apparatus. On the theoretical side, crystalline and ligand field calculations were made in various centers, notably in Japan, going into much more detail and lengthy computation than in the work of my group at Wisconsin in the 1930's.

For the rare earths the pre-war period may be described as the era of the rare earth sulphate octohydrates, as the meager magnetic measurements at that time were mainly on these compounds. These materials are particularly annoying as they have a very complicated crystal structure, with eight rare earth ions in the unit cell. However, the x-ray analysis (41) that yielded this disconcerting information had not been made at the time of Penney and Schlapp's work, and so they obtained the theoretical curve shown in Fig. 3, by making faute de mieux the simplifying assumption that the local crystalline field had cubic symmetry, and was the same for all the paramagnetic ions.

Undoubtedly the local potential is more complicated. Even today there have been few attempts to revaluate the crystalline field parameters for sulphate octohydrates, both because of theoretical complexity and the paucity of new experimental data. The most comprehensive crystalline field analysis for rare earth salts in modern times is on the ethyl sulphates ($Re(C_2H_5SO_4)_3 9H_2O$), which have only one ion in the unit cell and are magnetically dilute. One important result is that the higher order harmonics in the series development of the crystalline potential are much more important than one thought in the early days. These ethyl sulphates have hexagonal symmetry. Were only second order terms important, the crystalline potentia would be of the simple type (7a), but actually there are also important terms involving fourth and sixth order harmonics, including those of the type $(x \pm iy)^6$. One sometimes worries how meaningful and reliable are the crystalline field parameters deduced from spectroscopic data, but very comforting magnetic measurements have been published by Cooke and collaborators (42). They measured the susceptibility both parallel and perpendicular to the hexagonal axis, and as shown in Fig. 6 found that the experimental results agreed exceedingly well with the theoretical curve calculated with the spectroscopically determined (43) crystalline field parameters.

One of the spectacular developments associated with spectroscopy of the solid state was the first optical laser constructed by Maiman (44) in 1960. By a sheer coincidence it involved transitions between the same ruby energy levels that were interpreted in terms of crystal field theory by Finkelstein and myself (45) in 1940. Cynics can well claim that our theoretical labelling of the energy levels was no more germane to the successful instrumentation of a laser than the prior naming of a star was to astrophysical studies thereof. Still it may be true that any theoretical understanding of the nature and

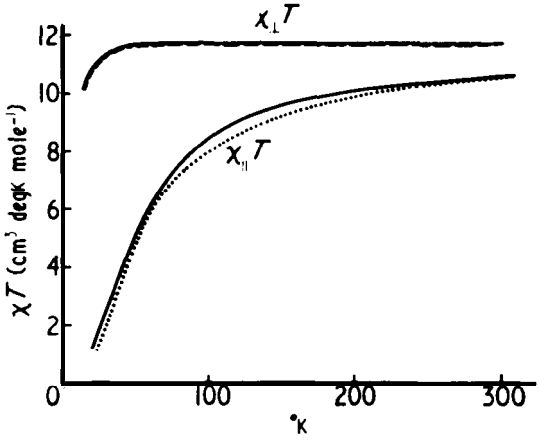


Fig. 6. The product of susceptibility times temperature for erbium ethyl sulphate as a function of temperature for directions parallel and perpendicular to the hexaconal axis. The broken curves represent experimental measurements of the susceptibility by Cooke, Lazemby and Leark, (42) the solid curves are calculated theoretically with the crystalline field parameters of Erath. (43)

relaxation rates of the different energy levels in solids may help the experimentalists a little.

Particularly gratifying to me were the improved determinations of spin-lattice relaxation times made at various laboratories (46). These confirmed the reality of the bottleneck effect. They also verified the proportionality of the relaxation time in a certain temperature range to T^{-9} which I had predicted for salts with Kramers degeneracy and of sufficient magnetic dilution that there is no bottleneck.

The year 1946 brought about the discovery of nuclear magnetic resonance independently by Purcell, Torrey and Pound, and by Bloch, Hansen and Packard (47). I need not tell you how enormously important the field of nuclear magnetism has become both for its basic scientific interest and its surprising technological applications. The nuclear magnetic resonance spectrometer has become a standard tool for any laboratory concerned with analytical chemistry, completely usurping the role of the Bunsen burner in earlier days. Measurements of transferred hyperfine-structure give a quantitative measure of incipient covalence in molecular orbital or ligand field theory. Little of my own research. has been concerned with. nuclear magnetism, but in 1948 Purcell asked me if I could explain theoretically the size of the line widths he and Pake (48) were observing in the resonance of the F nucleus in CaF_2. It occurred to me that this could be done by applying the method of moments that Waller (35) developed in 1932. The predicted magnitude of the mean square line breadth and its dependence on direction agreed on the whole very well with experiment. The only difference in this calculation (49) of the mean square dipolar broadening as compared with that originally performed by Waller is that he was concerned with the width in a weak magnetic field, whereas in the experiments by Pake and Purcell the dipolar energy is small compared to the Zeeman energy, and this necessitates the truncation of the Hamiltonian function, i.e., the omission of certain terms. A year previously I had also used Waller's method of moments in connection with explaining some apparently anomalous line shapes in some of the Leiden experiments on paramagnetic dispersion. Gorter was a visiting Professor at Harvard in 1947, and one morning we came to the laboratory and discovered that we had both overnight come to the conclusion that the explanation is to be found in an effect now generally known as exchange narrowing. Gorter had reached this conclusion on the basis of an intuitive picture, that the spin waves associated with exchange spoiled the coherence of the dipolar coupling, analogous to the motional narrowing discussed by Bloembergen, Purcell, and Pound in connection with nuclear magnetic resonance in liquids (50). On the other hand I used a more mathematical approach,, showing that exchange enhanced the fourth but not the second moment, thereby narrowing the line. The result was a joint paper by Gorter and myself (5 1).

So far I have not said much about ferromagnetism, partly because more of my own work has been in paramagnetism, but mainly because most ferromagnetic metals are very complicated since they are conductors. Over the years there have been arguments *ad infinitum* as to which is the best model to

use, each researcher often pushing his own views with the ardor of a religious zealot (52). Heisenberg's original model (14) was one in which the spins responsible for the ferromagnetism did not wander from atom to atom, whereas in the band picture developed by Stoner (53) the electrons carrying a free spin can wander freely through the metal without any correlation in their relative positions, as the exchange effects are approximated by an uncorrelated molecular field. Undoubtedly the truth is between the two extremes, and I have always favoured as a first approximation a sort of compromise model, which may be called that of minimum polarity (54). In nickel for instance, this model there is continual interchange of electrons between the configurations d^{10} and d^{9} but no admixture of d^{8}, d^{7} etc. as then the correlation energy is increased.

Neutron diffraction is a very powerful new tool for disclosing how atomic magnets are arranged relative to each other. It has led to the surprising and spectacular discovery that in certain materials notably rare earth metals, the elementary magnets are arranged in a spiral conical or wavy fashion, rather than pointing all in the same direction within an elementary domain (55). They can be ferromagnetic in one temperature region and antiferromagnetic in another. This weird kind of magnetism is sometimes called helical magnetism. Most rare earth metals belong to this category and the mathematical interpretation of the experimental results is complicated and difficult despite the fact that the *4f* electrons participate but little in electrical conductivity, unlike the *3d* electrons in iron or nickel. I have not been involved in any of the theoretical analysis except for a point connected with the magnetic anisotropy. When I attended the conference on quantum chemistry sponsored by Professor Lowdin in Florida in 1971, Bozorth presented some measurements

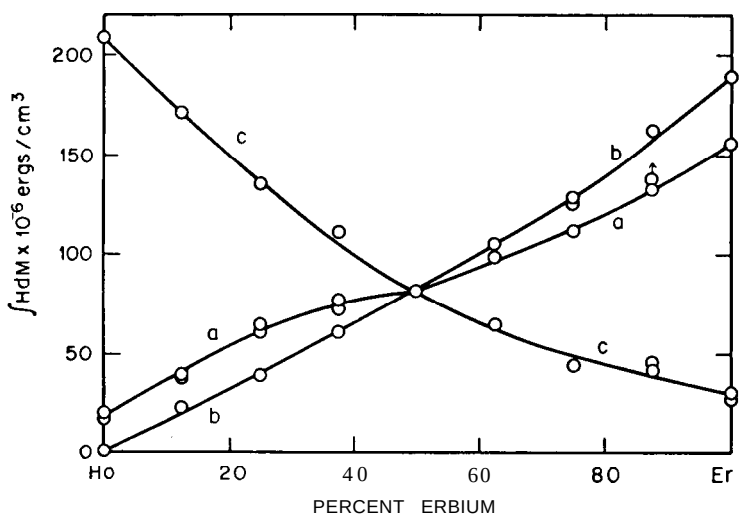


Fig. 7. The energy of magnetization for various amounts of Ho relative to Er. The three curves are for three different directions and would coincide were there is no magnetic anisotropy. The latter is measured by the differences between the ordinates of the three curves.

of the ferromagnetic anisotropy of Ho-Er alloys. He found that the anisotropy of pure holmium was approximately the negative of that of erbium, and vanished when there was an equal amount of Ho and Er, as shown on Fig. 7. It finally occurred to me that precisely the same property of spherical harmonics that explained the inversion of Fig. 4 in passing from Co^{++} to Ni^{++} also explained (56) the inversion of the anisotropy of Ho as compared to Er, with the obvious corollary that the Ho and Er contributions should cancel each other out for a 50% mixture. So sometimes primitive theory can still be useful, but in general a higher degree of mathematical sophistication is required as time progresses, and as more and more exotic magnetic phenomena are discovered by the experimentalists. This you will learn from the addresses by Anderson and Sir Neville Mott but one can still say that quantum mechanics is the key to understanding magnetism. When one enters the first room with this key there are unexpected rooms beyond, but it is always the master key that unlocks each door.

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Philip W. Anderson

PHILIP W. ANDERSON

My father, Harry Warren Anderson, was a professor of plant pathology at the University of Illinois in Urbana, where I was brought up from 1923 to 1940. Although raised on the farm-my grandfather was an unsuccessful fundamentalist preacher turned farmer-my father and his brother both became professors. My mother's father was a professor of mathematics at my father's college, Wabash, in Crawfordsville, Indiana, and her brother was a Rhodes Scholar, later a professor of English, also at Wabash College; on both sides my family were secure but impecunious Midwestern academics. At Illinois my parents belonged to a group of warm, settled friends, whose life centered on the outdoors and in particular on the "Saturday Hikers", and my happiest hours as a child and adolescent were spent hiking, canoeing, vacationing, picnicking, and singing around the campfire with this group. They were unusually politically conscious for that place and time, and we lived with a strong sense of frustration and foreboding at the events in Europe and Asia. My political interests were later strengthened by the excesses in the name of "security" and "loyalty" of the "McCarthy" years, to the extent that I have never accepted work on classified matters and have from time to time worked for liberal causes and against the Vietnam war.

Among my parents' friends were a number of physicists (such as Wheeler Loomis and Gerald Almy) who encouraged what interest in physics I showed. An important impression was my father's one Sabbatical year, spent in England and Europe in 1937. I read voraciously, but among the few intellectual challenges I remember at school was a first-rate mathematics teacher at the University High School, Miles Hartley, and I went to college intending to major in mathematics. I was one of several students sent to Harvard from Uni High in those years on the new full-support National Scholarships. The first months at Harvard were more than challenging, as I came to the realization that the humanities could be genuinely interesting, and, in fact, given the weaknesses of my background, very difficult. Nonetheless in time I relaxed and enjoyed the experience of Harvard, and was in the end pleasantly surprised to come out with a good record.

In those wartime years (1940-43) we were urged to concentrate in the immediately applicable subject of "Electronic Physics" and I was then bundled off to the Naval Research Laboratory to build antennas (1943-45). (It may be remembered that such war work was advisable for those of us who wore glasses, the "services" at that time being convinced that otherwise we would be best utilized as infantry.) This work left me with. a lasting admiration for Western Electric equipment and Bell engineers, and for the competence of ‘

my former physics (not electronics) professors at Harvard; after the war, I went back to learn what the latter could teach me.

Graduate school (1945-49) consisted of excellent courses; a delightful group of friends, including for instance Dave Robinson and Tom Lehrer, centered around bridge, puzzles, and singing; a happy decision that Schwinger and Q.E.D. would lead only to standing in the long line outside Schwinger's office, whereas van Vleck, whom I already knew from undergraduate school and a wartime incident, seemed to have time to think about what I might do; meeting and marrying one summer the niece of old family friends, Joyce Gothwaite, and therefore settling down to work on my problem. Further motivation was provided by the birth of a daughter, Susan. When I did settle down, I rather suddenly came to realize that the sophisticated mathematical techniques of modern quantum field theory which I was learning in advanced courses from Schwinger and Furry were really genuinely useful in the experimental problem of spectral line broadening in the new radio-frequency spectra, just then being exploited because of wartime electronics advances. Although I didn't know it, across the world-in England with Fröhlich and Peierls, in Princeton with Bohm and later Pines, and in Russia with Bogoliubov and especially Landau-the new subject of many-body physics was being born from similar marriages of maturing mathematical techniques with new experimental problems.

In spite of a number of contretemps, with the help of Van and of an understanding recruiter, Deming Lewis, who seemed to be the only person who believed me when I said I had solved my problem and wanted to do something else, I got to Bell Laboratories to work with the constellation of theorists who were then there : Bill Shockley, John Bardeen, Charles Kittel, Conyers Herring, Gregory Wannier, Larry Walker, John Richardson, and later others. Kittel in particular fostered my interest in linebroadening problems and introduced Wannier and me to antiferromagnetism, while Wannier taught me many fundamental techniques, and Herring put me in touch with the ideas of Landau and Mott and kept us all abreast of the literature in general. I learned crystallography and solid state physics from Bill Shockley, Alan Holden, and Betty Wood. And I learned most of all the Bell mode of close experiment-theory teamwork-at first with Jack Galt, Bill Yager, Bernd Matthias, and Walter Merz.

Much of the rest is a matter of record. One important experience was Ryogo Kubo's convincing the Japanese in 1952 that they should invite as their first Fulbright scholar in physics an unknown 28-year-old. This Sabbatical was postponed to 1953, the year of the Kyoto International Theoretical Physics Conference, which was dominated by Mott as the president of IUPAP, and was my first meeting with many other friends of later years. Lecturing has never come easily to me, but I gave, as best I could, lectures on magnetism and a seminar on linebroadening which included Kubo, Toru Moriya, Kei Yosida, Jun Kanamori, among other wellknown Japanese solid staters. I acquired an admiration for Japanese culture, art, and architecture, and learned of the existence of the game of GO, which I still play.

Another milestone for me was a year at the Cavendish Laboratory and Churchill College (1961-62) which was not at Oxford because Brian Pippard promised me that I could lecture and that the lectures would be attended. Mott kept asking me what my 1958 paper meant, and there were a lot of discussions centered around broken symmetry and some ideas of Brian Josephson, who attended my lectures.

