FROM RESEARCH IN MAGNETISM TO THE POLITICS OF ENGINEERING: THE EDUCATION OF A PHYSICIST

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Introduction

My trajectory has been quite a bit different from that of most of the previous speakers in this series, because I am going to be describing the life of a physical scientist, one who started as a "pure" physicist and ended up as an engineer. One of the main features of my intellectual trajectory has been discovering the similarities and the differences between scientists and engineers, and why they're often misunderstood.

Early Life

As a small boy I actually started out as an engineer. I built all sorts of machines, wired up batteries and light bulbs to make a signaling system from one floor to another, and I tried to dam a small stream to make a water wheel work – all before I was seven years old.

My parents had very little to do with this. My father had a law degree, but he made his living as a dealer in rare postage stamps, and my mother took me to the park and helped my father in his office. There was no science anywhere. But they humored me.

I was born in Vienna in 1930 and as I was growing up I learned about some of the political turmoil that was going on in Austria from my parents and from the radio. In the spring of 1934 there were socialist riots, and later that year some Nazis assassinated the prime minister, Dollfuss, by driving a truck into the courtyard of the Chancellery and bursting into his office. In March 1938 Hitler's troops marched into Austria and in January of 1939 we left for England. I was very much aware of all of these happenings at the time, but I think I just accepted them.

When we arrived in London I was immediately sent to a local school, and that was quite traumatic because I spoke hardly a word of English. Somehow I survived but the thing that really upset me was that they put me into a very low level class for mathematics, a subject I had always loved. After three months we moved, and I was sent to another school, one run by Jesuits, who were very strict and they got me going again. By the end of the first term there I was fluent in English and surprisingly near the top of the class.

In the summer of 1940 my father was suddenly, and quite unexpectedly, interned as an enemy alien, and when the bombing of London started a few months later, my mother evacuated my sister and me to Newbury, about 50 miles from London. So I started at another school where I stayed for about a year. My father was released after he had been interned for about six months, and when the time came to go back to London, one of the teachers asked my father to come in and told him that he should not send me back to the Jesuit school, because they would not encourage the subjects I liked. He suggested another school, one just up the road, which would be much better for me. My parents were puzzled, but they accepted the advice. So at age 12 I started at King's College School, Wimbledon, and I spent the next six years there.

For those of you not familiar with the English educational system I should perhaps explain how it worked. During my first three years at King's we studied a fairly broad but limited curriculum, with French, Latin and English, Physics, Chemistry, Mathematics and Mechanics, and a choice of History, Greek or German. At 15 we all took an exam in eight of these subjects. I had chosen German, for obvious reasons, so I had no History, and no Geography; in fact we had no Social Sciences, Literature, or Humanities at all.

But then for the next three years we all had to make a very much narrower choice between the sciences, languages and the arts. My father wanted me to become a businessman like himself, and so he thought that I should take languages, but I wanted to take math and science. I appealed to the headmaster and there was a meeting with much discussion – and I won. So for the last three years in high school I studied only math, physics and chemistry. Nothing else. That was, and to some extent still is, the English system, especially for boys.

New College

When I was 17, I sat for a scholarship examination at Oxford, and surprised my school by being awarded one to the college of my choice – New College – which, as some of you may know, was *new* in 1379. The selection process involved a rather unusual feature. After the written exams some of the boys were told to go home, and some of us were asked to stay to take a "practical exam." The practical exam was set up in a large teaching lab, and we were all given a Bunsen burner, some glassware and a stop watch, and we were asked to measure the viscosity of water as a function of temperature.

While I was trying to figure out exactly what to do, a number of men came by and started to ask me a lot of questions. Among other things, they wanted to know about my hobbies, which at that time involved making my own rockets and designing and building model airplanes that would be powered by these rockets. It was an exciting hobby because making rockets involved a lot of trial and error, and, for obvious reasons, I had called my various planes Phoenix I, Phoenix II, etc.

They seemed strangely interested in all of that, but I just wanted them to go away so I could measure the viscosity of water as a function of temperature. Little did I realize at the time that the whole "practical exam" was just a cover for informal interviews. Anyway, New College offered me an Open Scholarship, which is what I had hoped for, and I started at Oxford the following fall.

The reason I had chosen Oxford over Cambridge was that at Oxford I could study only math and physics, whereas at Cambridge I would have studied math, physics and chemistry. I didn't like chemistry, because one has to remember a lot of names of organic chemicals, and my memory for words was poor even then, and that's why I applied only to Oxford. It was actually ironic that in much of my later research we needed a lot of chemistry, because we had to produce our own samples, but fortunately that involved inorganic chemistry, which doesn't use long words, and I was able to learn the necessary chemistry on the job.