When he left Princeton for Illinois in 1959, David Pines bequeathed me a French student named Pierre Morel; Morel and I worked in 1959--61 on some unconventional ideas on anisotropic superfluidity I had, which became related to He, by discussions with Keith Brueckner; later we worked on solving the Eliashberg equations for superconductivity. Some of these ideas came to fruition working with a young experimentalist, John Rowell, on my return to Bell: we discovered the Josephson effect and worked on "phonon bumps".

In 1967 Nevill Mott managed what must have been a most difficult arrangement to steer through the Cambridge system : a permanent “Visiting Professorship” for two terms out of three at the Cavendish. This arrangement would have been totally impossible without the self-effacing and unsparing cooperation of Volker Heine who joined with me in leading the “TCM Group” (Theory of Condensed Matter) for eight productive and exciting years, spiced with warm encounters with students, visitors and associates from literally the four corners of the earth. One of our brainchildren is a still viable Science and Society course. Through the good offices of John Adkins, Jesus College gave me a Fellowship for this period. A souvenir of those years is a small cottage on the cliffs of Cornwall, where Joyce and I spend a spring month every year, hiking and seeing friends. After eight years the sense of being tourists in each of two cultures, with no really satisfactory role in either, led us reluctantly to return to the United States, and in 1975 the job at Cambridge was replaced with a half-time appointment at Princeton.

The years since the Nobel prize have been productive ones for me. For instance, in 1978, shortly after receiving the prize in part for localization theory, I was one of the “Gang of Four” (with Elihu Abrahams, T.V. Ramakrishnan, and Don Licciardello) who revitalized that theory by developing a scaling theory which made it into a quantitative experimental science with precise predictions as a function of magnetic field, interactions, dimensionality, etc.; a major branch of science continues to flow from the consequences of this work. (Most recently, “photon localization” has been in the news.)

In 1975 S. F. (now Sir Sam) Edwards and I wrote down the “replica” theory of the phenomenon I had earlier named “spin glass”, followed up in ‘77 by a paper of D. J. Thouless, my student Richard Palmer, and myself. A brilliant further breakthrough by G. Toulouse and G. Parisi led to a full solution of the problem, which turned out to entail a new form of statistical mechanics of wide applicability in fields as far apart as computer science, protein folding, neural networks, and evolutionary modelling, to all of which directions my students and/or I contributed. The field of quantum

valence fluctuations was another older interest which became much more active during this period, partly as a consequence of my own efforts.

Finally, in early 1987 the news of the new "high-T," cuprate superconductors galvanized the world of many-body quantum physics, and led many of us to reexamine older ideas and dig for new ones. Putting together a cocktail of older ideas of my own (the "RVB" singlet pair fluid state) and of many others, mixed with brand new insights, I have been able to arrive at an account of most of the wide variety of unexpected anomalies observed in these materials. The theory involves a new state of matter (the two-dimensional "Luttinger liquid") and a quite new mechanism for electron pairing ("deconfinement"). Experimental confirmations of the predictions of this theory are appearing regularly.

The prize seemed to change my professional life very little. Management chores at AT&T Bell Labs continued and culminated in an informal arrangement as consultant for the new Vice President of Research, Arno Penzias, during the first two years of his tenure, which coincided with the first difficult years of "divestiture" for the AT&T company. I thereupon gratefully retired in 1984 from Bell and am now full-time Joseph Henry Professor of Physics at Princeton. I served a 5-year stint as Chairman of the Board of the Aspen Center for Physics, retiring 3 years ago, and for 4 years was on the Council and Executive Committee of the American Physical Society. Since 1986 or so I have been deeply involved (though officially I am merely a co-vice-chairman) with a new, interdisciplinary institution, the Sante Fe Institute, dedicated to emerging scientific syntheses, especially those involving the sciences of complexity. Two other Nobelists are involved: Murray Gell-Mann, who is our science board chairman and an eloquent spokesperson for our ideas and ideals; and Ken Arrow, with whom I co-chaired the workshops founding our interdisciplinary study of the bases of economic theory. My own work in spin glass and its consequences has formed some of the intellectual basis for these interests.

The Nobel prize gives one the opportunity to take public stands. I happened to be in a position to be caught up in the campaign against "Star Wars" very early (summer '83) and wrote, spoke and testified repeatedly, with my finest moment a debate with Secretary George Schultz in the Princeton Alumni Weekly, reprinted in Le *Monde* in 1987. I have also testified repeatedly and published some articles in favor of Small Science.

Some further honors after the Nobel prize of which I am particularly conscious were the National Medal of Science; an ScD from my father's, mother's, sister's and wife's Alma Mater, the University of Illinois; foreign membership in the Royal Society, the Accademia Lincei, and the Japan Academy; and honorary fellowship of Jesus College, Cambridge.

We have kept our cottage on the cliffs of Cornwall, and our custom of seeing English and other friends in April there. We abandoned our much loved house, designed by Joyce, in New Vernon near Bell Labs for another of her good designs on some brushy acres with a view across the Hopewell

Valley near Princeton. Susan is established as a painter in Boston of, at the moment, primarily scenes of Martha's Vineyard, and teaching some drawing classes at MIT. A prize of which I am, vicariously, enormously proud is the designation as Northeast U.S. Tree Farmers of the Year earned by my sister and her husband of New Milford, Pa in 1990.

VITA

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Birth Date: December 13, 1923, Indianapolis, Indiana. B.S. summa cum laude Harvard, '43; MA '47, PhD '49, MA (Cantab) '67. M. 1947, Joyce Gothwaite, d. Susan Osborne b. 1948. Naval Research Lab. 1943-5 (Chief Spec. X, USNR 1944-5) Bell Tel Labs. 1949-53, 1954--; Memb. Tech. Staff to 1959, Dept. Head 1959-61, MTS 1961-74, Asst. Dir. 1974-76, Consult. Dir. 1977-. Fulbright Lecturer, Tokyo, 1953-4; Visiting Fellow, Churchill College, Cambridge, 1961-2; O.E. Buckley Prize A.P.S. 1964; Am. Acad. A&S 1966, N.A.S. 1967; Visiting Prof. Theor. Phys. U. of Cambridge 1967-75, Fellow Jesus College 1969-75; Prof. of Physics, Princeton U. 1975--; Dannie Heinemann Prize, Gottingen Acad. Sci. 1975.

LOCAL MOMENTS AND LOCALIZED STATES

Nobel Lecture, 8 December, 1977

bY
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I was cited for work both. in the field of magnetism and in that of disordered systems, and I would like to describe here one development in each held which was specifically mentioned in that citation. The two theories I will discuss differed sharply in some ways. The theory of local moments in metals was, in a sense, easy: it was the condensation into a simple mathematical model of ideas which. were very much in the air at the time, and it had rapid and permanent acceptance because of its timeliness and its relative simplicity. What mathematical difficulty it contained has been almost fully- cleared up within the past few years.

Localization was a different matter: very few believed it at the time, and even fewer saw its importance; among those who failed to fully understand it at first was certainly its author. It has yet to receive adequate mathematical treatment, and one has to resort to the indignity of numerical simulations to settle even the simplest questions about it. Only now, and through primarily Sir Nevill Mott's efforts, is it beginning to gain general acceptance.

Yet these two finally successful brainchildren have also much in common: first, they flew in the face of the overwhelming ascendancy. at the time of the band theory of solids, in emphasizing locality: how a magnetic moment, or an eigenstate, could be permanently pinned down in a given region. It is this fascination with the local and with the failures, not successes, of band theory, which the three of us here seem to have in common. Second, the two ideas were born in response to a clear experimental signal which contradicted the assumptions of the time; third, they intertwine my work with that of my two great colleagues with whom I have been jointly honored; and fourth, both subjects are still extremely active in 1977.

I. The "Anderson Model" : Local Moments in Metals

To see the source of the essential elements of the model I set up for local moments in metals, it will help to present the historical framework. Just two years before, I had written a paper on "superexchange" (1) discussing the source and the interactions of the moments in insulating magnetic crystals such as MnO, $CuSO_4$. $5H_2O$, etc. I had described these substances as what we should now call "Mott insulators" on the insulating side of the Mott transition, which unfortunately Sir Nevill says he will not describe. Briefly, following a suggestion of Peierls, he developed the idea that these magnetic insulating salts were so because to create an ionized electronic excitation would require an additional excitation energy U, the energy necessary to change the configurations of two distant atoms from $d^n + d^n$ to $d^{n-1} + d^{n+1}$. This energy *U*

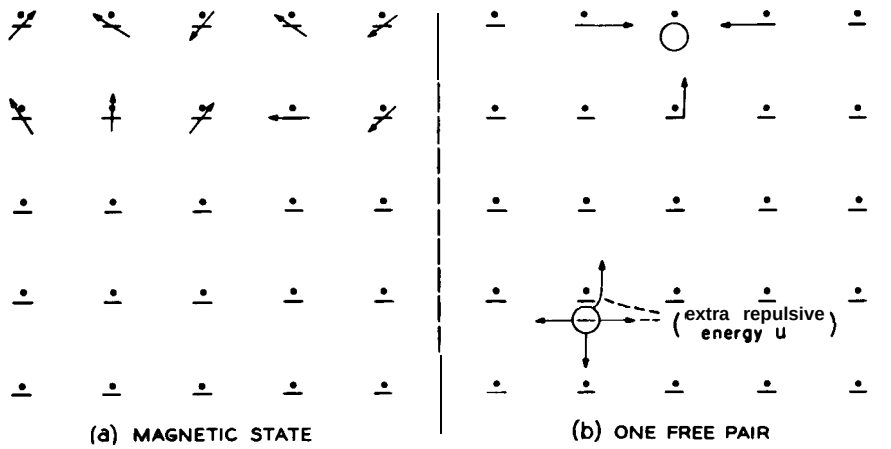


Fig. 1. Mott-Peierls mechanism for magnetic state. State with free pair has extra repulsive energy "U" of two electrons on same site.

is essentially the Coulomb repulsive energy between two electrons on the same site, and can be quite large (see Fig. 1). To describe such a situation, I set up a model Hamiltonian (now called the "Hubbard" Hamiltonian).

$$H = \sum_{i,j,\sigma} b_{ij}\, c_{i\sigma}\, c_{j\sigma} + \sum_{i} U n_{i\uparrow} n_{i\downarrow} \tag{1}$$

Here b_{ij} represents the amplitude for the electron to "hop" from site to site—such hops as shown in Fig. 1, right half—and U the repulsion energy between two opposite spin electrons on the same site (parallel, of course, being excluded). With (1)—appropriately generalized—it was possible to understand the predominantly antiferromagnetic interactions of the spins in these Mott insulators, which include the ancient "lodestone" or magnetite, as well as the technically important garnets and ferrites. These interactions are caused by the virtual hopping of electrons from a site to its neighbor and return, which is only

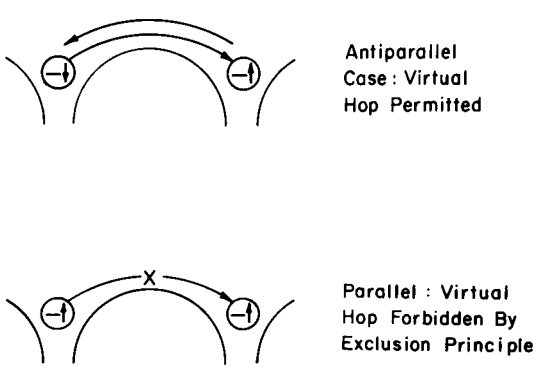


Fig. 2. Virtual hopping as the origin of superexchange.

possible for antiferromagnetism, where the requisite orbital is empty. From simple perturbation theory, using this idea,

$$\mathcal{J}_{ij} = \frac{-2b^2_{ij}}{U} \tag{2}$$

where b represents the tendency of electrons to hop from site to site and form a band. (The provenance of (2) is made obvious in Fig. 2.) In fact, I showed later in detail (2) how to explain the known empirical rules describing such interactions, and how to estimate parameters b and U from empirical data.

The implications for magnetism in metals-as opposed to insulators--of this on-site Coulomb interaction U were first suggested by Van Vleck and elaborated in Hurwitz' thesis (3) during the war, and later in a seminal paper which. I heard in 1951, published in 1953 (4). Also, very influential for me was a small conference on magnetism in metals convened at Brasenose College, Oxford, September 1959, by the Oxford-Harwell group, where I presented some very qualitative ideas on how magnetism in the iron group might come about. More important was my first exposure to Friedel's and Blandin's ideas on resonant or virtual states (5, 6) at that conference. The essence of Friedel's ideas were 1) that impurities in metals were often best described not by atomic orbitals but by scattering phase shifts for the band electrons, which would in many cases be of resonant form; 2) that spins in the case of magnetic impurities might be described by spin-dependent scattering phase shifts.

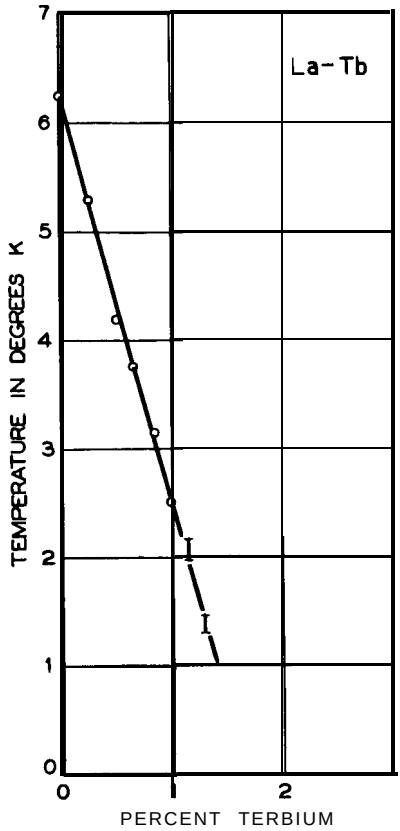
Matthias and Suhl, at Bell, were at that time much involved in experiments and theory on the effect of magnetic scatterers on superconductivity (7). For many rare earth atoms, the decrease in T_c due to adding magnetic impurities is clear and very steep; (see Fig. 3a), and even steeper for most transition metal impurities. For instance, Fe at the 10^{-5} level completely wipes out superconductivity in Mo. But in many other cases, e.g., Fe in Ti, a nominal11 magnetic atom had no effect, or raised T_c (as in Fig. 3b). A systematic study of the occurrence of moments was carried out by Clogston et al (8). As yet, no real thought (except see Ref. (6)) had been given to what a magnetic moment in a metal *meant:* the extensive investigations of Owen *et al* (9) and of Zimmermann (10), for instance, on Mn in Cu, and the Yosida calculation (11), essentially postulated a local atomic spin given by God and called S, connected to the free electrons by an empirical exchange integral $\mathcal{J}$; precisely what we now call the "Kondo Hamiltonian":

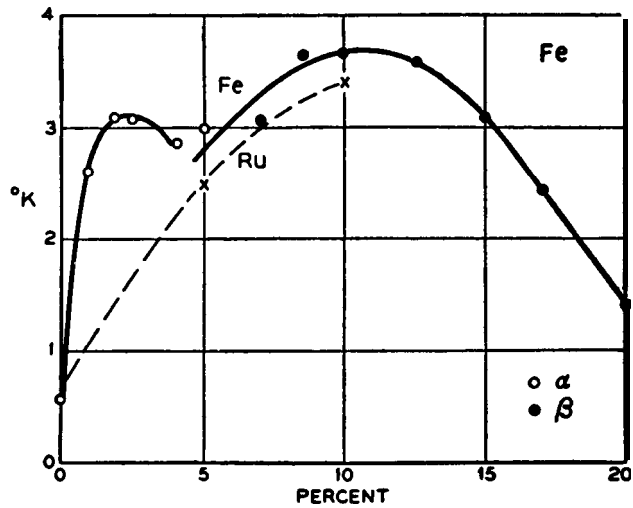
$$H = \sum_{k\sigma} \varepsilon_k \, n_{k\sigma} + \mathcal{J} \, \vec{S} \cdot \vec{s} \tag{3}$$

$$\text{where } s = \sum c^{+}_{k\sigma} \, \vec{\sigma}_{\sigma\sigma'} \, c_{k'\sigma'}$$

is the local spin density of free electrons at the impurity.

The "Anderson model" (12) is the simplest one which provides an electronic mechanism for the existence of such a moment. We insert the vital on-site exchange term U, and we characterize the impurity atom by an additional orbital φ_d, with occupancy $n_{d\sigma}$ and creation operator $c^{+}_{d\sigma}$, over and above





Superconducting transition temperatures of iron or ruthenium solid solutions in titanium.

Fig. 3. Effect of magnetic impurities on T_c of a superconductor (a) ; when nonmagnetic T_c goes up (b).

the free electron states near the Fermi surface of the metal (the obvious over-completeness problem is no real difficulty, as I showed later (13). The physics should be clear by reference to Fig. 4. The Hamiltonian is

$$H = \sum_{k\sigma} \varepsilon_k \, n_{k\sigma} + U n_{d\uparrow} n_{d\downarrow} + E_d \, (n_{d\uparrow} + n_{d\downarrow}) \tag{4}$$

$$+ \sum_{k\sigma} V_{dk} \, (c_{d\sigma} c_{k\sigma} + cc)$$

where in addition to free electrons and the magnetic term U, we have a d-to-k tunneling term V_{dk} representing tunneling through the centrifugal barrier which converts the local orbital φ_d into one of Friedel's resonances. The resonance would have a width

$$\Delta = \pi < V^2_{dk} > \varrho(E_d) \tag{5}$$

and in the absence of U would be centered at E_d, the energy of the d resonance (if the density of states ϱ is sufficiently constant—see Fig. 4 again).

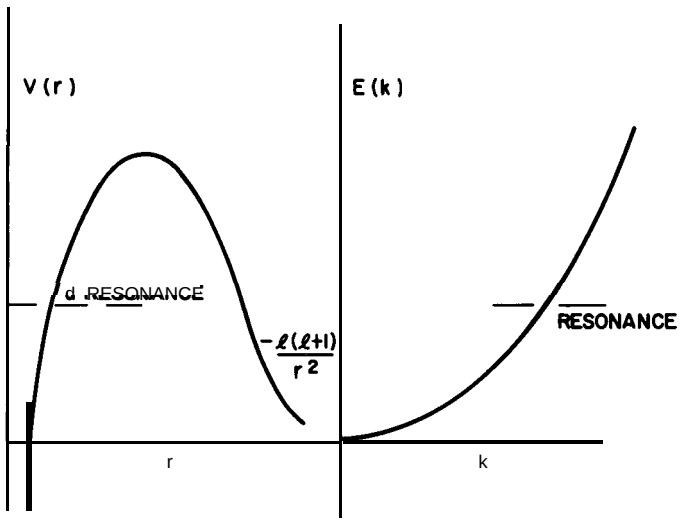


Fig. 4. d-Resonance due to tunneling through the centrifugal barrier.

A simple Hartree-Fock solution of this Hamiltonian showed that if E_d is somewhat below E_F, and if $\Delta/U < \pi$, the resonance will split as shown in Fig. 5 (from the original paper). One has two resonances; one for each sign of spin, a mostly occupied one below the Fermi level and a mostly empty one above. This leads to a pair of equivalent magnetically polarized solutions, one for each direction of spin. In these solutions, the local state φ_d is mixed into scattered free-electron states: there are no local bound electronic states, but there is a local moment. Again, in Hartree-Fock theory, the magnetic region is shown in Fig. 6. The parameters could be estimated from chemical data or from first principles, and it was very reasonable that Mn or Fe in

polyelectronic metals should be non-magnetic as was observed, but magnetic in, for instance, Cu.

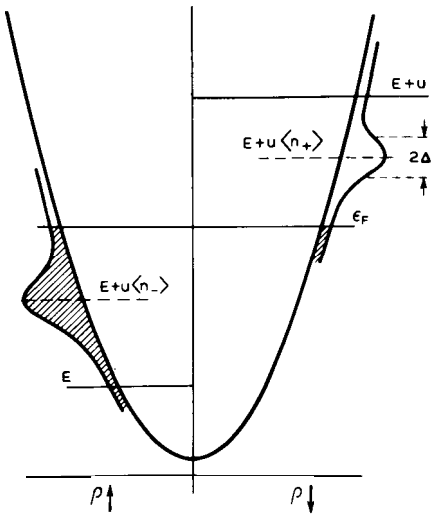


Fig. 5. Spin-split energy levels in the magnetic case.

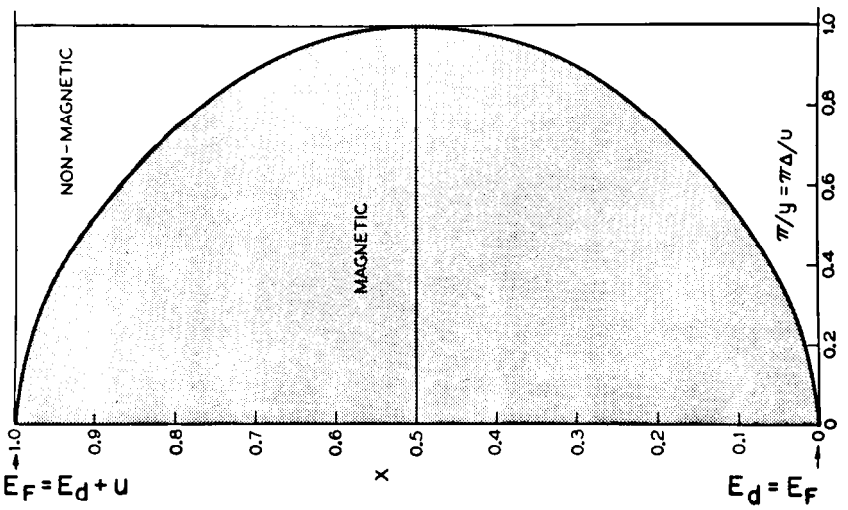


Fig. 6. Magnetic region of parameter space in the "Anderson Model".

This seems and is a delightfully simple explanation of a simple effect. The mathematics is shamelessly elaborated (or simplified) from nuclear physics (Friedel's improvements on Wigner's theory of resonances) and similar **things** occur in nuclear physics called "analog resonances". Nonetheless, it has led to an extraordinary and still active ramification of interesting physics.

Before discussing some of these branchings, let me say a bit about the model's simplicity, which is to an extent more apparent than real. The art of

model-building is the exclusion of real but irrelevant parts of the problem, and entails hazards for the builder and the reader. The builder may leave out something genuinely relevant; the reader, armed with too sophisticated an experimental probe or too accurate a computation, may take literally a schematized model whose main aim is to be a demonstration of possibility. In this case, I have left out (1) the crystal structure and in fact the atomic nature of the background metal, which is mostly irrelevant indeed. (2) The degeneracy of the d level, which leads to some important physics explored in an Appendix of the paper and later and much better by Caroli and Blandin (14). In the Appendix I showed that if the resonance was sufficiently broad compared to other internal interactions of the electrons in the *d* orbitals, the different *d* orbitals would be equally occupied as is usually observed for transition metal impurities; in the opposite case the orbital degrees of freedom will be "unquenched", as is almost always the case for rare earth atoms. (3) Left out are all correlation effects except *U;* this relies on the basic "Fermi liquid" idea that metallic electrons behave as if free, but detaches all parameters from their values calculated naively: they are *renormalized,* not "bare" parameters. This is the biggest trap for the unwary, and relies heavily on certain fundamental ideas of Friedel on scattering phase shifts and Landau on Fermi liquids. I have also left out a number of real possibilities some of which we will soon explore.

One of my strongest stylistic prejudices in science is that many of the facts Nature confronts us with are so implausible given the simplicities of nonrelativistic quantum mechanics and statistical mechanics, that the mere demonstration of a reasonable mechanism leaves no doubt of the correct explanation. This is so especially if it also correctly predicts unexpected facts such as the correlation of the existence of moment with low density of states, the quenching of orbital moment for all d-level impurities as just described, and the reversed free-electron exchange polarization which we shall soon discuss. Very often such, a simplified model throws more light on the real workings of nature than any number of "ab initio" calculations of individual situations, which even where correct often contain so much detail as to conceal rather than reveal reality. It can be a disadvantage rather than an advantage to be able to compute or to measure too accurately, since often what one measures or computes is irrelevant in terms of mechanism. After all, the perfect computation simply reproduces Nature, does not explain her.

To return to the question of further developments from the model : I should like to have had space to lead you along several of them. Unfortunately, I shall not, and instead, I shall show you a Table of the main lines, and then follow one far enough to show you an equation and a picture from the recent literature.

The one of these lines I would like to take time to follow out a bit is the "model" aspect I. This started as a very physical question: what is the sign and magnitude of the spin-free electron interaction? Already in '59 *before* the model appeared, I made at **the** Oxford Discussion a notorious bet of one pound with (now Sir) Walter Marshall that the free-electron polarization caused by the spins in metals would be negative, for much the same reason as in

Table 1 : Ramifications of the Anderson Model

I.	AM as an exact field-theoretic model-see text: a) AM = Kondo; Anderson, Clogston, Wolff, Schrieffer b) Fundamental difficulties of both: Alexander, Schrieffer, Kondo, Suhl, Nagaoka, Abrikosov c) Solution of Kondo: PWA, Yuval, Hamann, Yosida, Wilson, Nozieres, etc. d) Solution of AM: Hamann, Wilson, Krishna-Murthy, Wilkins, Haldane, Yoshida, etc.
II.	"Microcosmic" view of magnetism in metals; interacting AM's and rules for alloy exchange interactions, Alexander, PWA (15), Moriya (16)
III.	Applications to Other Systems a) Adatoms and molecules on surfaces, Grimley (17), Newns (18), etc. b) Magnetic impurities in semiconductors, Haldane (19) c) With screening+phonons, - U: mixed valence, surface centers, etc., Haldane (20) The sky seems to be the limit.

superexchange: the occupied spin state *below* the Fermi level is repulsive, that above is attractive because it can be occupied by the free electrons of the same spin. Clogston and I published this for the Anderson model (21). This was formalized by Peter Wolff, and published later with. Schrieffer (22), into a perturbative equivalence of "Kondo" and "Anderson" models with the exchange integral $\mathcal{J}$ of (3) being

$$\mathcal{J} = \frac{2\Delta}{\pi}\left(\frac{1}{E_d} - \frac{1}{E_d+U}\right) \tag{6}$$

$$(\Delta = \pi < |V_{dk}|^2 > \varrho(E_F))$$

Soon, however, it came to be realized that neither Kondo nor Anderson models behaved reasonably at low temperatures (Kondo (23), Suhl (24), Schrieffer (25), etc.), but exhibited nasty divergences at low temperatures which seemed to signal disappearance of the local moment. The best physical description of what happens (for a more extensive review for nonspecialists perhaps my series of papers in Comments on Solid State Physics will suffice) is that at high temperatures or on high energy (short time) scales, the Hartree-Fock theory given above is correct, and there is a free spin. But as the energy scale is lowered, the effective antiferromagnetic coupling between this spin and the free-electron gas "bootstraps" itself up to a very large value, eventually becoming strong enough to bind an antiparallel electron to it and become non-magnetic. This is a very precise analog of the process of continuous "confinement" of the color degrees of freedom of modern quark theories (26) and is a delightful example of the continuing flow of ideas and techniques back and forth between many-body physics and quantum field theory.

In the past few years extensive investigations via renormalization group theory (which, in a nearly modern form, was first applied to this problem (27)) have led to the essential solution of this "Kondo problem". A very succinct way of describing that solution is the computation of the scaling of the susceptibility as a function of temperature by Wilson (28) (Fig. 7). For comparison, and to show the remarkable precision of the Schrieffer-Wolff transformation,

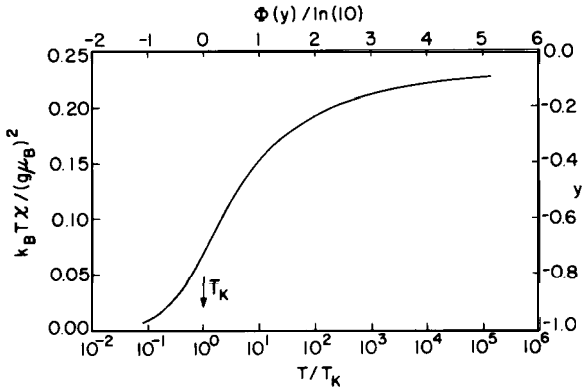


Fig. 7. Susceptibility of the Kondo Model as calculated by Wilson.

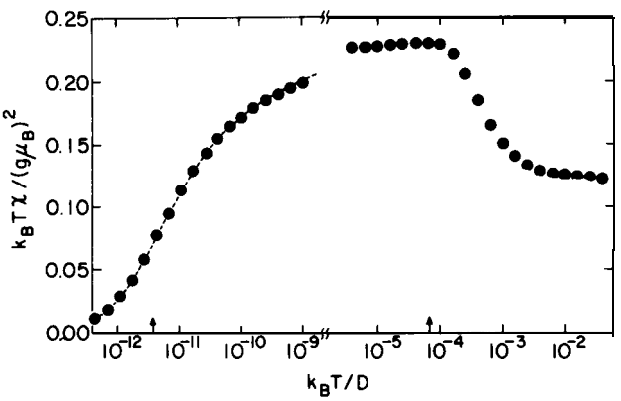


Fig. 8. Susceptibility of the "Anderson Model" showing equivalence to Kondo. (from Krishna-Murthy et al (29))

we give as the last figure of this subject Krishna-Murthys' corresponding calculation (29) for the Anderson model (Fig. 8) and one equation: Haldane's precise equivalencing of the parameters of the two models, from his thesis (20) :

$$T_K = \frac{1}{2\pi}\left(\frac{2\Delta U}{\pi}\right)^{1/2} \exp\left[\frac{E_d(E_d+U)}{2\Delta U}\right] \tag{7}$$

which may be used to find the properties of one model from the other: e.g.,

$$X(t\rightarrow 0) = \cdot\frac{103}{T_K} \text{ etc.}$$

I am indebted to a London Times article about Idi Amin for learning that in Swahili "Kondoism" means "robbery with violence." This is not a bad description of this mathematical wilderness of models; H. Suhl has been heard to say that no Hamiltonian so incredibly simple has ever previously done such violence to the literature and to national science budgets.

II. The Origins of Localization Theory

In early 1956, a new theoretical department was organized at the Bell Laboratories, primarily by P. .A. Wolff, C. Herring and myself. Our charter was unusual in an industrial laboratory at the time: we were to operate in an academic mode, with postdoctoral fellows, informal and democratic leadership, and with an active visitor program, and that first summer we were fortunate in having a large group of visitors of whom two of those germane to this story were David Pines and Elihu Abrahams."

The three of us took as our subject magnetic relaxation effects in the beautiful series of paramagnetic resonance experiments on donors in Si begun by Bob Fletcher and then being carried on by George Feher. Feher was studying (primarily) paramagnetic resonance at liquid He temperatures of the system of donor impurities (e.g., P, As, etc.) in very pure Si, in the concentration range 10^{15}-10^{18} impurities/cc encompassing the point of "impurity band" formation around 6×10^{17}. At such temperatures most of the donors were neutral (except those emptied by compensating "acceptor" impurities such as B, Al or Ga), having four valences occupied by bonds, leaving a hydrogenic orbital for the last electron which, because of dielectric screening and effective mass, has an effective Bohr radius of order 20 Å (Fig. 9). The free spin of

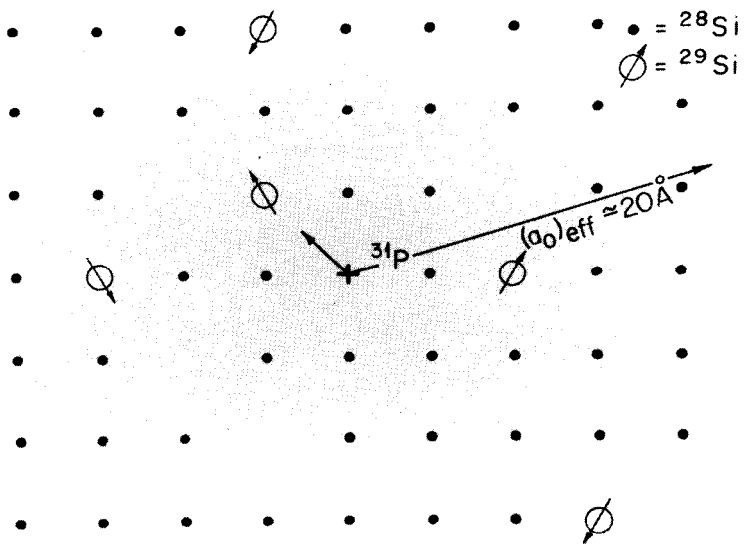


Fig. 9. Donor wave functions in Si and Si_{29} nuclei: schematic.

* It may be of interest to note that theorists permanently or temporarily employed at Bell Labs that summer were at least the following: a) (permanent or semipermanent) P. W. Anderson, C. Herring, M. Lax, H. W. Lewis, G. H. Wannier, P. A. Wolff, J. C. Phillips; b) (temporary) E. Abrahams, K. Huang, J. M. Luttinger, W. Kohn, D. Pines, J. R. Schrieffer, P. Nozieres; c) (permanent but not in theory group) : L. R. Walker. H. Suhl, W. Shockley.

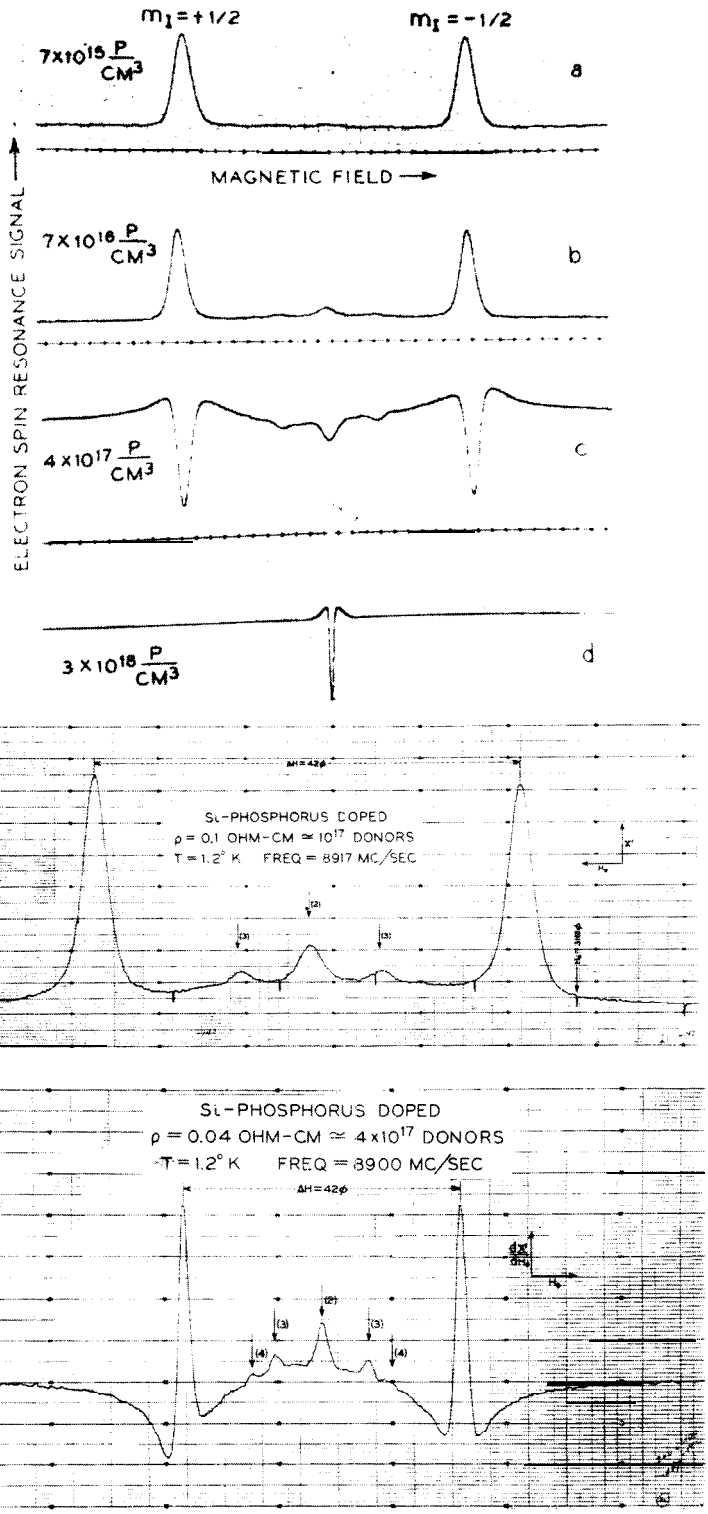
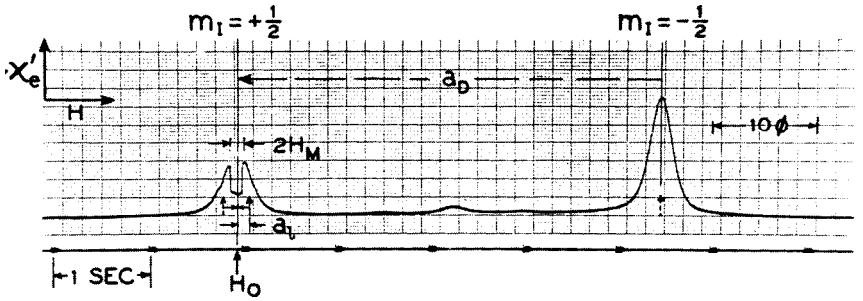


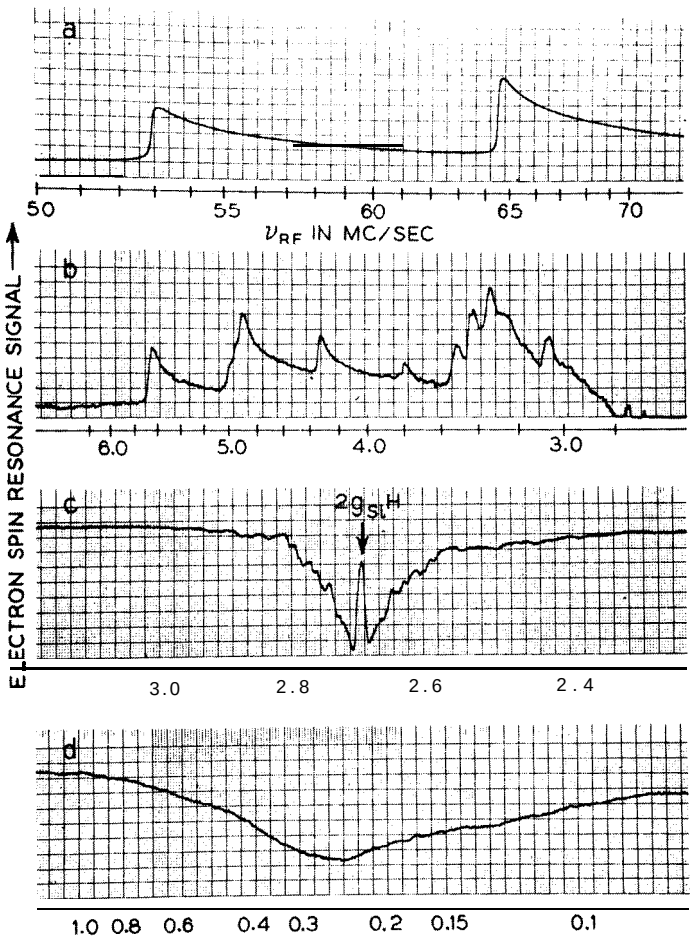
Fig. 10. a) Hyperfine structures of donor EPR at increasing donor (P) concentrations the Mott-Anderson metal-insulator transition. b) Example of well-developed cluster lines.

this extra donor orbital has a hyperfine interaction with the donor nucleus (^{31}P or As, for instance) leading to the clean hfs (30) shown in Fig. 10. In addition, isotopic substitution proved that most of the residual breadth of the lines is also caused by hfs interactions, of the very extended electronic orbital with the random atmosphere of ~ 5 % of Si_{29} nuclei in natural Si, and for reasonably low donor densities of ~ 10^{16}/cc the actual spin-spin and spin-lattice relaxation times were many seconds. That is, the lines were "inhomogeneously broadened", so that many very detailed experimental techniques were available. Feher and Fletcher (31) had already probed what we would now call the Mott-Anderson transition in these materials (Fig. 10a) . As the concentration was raised, first lines with fractional hfs appeared, signifying clusters of 2, 3, 4, or more spins in which the exchange integrals between donors overweighed the hf splitting and the electron spins saw fractionally each of the donor nuclei in the cluster. (A good example is shown in Fig. 10b.) Finally, at ~ 6 x 10^{17}, came a sudden transition to a homogeneously broadened free-electron line: the electrons went into an "impurity band" at that point. Pines, Bardeen and Slichter (33) had developed a theory of spin-lattice relaxation for donors, and it was our naive expectation that we would soon learn how to apply this to Feher's results. In fact, no theoretical discussion of the relaxation phenomena observed by Feher was ever forthcoming, only a description of the experiments (34). What the three of us soon realized was that we were confronted with a most complex situation little of which we understood. In particular, we could not understand at all the mere fact of the extremely sharp and well-defined "spin-packets" evinced by such experiments as "hole digging" and later the beautiful "ENDOR" effect (32, 34). (In the ENDOR experiment Feher would select a spin packet by saturating the line at a specific frequency ("digging a hole", Fig. 1 la) and monitor the nmr frequencies of ^{29}Si nuclei in contact with packet spins by exciting with the appropriate radio frequency and watching the desaturation of the packet (Fig. 1 lb). In this figure, the many seconds recovery time after passing the ENDOR line is actually an underestimate of the packet T_2 because the system is driven.) Thus every individual *P* electron had its own frequency and kept it for seconds or minutes at a time.

We assumed from the start the basic ideas of Mott with regard to actual electron motion: that since there were few compensating acceptors, Coulomb repulsion kept most of the donors singly-occupied leaving us with the paramagnetic spin system we observed. W. Kohn seems to have suggested that even the empty donors would be pinned down by staying close to their compensating negatively charged acceptors because of Coulomb attraction (see Fig. 12). Thus there was little actual electron motion, and we noticed only some speeding up of the relaxation times as we approached what now would be called the "Mott-Anderson" transition. Stretching our gullibility a bit, we could believe that nothing spectacular was *necessarily* required to prevent mobility of the actual charged electron excitations. (It was, however, at this time that I suggested to Geballe the study of dielectric relaxation in these materials to probe this motion, which led to the discovery of the now well-



DEMONSTRATION OF THE INHOMOGENEOUS BROADENING IN P-DOPED SILICON ($\rho = 0.25\ \Omega$ CM, T = 1.2° K, $H_O = 3000\ \phi$) IN THE ENDOR TECHNIQUE SPIN PACKETS ARE FLIPPED AS INDICATED BY ARROWS



DOUBLE RESONANCE SPECTRUM IN P-DOPED Si.
$\approx 2 \times 10^{16}$ P/CM^3, T=1.3° K, $\nu_e \approx 9000$ MC/SEC

Fig. 11. a) "Hole-digging" and ENDOR spectrum. While saturating a specific frequency (11a) an rf signal of variable frequency is applied (11b). Note slow refilling of hole (exponential recoveries) in Fig. 11b: sweep time is several minutes. This is the Si^{29} spin-lattice relaxation, enhanced by the rf power applied.

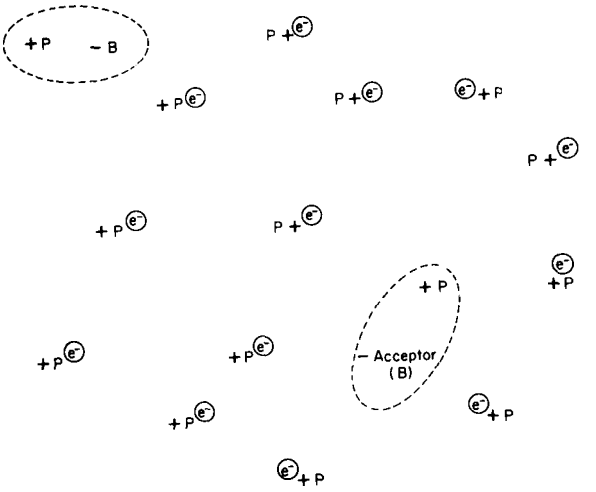


Fig. 12. Hypothetical binding of charged donors to acceptors.

known Pollak-Geballe "$\omega^{0,8}$" conductivity (34). I felt that the absence of conduction in the impurity band was also a serious question, in this as in many other systems.)

No arguments using Coulomb interaction saved us from a second dilemma: the absence of spin diffusion. Bloembergen, in 1949 (36), had proposed the idea of spin diffusion in nuclear spin systems, which has since had much experimental verification. His idea was that the dipolar interactions caused mutual precessions which, in the high temperature paramagnetic state of a spin system, could by diffusion equilibrate the spin temperature in space, thereby giving a means-for example-for nuclear spins to relax by diffusing to the neighborhood of an electronic spin impurity. To calculate the process he used a simple estimate from the Golden Rule plus random walk theory.

Portis (37), in 1953, introduced the idea of random "inhomogeneous broadening" where complete equilibration within a spectral line is impeded, and instead one speaks of "spin packets" of spins having a definite resonance frequency within the line (Fig. 13). (Such packets are spatially random, of

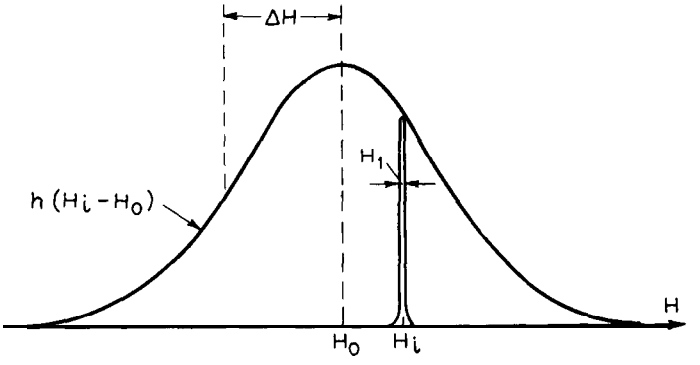


Fig. 13. "Spin packets" and spectral diffusion.

course; in macroscopically inhomogeneous systems the same phenomena had been seen much earlier.) Portis estimated that if the interaction of neighboring spins was $\mathcal{J}_{ij}$, the lifetime of a spin packet (38) should be of order

$$\hbar/\tau \simeq \pi|\mathcal{J}_{ij}|^2 \times \frac{z}{W} \tag{8}$$

W being the width of the line and z the number of neighbors: this is apparently obvious by the Golden Rule. But when Elihu Abrahams estimated $\mathcal{J}_{ij}$ for our system, he found that according to (8), τ should have varied from .1 to 10^{-6}s, whereas Feher's spin packets stayed saturated for 10-100 s in a typical ENDOR experiment. His estimates were checked by the "cluster" phenomenon of Fig. 10.

I find in my notes a reference on 6/20/56 to a discussion with Pines where I suggested an "All or Nothing" theorem to explain this. Later, on 10/31/56, comes an optimistically claimed "proof" of "Anderson's Theorem", much like an unsophisticated version of my final paper which even so is hardly a "proof" ; such. does not yet really exist. I also seem to have spoken to an uninterested audience at the Seattle International Theoretical Physics Symposium. But the actual work was not completed until shortly before I talked about it to much the same group of residents and visitors on July 10th. and 17th, 1957. By that time, I had clearly been a nuisance to everyone with "my" theorem: Peter Wolff had given me a short course in perturbation theory, Conyers Herring had found useful preprints from Broadbent and Hammersley on the new subject of percolation theory, Larry Walker had made a suggestion and Gregory Wannier posed a vital question, etc. But my recollection is that, on the whole, the attitude was one of humoring me.

Let me now give you the basics of the argument I then presented (39) but in much more modern terminology (the mathematics is the same, essentially). I don't think this is the only or final way to do it; a discussion which is more useful in many ways, for instance, can be based on Mott's idea of minimum metallic conductivity as used by Thouless and co-workers and as he will touch upon; but I think this way brings out the essential nature of this surprising nonergodic behavior most clearly. I apologize for this brief excursion into mathematics, but please be assured that I include the least amount possible.

The first problem was to create a model which contained only essentials. This was simple enough: a linearized, random "tight-binding" model of non-interacting particles :

$$H = \sum_i E_i n_i + \sum_{ij} V_{ij}\, c_i^{+} c_j \tag{9}$$

in which the "hopping" integrals V_{ij} were taken to be nonrandom functions of r_{ij} (the sites i can sit on a lattice if we like) but E_i was chosen from a random probability distribution of width W (Fig. 14). The objects c_i could be harmonic oscillator (phonon) coodinates, electron operators, or spinors for which $V_{ij} \simeq \mathcal{J}_{ij}$ and we neglect the $\mathcal{J}_{ij} S_i^z S_i^z$ interactions of the spin flips. The essential thing is that (9) leads to the linear equation of motion

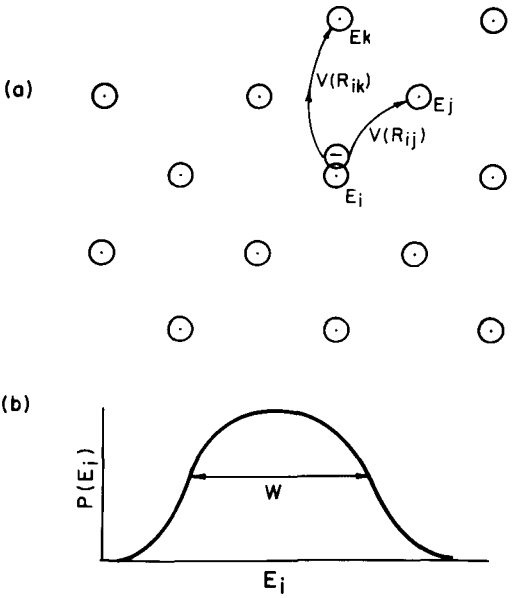


Fig. 14. Model for diffusion in a random lattice
a) sites and hopping integrals
b) probability distribution of E_i

$$ihc_i = E_i c_i + \sum V_{ij} c_j \qquad (10)$$

If W is zero and all E_i the same (say 0), (10) describes a band of Bloch states of width about $\sum \bar{V}_{ji}$. For $W \ll \bar{V}_{ij} = V$, the theories of transport recently developed by Van Hove and Luttinger (40) clearly would describe resistive impurity scattering of free waves (say, electrons, for simplicity). If, on the other hand, $W \gg V$, that would describe our system of local hf fields large compared to $\mathcal{J}_{ij}$; or of random Coulomb and strain energies large compared to the hopping integrals for the electrons from donor to donor.

What is clearly called for is to use W as a perturbation in the one case, and V_{ij} in the other; but what is not so obvious is that the behavior of perturbation theory is absolutely different in the two cases. For definiteness, let us talk in terms of the "resolvent" or "Greenian" operator which describes all the exact wavefunctions φ_a and their energies E_a:

$$G = \frac{1}{\mathrm{E}-\mathrm{H}}; \text{ i.e. } G(r, r') = \sum_a \varphi_a(r) \frac{1}{E - E_a} \varphi_a(r') \qquad (11)$$

where the φ_a and E_a are the exact eigenfunctions of the Hamiltonian (9). In the conventional, "transport" case, we start our perturbation theory with plane-wave-like states

$$\varphi_k^0 = \frac{1}{\sqrt{N}} \sum_i e^{ik \cdot R_i} \varphi_i$$

with energy

$$E_k = \sum_{(i-j)} V_{ij} \cos k \cdot (R_i - R_j)$$

which we assume are only weakly perturbed by the scattering caused by randomly fluctuating E_i's. The E_k's are a continuum in the limit of a large system and we take advantage of this to rearrange perturbation theory and get

$$G_{kk} = \frac{1}{E - E_k - \sum(k, E)} \tag{12}$$

where $\sum$, the "self-energy", is itself a perturbation series (Fig. 15a)

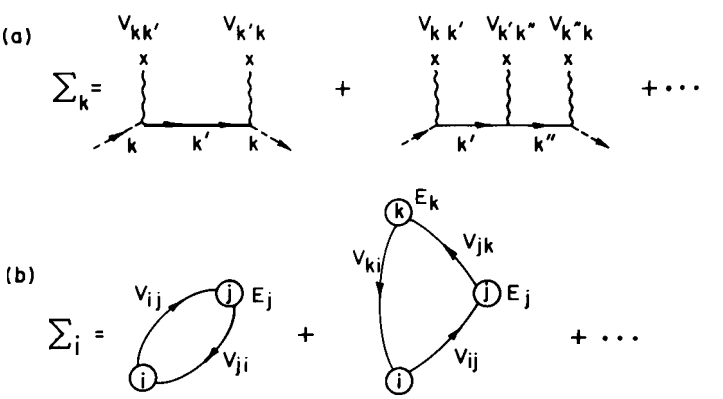


Fig. 15. a) Self-energy diagrams in conventional "propagator" theory.
b) Self-energy diagrams in "locator" theory.

$$\sum = \sum_{k' \neq k} (V_{kk'})^2 \frac{1}{E - E_{k'}} + \sum_{k'', k' \neq k} \frac{V_{kk''} V_{k''k'} V_{k'k}}{(E - E_{k'})(E - E_{k''})} + \ldots \tag{13}$$

which, since E_k is a continuum, has a finite imaginary part as E approaches the real axis

$$\lim_{\mathrm{Im}E \to \pm 0} \mathrm{Im} \sum = \pm \pi \int |V_{kk'}|^2 \delta(E_{k'} - E) + \ldots \tag{14}$$

Note that $V_{kk'}$ in this case comes from the width "W" not V_{ij}.)

This equation means that E_k has a finite width. in energy, and ImG, the density of states, is a finite, continuous function of E (Fig. 16).

$$\lim_{s \to \pm 0} G_k(E + is) = \frac{1}{E - E_k - R_e \sum \mp i\Delta(E)} \tag{15}$$

G has a genuine *cut* on the real axis, and there is a continuum of energy states at every site, of every energy in the band: the states are what we now call "extended". That is, the definition (11) of G basically tells us

$$\mathrm{Im}G(i, i; E) = \pi \sum_a |\varphi_a(r_i)|^2 \delta(E - E_a). \tag{16}$$

Transforming (15) to find G_{ii}, we find that the $|\varphi_a(r_i)|^2$ are each infinitesimal

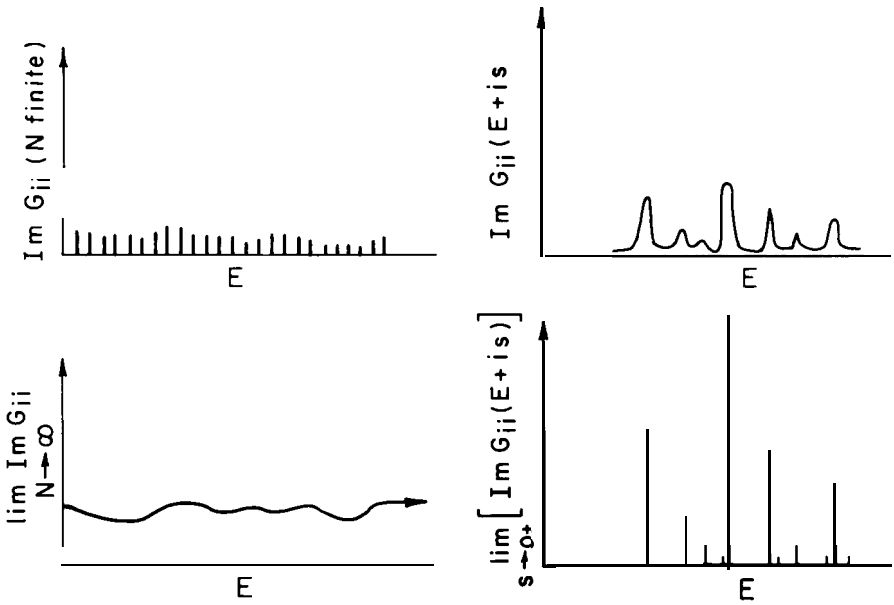


Fig. 16. a) Im G_{ii} in extended case
b) Im G_{ii} in localized case

of order $\frac{1}{\sqrt{N}}$, forming in the limit $N \to 0$ a true continuum of states of every energy at site i. Of course, there are sum rules stating that every state is somewhere and that no states get lost:

$$\sum_i |q_a(i)|^2 = \sum_a |q_a(i)|^2 = 1 \tag{17}$$

and these are satisfied by $q_a(i) \sim (\sqrt{N})^{-1}$, where N is the total number both of *a's* and *i's*.

My contribution was just to show that this is not the only possible case, other than just an empty band of energies, or a set of discrete states as one may have near a single attractive potential like a hydrogen atom. What I showed is that one may have a continuum in energy but not in *space*. This is immediately made plausible just by doing perturbation theory in the opposite order.

In this case one takes E_i as the big term, and the starting eigenfunctions and eigen-energies are just

$$\varphi_i^0 = \varphi_i,\ E_i^0 = E_i \tag{18}$$

and V_{ij} is the perturbation. In this case, (which Larry Walker suggested I call "cisport") we use a "locator" instead of a "propagator" series, for the "locator" G_{ii} not the "propagator" G_{kk}:

$$G_{ii}(E+is) = \frac{1}{E+is-E_i-\sum_i(E+is)} \tag{19}$$

where now the self-energy $\sum$ is a superficially similar series to (13) (Fig. 15b)

$$\sum_i = \sum_{j \neq i} \frac{(V_{ij})^2}{E - E_j} + \sum \frac{V_{ij}\, V_{jk}\, V_{ki}}{(E-E_j)(E-E_k)} + \cdots \tag{20}$$

If at this point we make one tiny mistake, we immediately arrive back at Portis' answer (8) : namely if we average in any way, we get

$$\mathrm{Ave}\left\{ \lim_{s \to 0+} \left(\mathrm{Im} \sum_{j,\, k \neq i} (E + is) \right) \right\} \simeq \frac{\langle V_{ij}^2 \rangle}{W} \tag{21}$$

But there is a very important fundamental truth about random systems we must always keep in mind *: no real atom is an average atom, nor is an experiment ever done on an ensemble of* samples. What we really need to know is the *probability distribution* of $\mathrm{Im}\sum$, *not* its average, because it's only each specific instance we are interested in. I would like to emphasize that this is the important, and deeply new, step taken here: the willingness to deal with *distributions,* not averages. Most of the recent progress in the fundamental physics of amorphous materials involves this same kind of step, which implies that a random system is to be treated not as just a dirty regular one, but in a fundamentally different way.

Having taken this point of view, it is sufficient to study only the first term of (20), it turns out. Let us first pick a finite s, and then take the limit as s → o. With a finite s,

$$\mathrm{Im}\left(\frac{\sum}{s} \right) = \sum_j \frac{|V_{ij}|^2}{(E-E_j)^2 + s^2}$$

The condition that E_j appear as a peak of Im $\frac{\sum}{s}$ is that E_j be within s of E, and that $V_{ij} >$ s. To assess the probability that V_{ij} is large enough, use the physically realistic assumption of exponential wavefunctions:

$$V(R) = V_0\, \mathrm{e}^{-R/R_0}.$$

In the energy interval of size s, there will be ns/W energies E_j per unit volume ($\mathcal{N}$ is the site density per unit volume), while $V >$ s implies

$$V > s : R < R_0 \ln \frac{s}{V_0}$$

and the probability that both V > s and $E\text{-}E_j < s$ is

$$P(V > s,\, |E - E_j| < s) = \mathcal{N} \cdot \frac{4\pi R_0^3}{3} \left(\frac{s}{W} \right) \left(\ln \frac{s}{V_0} \right)^3$$

$$P(s \to \mathrm{o}) = 0$$

It is easy to formalize this: one may show that the probability distribution of $\mathrm{Im}\sum$ is essentially

$$\lim_{s \to 0} P\left(\mathrm{Im}\left(\frac{\sum}{s} \right) = X \right) \mathrm{d}X = \frac{\mathrm{d}X}{X^{3/2}}\, \mathrm{e}^{-\frac{s}{X}} \tag{22}$$

which indeed has a divergent average as it should, but is finite nonetheless, so that $\mathrm{Im}\sum \propto s$ and there is not a finite cut at the real axis.

When we stop and think about what this means, it turns out to be very simple. It is just that we satisfy the sum rules (17) not by each $\varphi_a(i)$ being infinitesimal, but by a discrete series of finite values: the biggest φ_a^i is of order 1, the next of order l/2, etc., etc., (see Fig. 16b). Thus, $\mathrm{Im}G_{ii}$ is a sum of a discrete infinite series of &functions with convergent coefficients. This is the *localized* case.

That is more than enough mathematics, and is all that we will need. The rest boils down simply to the question of when this lowest-order treatment is justified, and how it breaks down.

The bulk of the original paper was concerned with how to deal with the higher terms of the series and show that they don't change things qualitatively : what they do, actually, is just to *renormalize* V_{ij} and the *Ej's* so that even if V_{ij} is short-range initially, it becomes effectively exponential; and, of course, the $V_{ij's}$ broaden the spectrum. If this is the case, one then realizes that the extended case can only occur because of a breakdown of perturbation theory. This comes about as the higher terms of perturbation theory "renormalize" $V(R_j)$ and stretch it out to longer and longer range, so that the exponentially localized function become less so and finally one reaches a "mobility edge" or "Anderson transition".

Here we begin to tie in to some of the ideas which Professor Mott will describe. First, it is evident that the self-energy series is a function of E-i.e., of where we are on the real energy axis-so it will cease to converge first at one particular energy *E,* the "mobility edge." For a given model, it is reasonable-in fact usual-to have the localized case for some energies, the extended one for others, separated by a "mobility edge". The significance of this fact was realized by Mott.

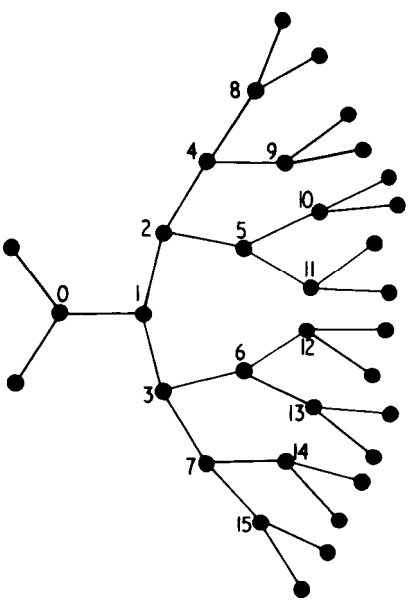


Fig. 17. "Cayley Tree" on which localization theory is exact.

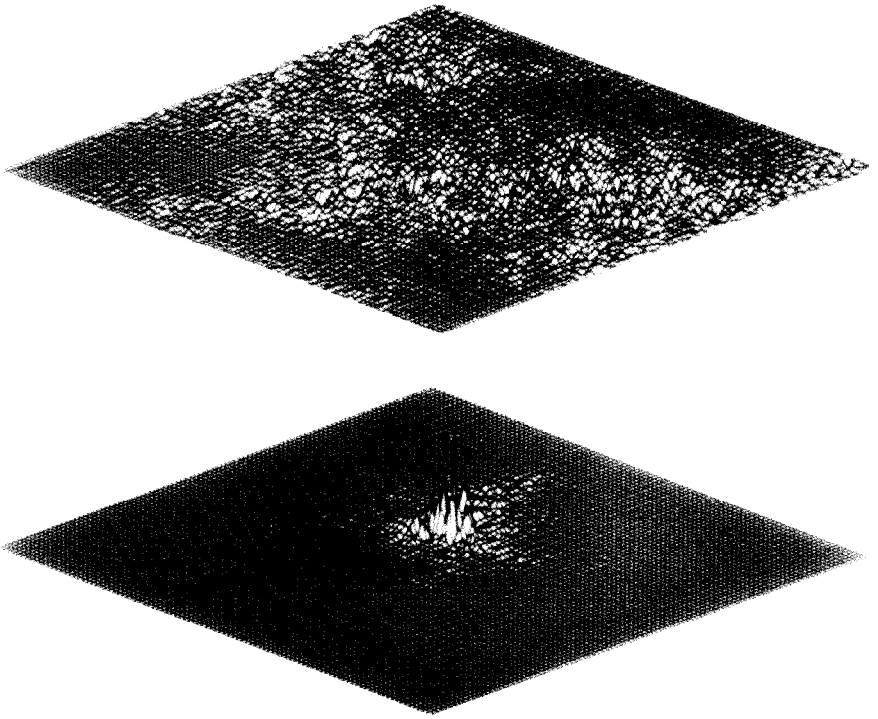


Fig. 18. Computer demonstration of localization (courtesy of Yoshino and Okazaki).
a) W/V = 5.5
b) W/V = 8.0

The actual calculation of this divergence or "Anderson transition" was carried out by me using conservative approximations in the original paper, but it was only much later realized (41) that that calculation was exact on a "Cayley tree" or Bethe Lattice (Fig. 17). Much earlier, Borland (42) and Mott and Twose (43) had shown that localization always occurs in one dimension (also a Cayley tree case, with K = 1). Since it is easy to convince oneself that the Cayley tree is a lattice of infinite dimensionality *d* (though finite neighbor number) it is likely that delocalization first occurs at some lower critical dimensionality d_c, which we now suspect to be 2, from Thouless' scaling theory (44). This dimensionality argument (or equivalent ones of Thouless) first put to rest my earliest worry that my diagram approximations were inexact : in fact, they underestimate localization, rather than otherwise. A second reason why I felt discouraged in the early days was that I couldn't fathom how to reinsert interactions, and was afraid they, too, would delocalize. The realization that, of course, the Mott insulator localizes without randomness, because of interactions, was my liberation on this: one can see easily that the Mott and Anderson effects supplement, not destroy, each other, as I noted in some remarks on the "Fermi Glass" (45) which more or less marked my re-entry into this problem. The present excitement of the field

for me is that I feel a theory of localization with interactions is beginning to appear, in work within my group as well as what Professor Mott will describe. It is remarkable that in almost all cases interactions play a vital role, yet many results are not changed too seriously by them.

I will close, then, and leave the story to be completed by Professor Mott. I would like, however, to add two things: first, a set of figures of a beautiful computer simulation by Yoshino and Okazaki (46), which should convince the most skeptical that localization does occur. The change in W between these two figures is a factor 1.5, which changed the amplitudes of a typical wave function as you see, from extended to extraordinarily well localized. (see Fig. 18).

Finally, you will have noted that we have gone to extraordinary lengths just to make our magnetic moments-in the one case---or our electrons-in the other-stay in one place. This is a situation which was foreshadowed in the works of an eminent 19th century mathematician named Dodson, as shown in the last figure (Fig. 19). “Now here, you see, it takes all the running you can do, to keep in the same place.”

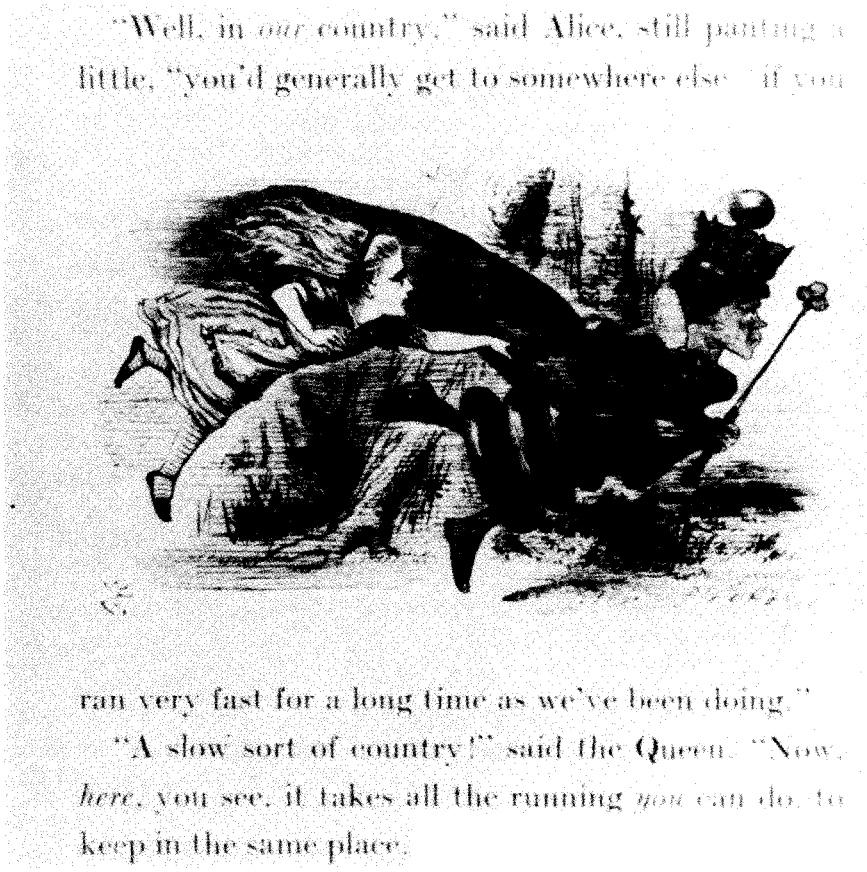


Fig. 19. Efforts to avoid localization (Dodson).

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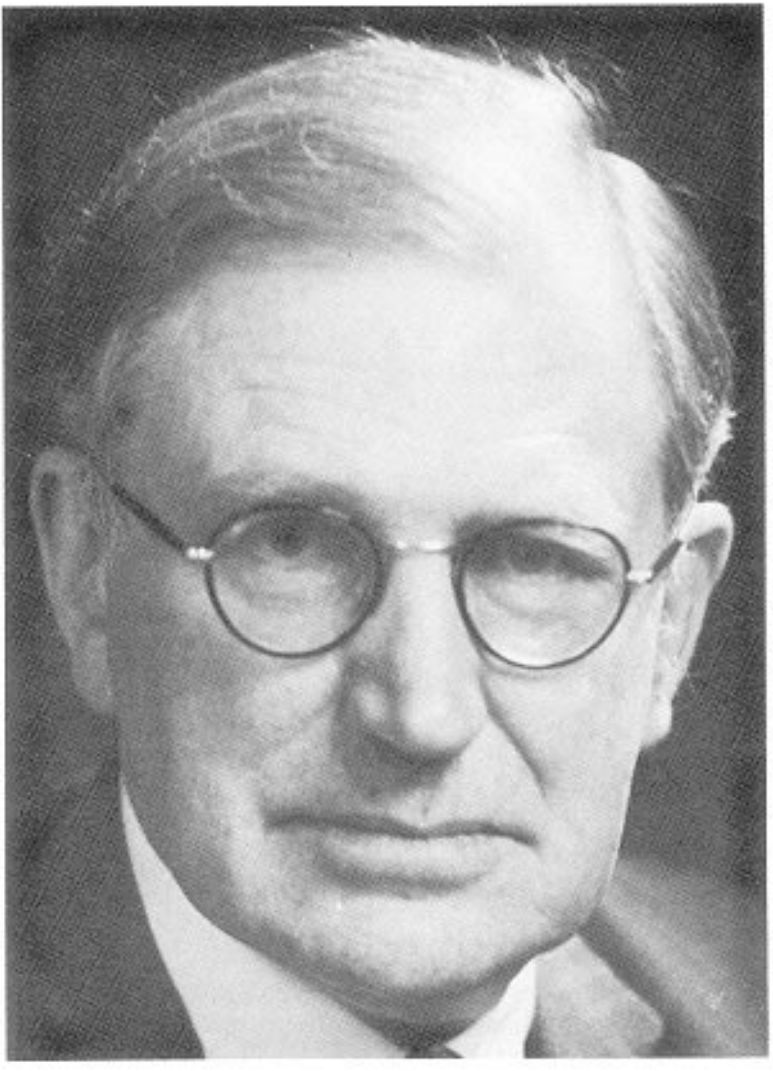

Neville Mott

NEVILL FRANCIS MOTT

Nevill Francis Mott was born in Leeds, U.K., on September 30th, 1905. His parents, Charles Francis Mott and Lilian Mary (née) Reynolds, met when working under J. J. Thomson in the Cavendish Laboratory; his great grandfather was Sir John Richardson, the arctic explorer. He was educated at Clifton College, Bristol and St. John's College, Cambridge, where he studied mathematics and theoretical physics. He started research in Cambridge under R. H. Fowler, in Copenhagen under Niels Bohr and in Göttingen under Max Born, and spent a year as a lecturer at Manchester with W. L. Bragg before accepting a lectureship at Cambridge. Here he worked on collision theory and nuclear problems in Rutherford's laboratory. In 1933 he went to the chair of theoretical physics at Bristol, and under the influence of H. W. Skinner and H. Jones turned to the properties of metals and semiconductors. Work during his Bristol period before the war included a theory of transition metals, of rectification, hardness of alloys (with Nabarro) and of the photographic latent image (with Gurney). After a period of military research in London during the war, he became head of the Bristol physics department, publishing papers on low-temperature oxidation (with Cabrera) and the metal-insulator transition.

In 1954 he was appointed Cavendish Professor of Physics, a post which he held till 1971, serving on numerous government and university committees. The research for which he was awarded the Nobel prize began about 1965. Some of his main books are "The Theory of Atomic Collisions" (with H. S. W. Massey), "Electronic Processes in Ionic Crystals" (with R. W. Gurney) and "Electronic Processes in Non-Crystalline Materials" (with E. A. Davis).

Outside research in physics he has taken a leading part in the reform of science education in the United Kingdom and is still active on committees about educational problems. He was chairman of a Pugwash meeting in Cambridge in 1965. He was chairman of the board and is now president of Taylor & Francis Ltd., scientific publishers since 1798. He was Master of his Cambridge college (Gonville and Caius) from 1959-66. He was President of the International Union of Physics from 1951 to 1957, and holds more than twenty honorary degrees, including Doctor of Technology at Linköping.

In 1930 he married Ruth Eleanor Horder. They have two daughters and three grandchildren, Emma, Edmund and Cecily Crampin.

For the last ten years he has lived in a village, Aspley Guise, next door to his son-in-law and family. During this period he has written an autobiography "A Life in Science" (Taylor and Francis) and edited a book with

several authors on a religion-science interface “Can Scientists Believe?” (James and James, London), together with many scientific papers, mainly in the last 3 years on high-temperature superconductors.

ELECTRONS IN GLASS

Nobel Lecture, 8 December, 1977

by
NEVILL MOTT
Cavendish Laboratory, Cambridge, England

The manufacture of glass, along with the forming of metals, is an art that goes back to prehistoric times. It always seems to me remarkable that our first understanding of the ductility of metals in terms of atomic movements came after the discovery of the neutron. Geoffrey Taylor (1) was the great name here, and Nabarro (2) and I first tried to explain why metallic alloys are hard. The years that passed before anyone tried to get a theoretical understanding of electrons in glass surprises me even more. After all, the striking fact about glass is that it is transparent, and that one does not have to use particularly pure materials to make it so. But, in terms of modern solid state physics, what does "transparent" mean? It means that, in the energy spectrum of the electrons in the material, there is a gap of forbidden energies between the occupied states (the valence band) and the empty states (the conduction band); light quanta corresponding to a visible wave-length do not have the energy needed to make electrons jump across it. This gap is quite a sophisticated concept, entirely dependent on quantum mechanics, and introduced for solids in the 1930's by the pioneering work of Bloch, Peierls and A. H. Wilson. The theory was based on the assumption that the material was crystalline. The gap, in most treatments, was closely related to Bragg reflection of the electron waves by the crystal lattice and the mathematical analysis was based on the assumption of a perfect crystal. Glass, and amorphous materials generally, do not give a sharp Bragg reflection; it is curious, therefore, that no one much earlier than my coworkers and I (3) in Cambridge less than ten years ago seems to have asked the question "how can glass be transparent?".

Actually our curiosity was stimulated by the investigation of the Leningrad school under Kolomiets (4) from 1950 onwards of electrical rather than the optical properties of the glassy semiconductors. These are black glasses, containing arsenic, tellurium and other elements, and for them the band-gap lies in the infra-red. The gap is sufficiently small to ensure that at room temperature an electron can be excited across it. The Leningrad experiments showed, it seems to me, that the concepts of a conduction and a valence band could be applied to glasses, and, more remarkably, that the gap, and hence the conductivity, did not depend sensitively on composition. This is related to the fact that oxide glasses are normally transparent and can only be coloured, as in medieval stained glass, by the addition of transition metal atoms, where an inner shell produces its own absorption spectrum, depending little on the surroundings. These properties of glass are in sharp contrast with the behaviour of crystals, where the whole of silicon technology depends on the fact that if, for instance, phosphorus with its five electrons is added, four form bonds but

the fifth is very loosely bound. The discovery of this property of glasses certainly makes Kolomiets one of the fathers of the branch of science that I am describing, as were others in Eastern European countries, notably Grigorovici in Bucurest and Taut in Prague. The explanation in chemical terms (5) of this property seems to be that in a glass each atom will have the right number of neighbours to enable all electrons to be taken up in bonds. There are important exceptions to this, mainly for deposited films, which I will come to, but in most glasses cooled from the melt it seems to be true.

This being so, what is the nature of the "conduction band" in amorphous materials? Is there necessarily a "tail" of states extending through the gap, as assumed in an early and important paper by Cohen, Fritzsche and Ovshinsky (6)? The fact that most glasses are transparent makes this unlikely. Clues came from another Leningrad idea due to Ioffe and Regel (7), namely that the mean free path cannot be shorter than the electron wavelength, and from the vastly important paper published by Anderson (8) in 1958, "Absence of diffusion in certain random lattices", described in his Nobel lecture this year.

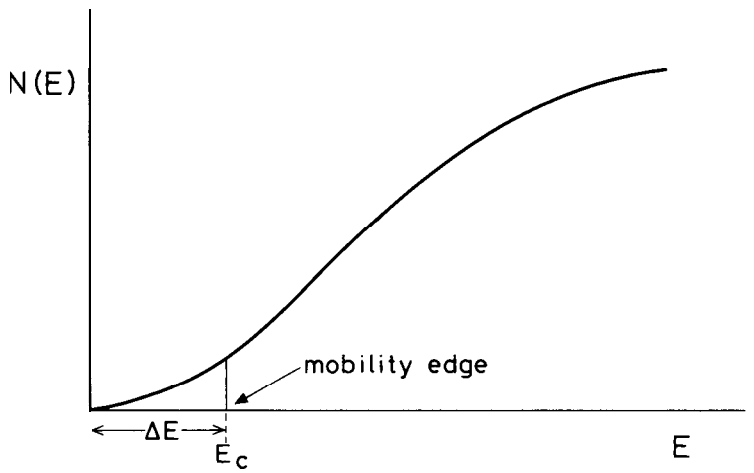

Fig. 1. The density of states in the conduction band of a non-crystalline material, showing the mobility edge E_c separated by an energy $\Delta \mathbf{E}$ from the band edge.

We now understand that in any non-crystalline system the lowest states in the conduction band are "localized", that is to say traps, and that on the energy scale there is a continuous range of such localized states leading from the bottom of the band up to a critical energy (9) E_c, called the mobility edge (6), where states become non-localized or extended. This is illustrated in fig. 1, which shows the density of states. There is an extensive literature calculating the position of the mobility edge with various simple models (10), but it has not yet proved possible to do this for a "continuous random network" such as that postulated for SiO_2, As_2Se_3, amorphous Si or any amorphous material where the co-ordination number remains the same as in the crystal. This problem is going to be quite a challenge for the theoreticians-but up till now we depend on experiments for the answer, particularly those in which

electrons are injected into a non-crystalline material and their drift mobilities measured. What one expects is that at low temperatures charge transport is by "hopping" from one localized state to another, a process involving interaction with phonons and with only a small activation energy, while at high temperatures current is carried by electrons excited to the mobility edge, the mobility behaving as $\mu_0 \exp(-\Delta E/kT)$. With this model the drift mobility, conductivity, and thermopower are illustrated in fig. 2 and (following a theory due to Friedman (11)) the Hall mobility can also be calculated. Owing to the brilliant work of Spear, Le Comber (12) and co-workers it is clear that this is just what happens in at least one material, silicon deposited from SiH_4 in a glow-discharge. As regards other materials, there is good evidence (13) that "holes" in arsenic telluride behave the same way, though there are other

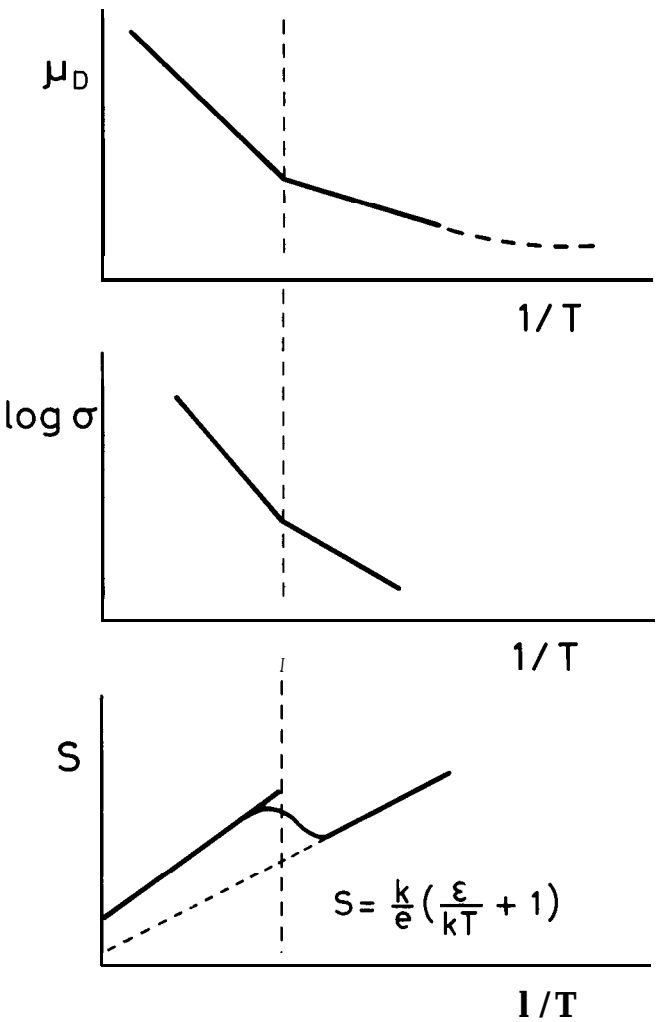

Fig. 2. The diagram shows schematically as functions of the reciprocal temperature the drift mobility μ_D, the conductivity σ and thermopower S of a material where the conduction band is as in fig. 1. ε is equal to E_c-E_F .

interpretations (14). But in other non-crystalline materials, notably for electrons in liquid rare gases (15), vitreous silicon dioxide (16) and some others there is no evidence for a mobility edge at all, the drift mobility *decreasing* with increasing temperature. In some materials, then, the range of localized states (*AE* in fig. 1) must be smaller than *kT* at room temperature. We await theoretical predictions of when this should be so.

For semiconductors, then, the data are rather scanty and we may ask how strong is the evidence for the existence of localized states and for a mobility edge generally for electrons in disordered systems? Apart from glow-discharge deposited silicon, far and away the strongest evidence, in my view, comes from systems of the type which Anderson has called "Fermi glasses". Here one must go back to the model of a metal introduced in the very early days of quantum mechanics by Sommerfeld. Electron states in a crystalline metal are occupied up to a limiting Fermi energy E_F, as in fig. 3. The density of states at the Fermi level, which I denote by $N(E_F)$, determines the electronic specific heat and the Pauli paramagnetism. These statements remain true if the medium is non-crystalline, or if there is a random field of any kind as in an alloy; but in this case states at the bottom of the band, or possibly right through it, are localized. They may be localized at the Fermi energy. If so, we call the system a Fermi glass. Although the specific heat and Pauli magnetism behave as in a metal, the conductivity does not: it tends to zero with decreasing temperature.

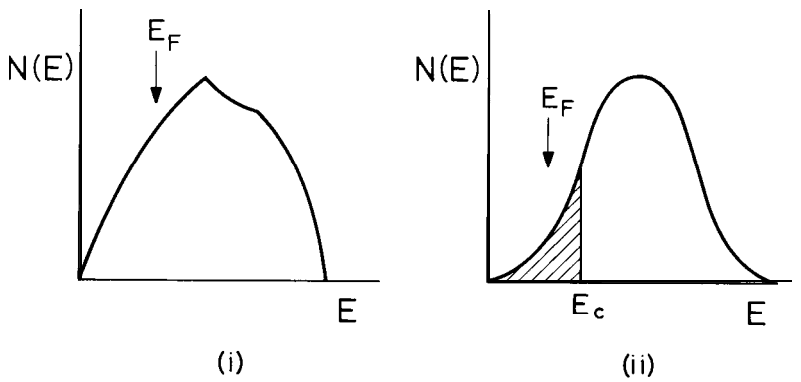

Fig. 3. Density of states in a metallic conduction band, with states occupied up to a limiting Fermi energy E_F. (i) is for a crystal, (ii) for an amorphous or liquid material, with localized states shaded and a mobility edge at E_c.

Let us examine a system in which the density of electrons or degree of disorder can be varied, either by changing the composition or in some other way. Thus if the Fermi energy crosses the mobility edge, a "metal-insulator transition" occurs, of a kind which I have called an Anderson transition (17). I will now examine the electrical behaviour of such a system. If the Fermi energy E_F lies well above any mobility edge, we expect the behaviour familiar in most liquid metals, and the conductivity can be treated by the theory put forward by Ziman (18) in 1961-one of the first successful approaches to conduction in non-crystalline materials, which showed that such problems

were capable of exact treatment and encouraged the rest of us to try our hands. Ziman's theory is a "weak scattering" theory, the mean free path (L) being large compared with the distance between atoms (a). As one increases the strength of the scattering, one reaches the Ioffe-Regel condition (in this case $L \sim a$), and the conductivity is then about

$$\frac{1}{3} e^2/\hbar a \sim 3000\ \Omega^{-1}\,\mathrm{cm}^{-1}$$

if a ~ 3 A. If the disorder gets stronger and stronger, Anderson localization sets in. The conductivity just before it occurs is then

$$\text{const}\ e^2/\hbar a,$$

where the constant depends on the Anderson localization criterion, and is probably in the range 0.1-0.025. I have called this quantity the "minimum metallic conductivity (9, 19) and denoted it by $\sigma_{\min}$. For a a ~ Å it is in the range 250-1000 Ω^{-1} cm^{-1}, though in systems for which a is larger, such as impurity bands, it is smaller. I have maintained for several years that if the conductivity is finite in the limit of low temperatures, it cannot be less than this. This really does seem to be the case, and there is quite strong evidence for it, some of which I will describe. But the proposal proved very controversial (20), and only recently due to the numerical work of Licciardello and Thouless (21), and other analytical work is it carrying conviction among most theorists.

Now let me ask what happens when the Fermi energy lies below the mobility edge, so that states at the Fermi energy are localized, and the material is what I called a "Fermi glass". There are two mechanisms of conduction; at high temperatures electrons are excited to the mobility edge, so that

$$\sigma = \sigma_{\min} \exp\left\{-(E_c - E_F)/kT\right\}, \tag{1}$$

and at low temperatures conduction is by thermally activated hopping from one level to another. In 1969 I was able to show[5] that the latter process should give a conductivity following the law

$$\sigma = A \exp(-B/T^{\frac{1}{4}}) \tag{2}$$

with B depending on the radial extension of the wave functions and the density of states. In two dimensions $T^{\frac{1}{4}}$ becomes $T^{\frac{1}{3}}$. There has been quite a literature on this (22), following my elementary proof, and perhaps the effect of correlation is not yet perfectly understood, but I am convinced (23) that $T^{\frac{1}{4}}$ behaviour is *always* to be expected in the limit of low temperatures.

It follows, then, that for a system in which one can vary the number of electrons, the plot of resistivity against 1/T will be as in fig. 4. If there is a high density of electrons, and E_F lies above E_c the conductivity should be nearly independent of temperature. As the density of electrons is lowered, the Fermi energy falls till it reaches E_c and then $\sigma = \sigma_{\min}$. If the density falls still further, states are localized giving conduction by the two mechanisms of (1) and (2) at high and low temperatures respectively.

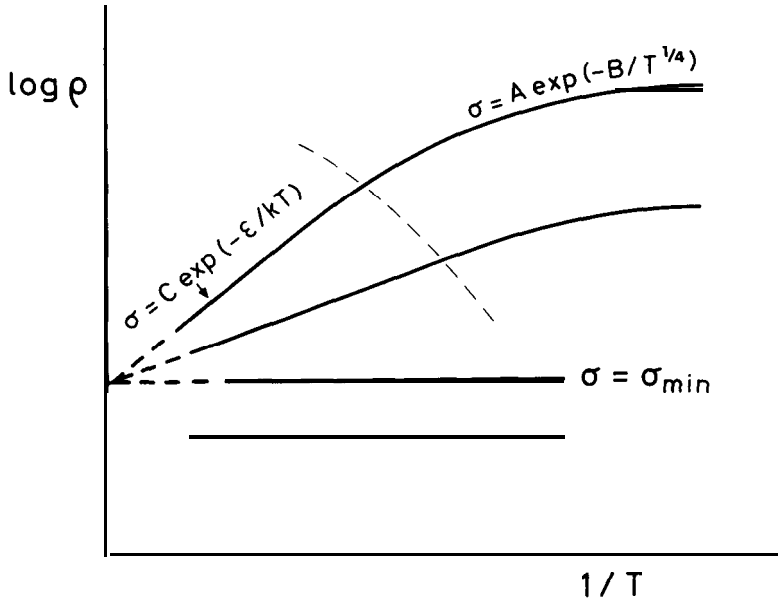

Fig. 4. Plot of log(resistivity) against 1/T for a system in which the density of electrons can be altered so that ε (= E_c - E_F) changes sign, giving a metal-insulator transition of Anderson type.

As regards the systems to which this concept can be applied, there are many. One is the alloy $La_{1-x}Sr_xVO_3$, which I owe to my colleagues (24) in Professor Hagenmuller's laboratory at Bordeaux. In these, a vanadium *d* band contains a number of electrons which varies with x, and thus with composition. But the simplest system is the MOSFET (metal-oxide-silicon-field-effect-transistor) illustrated in fig. 5. In this, *two dimensional* conduction takes place in an inversion layer at the Si-SiO_2 interface, the "band bending" being illustrated

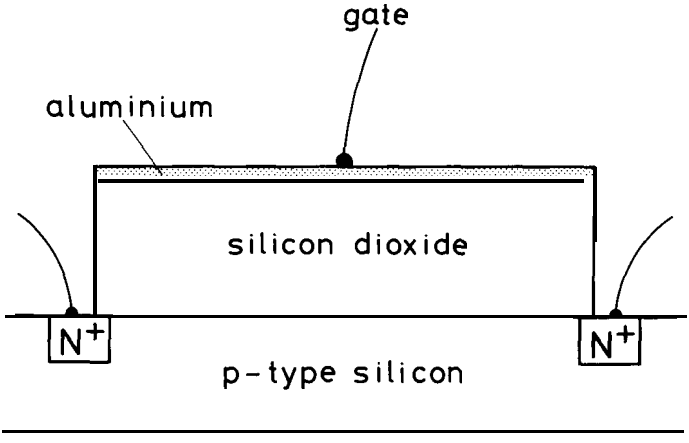

Fig. 5. A MOSFET device, for demonstration of two-dimensional conduction along the interface between the p-type Si and SiO_2.

in fig. 6. The electron gas in the inversion layers is degenerate at helium temperatures, and the beauty of the system is that the density of electrons can be varied simply by changing the gate voltage. Disorder arises because the oxide contains random charges-capable of being controlled by the technology. The investigations of Pepper and co-workers (25, 15) showed behaviour confirming the pattern of fig. 4 in every detail, and reasonable values of σ_{min} (expected to be 0.1 $e^2/\hbar$ in two dimensions).

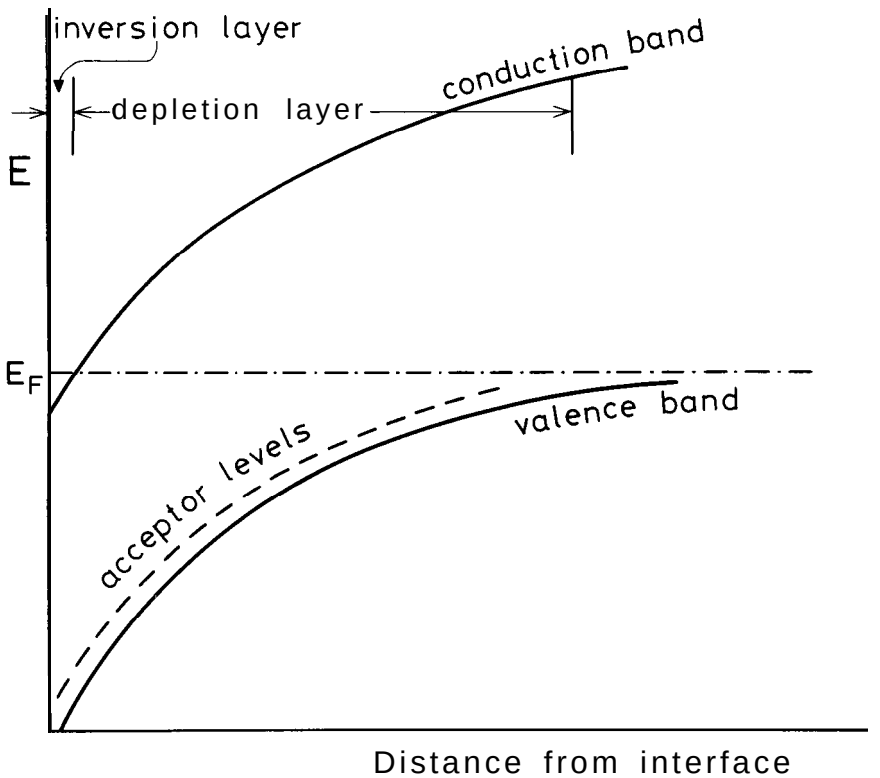

Fig. 6. Application of a field to the surface of a p-type semiconductor inducing an n-type surface layer.

$T^{\frac{1}{4}}$ behaviour occurs also in many amorphous semiconductors, such as Si and Ge, and indeed was first observed in amorphous silicon by Whalley (26) and $T^{\frac{1}{3}}$ in thin films by Knotek, Pollak et al (44). The Marburg group under Professor Stuke (27) has investigated this phenomenon and its relation to electron spin resonance in detail. The idea here is that many amorphous materials contain "deep levels" due to defects such as dangling bonds; a photograph (fig. 7) is included to show what is meant. Some of these may be charged and some not; if so, the density of states at the Fermi level is finite, and electrons hopping from one of these levels to another can occur, giving a conductivity following eqn (2).

Now I would like to finish the scientific part of this lecture by mentioning two new things and two old ones.

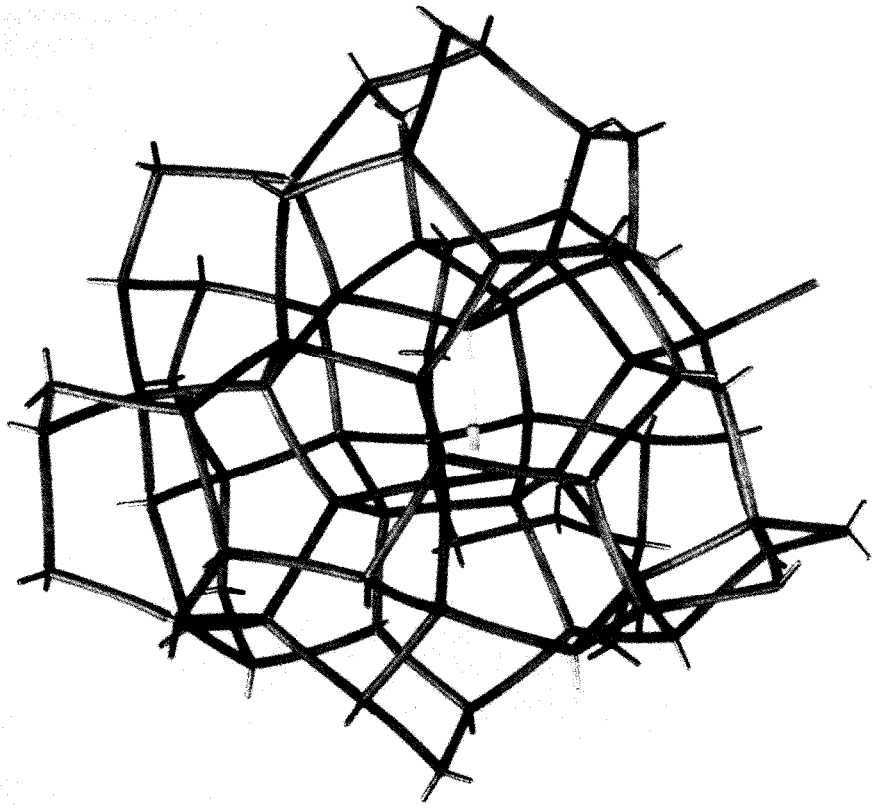

Fig. 7. A "dangling bond" in a continuous random network with fourfold co-ordination (courtesy of Dr E. A. Davis).

One of the new things is the important discovery by Spear and co-workers (28, 29) that one can dope deposited films of silicon, for instance by depositing PH_3 with SiH_4. Much of the phosphorus seems to go in with three nearest neighbours, so that there are no loosely bound electrons, but sufficient take up fourfold co-ordination so as to give donors. These lose their electrons to states in the gap, but the Fermi energy can be shifted very near to the conduction or the valence bands. It is thus possible to make comparatively cheap p-n junctions, with important implications for the economics of solar cells.

The other new thing is the introduction of the "negative Hubbard U" by Anderson (30), and the application of the idea to specific defects by Street and Mott (31), and by Mott, Davis and Street (32), with subsequent development by Kastner, Adler and Fritzsche (33). It is here supposed by the latter authors that there is a real difference in glasses between defects and fluctuations in density, each making their specific contribution to the entropy (34).

We think the model is applicable to materials in which the top of the valence band consists of lone pair orbitals (35), for instance in selenium p orbitals that do not take part in a bond. If so, we believe that "dangling

bonds" as shown in fig. 5 will *either* contain two electrons or none, and thus show no free spin and be positively or negatively charged. The repulsive energy (the "Hubbard U") due to two electrons on one site is compensated because the positive centre can form a strong bond if it moves towards another selenium, which is thus threefold co-ordinated. The positive and negative centres thus formed have been called by Kastner et al "valence alternating pairs". The important point that these authors show is that one can form a pair without breaking a bond, while a neutral centre (dangling bond) costs much more energy to form it. The evidence that there are charged centres in these materials comes mainly from the experimental work of Street, Searle and Austin (36) on photoluminescence. We now think that the model is capable of explaining a great many of the properties of chalcogenide glasses, and perhaps of oxide glasses too. In particular, it shows how the Fermi energy can be pinned without introducing free spins, it seems capable of giving an explanation of dielectric loss and it provides traps which limit the drift mobility. I feel that this work, particularly as formulated by Anderson, is another example of the Kolomiets principle, that glasses cannot be doped; they form complete bonds whenever they can, even if the cost is negative and positive centres.

I said I would end by talking about two old things. One of course is the use of amorphous selenium for office copying by the Xerox company-a multi-billion dollar industry developed, as is so often the case, before anybody had tried to make theories of the processes involved. When the subject became fashionable all over the world, we found of course that the Xerox scientists knew a great deal about it; and their recent contributions, particularly on dispersive transport (37), are of the highest importance.

The other comparatively "old" thing is the threshold switch invented by S. R. Ovshinsky (38). This in its simplest form consists of a deposited film of a chalcogenide glass about one micron thick, with a molybdenum or carbon electrode on each side. Such a system switches into a highly conducting state as the potential across it is increased, switching off again when the current through it drops below a certain value (fig. 8). The claims made for this device generated a considerable amount of controversy, it being suggested that a thermal instability was involved and that similar phenomenon had been observed many years ago. I do not think this is so, and proposed (39) in 1969, soon after the phenomenon was brought to my notice, that the phenomenon is an example of double injection, holes coming in at one electrode and electrons at the other. This is still my opinion. Experimental work, notably by Petersen and Adler (40) and by Henisch (41), make it practically certain that the conducting channel is not hot enough appreciably to affect the conductivity. The work of Petersen and Adler shows that in the on-state the current flows in a channel in which the density of electrons and holes and the current density do not depend on the total current; as the current increases, the channel simply gets wider, and can be much thicker than the thickness of the film. My own belief (42) is that the channel has strong similarities to the electron-hole droplets in crystalline germanium, that even at room temperature

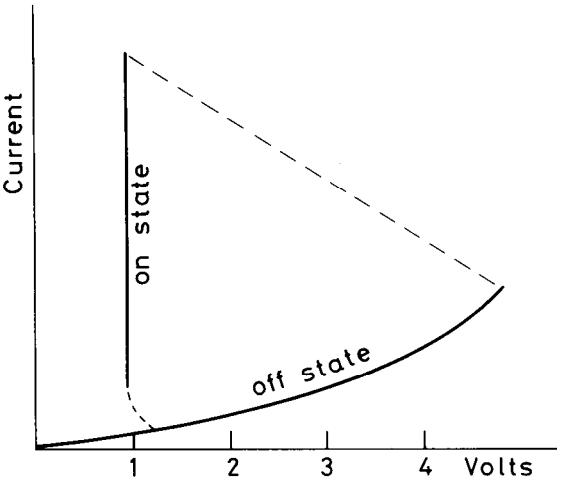

Fig. 8. Current voltage curve of a threshold switch, consisting of a thin chalcogenide film between two electrodes.

one has to do with a *degenerate* plasma of electrons and holes, and that the density of carriers is such that the Fermi energies of both gases lie above the respective mobility edges; only thus can the observed mobilities (~ 1 cm^2/V s) be explained. But we are still far from a full understanding of the behaviour of this fascinating device.

Finally, since I think that mine is the first Nobel prize to be awarded wholly for work on amorphous materials, I would like to say that I hope this will give a certain status to a new, expanding and at times controversial subject. The credit for the prize must certainly be shared with people with whom I've talked and corresponded all over the world. I myself am neither an experimentalist nor a real mathematician; my theory stops at Schrödinger equation. What I've done in this subject is to look at all the evidence, do calculations on the back of an envelope and say to the theoretician, "if *you* apply your techniques to this problem, this is how it will come out" and to the experimentalists just the same thing. This is what I did for $T^{\frac{1}{4}}$ hopping and the minimum metallic conductivity. But without these others on both sides of the fence I would have got nowhere. My thanks are due particularly to my close collaborator Ted Davis, joint author of our book on the subject (43), to Walter Spear and Mike Pepper in the U.K., to Josef Stuke in Marburg, to Karl Berggren in Sweden, to Hiroshi Kamimura in Japan, to Mike Pollak, Hellmut Fritzsche and to many others in the United States and of course to Phil Anderson.

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