Another feature of the system at Oxford at that time was that one had to take only two lots of exams. One at the end of the first year, and one at the end of three years. Nothing in between. In retrospect, the contrast with a place like Yale, where one has strict distributional requirements, and one is graded on every course every term is really striking. One result was that in a place such as Oxford one learned a lot more about one's chosen major, but nothing about anything else, – at least not from the formal lectures.

Graduate Research

As the time for the final exams drew near I had absolutely no idea what I would do next. My tutor in New College, Dr. Cooke, already had plans for me, but he had to wait till he knew I had done well enough in the final exams. He then told me that I would be awarded a research fellowship for three years, and he asked me to join his group.

The title of my dissertation was "The Properties of Some Paramagnetic Salts at Very Low Temperatures." The very low temperatures in this case meant from four degrees down to within a few thousandth of a degree above absolute zero, and to interpret the magnetic properties one had to understand the quantum mechanical behavior of electrons and nuclei, and their interactions. It was a very nice field involving both theory and experiment.

The goal of the research clearly had no immediate practical applications, but the work involved a lot of real hands-on science, making things in the machine shop, and also getting professional machinists and glass blowers to make ever more complicated things to my design. The experiments involved first having to produce the very low temperatures, which in those days was quite an elaborate process, and then getting the specific measurements to work. A typical experiment would take about 21 hours to complete, starting in the morning one day and finishing around 6 a.m. the next day. My advisor was there much of the time, plotting the results on huge sheets of graph paper as we made measurements.

Having Dr. Cooke around for many hours day after day taught me a lot, not only about physics, but also how to deal with the world by keeping one's sense of humor, which one needs more and more as the experiment goes on through the night. He also showed me the importance of keeping a close contact with one's graduate students, and that has been a lesson I remembered throughout my research career.

Interlude

When I finished my DPhil (Oxford didn't go in for PhDs) I was just 24 years old, and that was a problem because it meant that I was no longer deferred from National

Service, for which one was eligible until one was 26. This was nine years after the end of the war, and I felt that my time might be spent more profitably doing something other than joining the army. So I went to discuss the situation with the head of the physics department, Lord Cherwell, who had been Winston Churchill's science advisor during the war. I was glad that he agreed that it would indeed be a waste of time for me to join the army, and he suggested that I do some scientific work of national importance instead. He made some enquiries and then he proposed that I should study the magnetic properties of uranium, and plutonium compounds, which were of interest following the use of these elements in the atomic bomb.

The only problem with these materials is that they are radioactive and extremely toxic, and I couldn't bring them into any of the labs. So Lord Cherwell arranged to have a trailer parked just behind the physics department. We connected the caravan to power and water, and I constructed an apparatus inside the trailer to measure the magnetic properties. The idea was that, if I ever spilled any of the materials, they could just disconnect the trailer from the building and tow it away again. It was never quite clear what would happen to me in such an event, but fortunately that question never arose.

America – First Visit

During this time a very important event in my trajectory took place – I got married to my fellow physics graduate student, Liz. I've learned very many different things from Liz over the past 56 years, but one of the first important things she said early on was that we just had to go to see the United States. A number of people from the lab were coming back with great tales about the lifestyle, scientific opportunities and the great sights to be seen – and we just had to go.

Professor Van Vleck at Harvard was the most famous physicist in magnetism, and so I tried to get a post-doctoral position there. Van was a theorist who didn't have funds for postdocs, but he wanted me to come, so he arranged for me to join an experimental group headed by Professor C. Lester Hogan. Hogan was a very practical man who had recently come to Harvard from Bell Labs, so I would have an opportunity to learn about more applied aspects of magnetism. That sounded interesting.

Of course I was also keen to learn from Van Vleck, but it turned out that he was not an easy person to understand, because he had his own way of thinking about everything, which I suppose is why he later got the Nobel Prize. But under his influence I did start to think along new lines as well, and after a few months I produced a theoretical paper that I find is still being cited more than 50 years later.

Up to that point I had never thought about practical applications of science, and I found the whole atmosphere in the Harvard Division of Engineering and Applied Physics novel and stimulating. In particular, I could see why it was important to understand the microscopic properties of materials that might have practical applications and, even as a lowly post-doc, I soon received offers to consult from industrial labs such as IBM, Dupont and Hughes Aircraft. We certainly liked the money that brought in, but I also liked the idea of working on projects that might lead to practical applications.

We had a great time during that year, enjoying the lifestyle, driving around in our very first car, and visiting beautiful places and a lot of interesting labs. We really enjoyed the tremendous vitality of this country, which was in stark contrast to the still war-weary mood in England at that time.

At the end of my year as a post-doc, Van Vleck asked me to stay as an Assistant Professor and I was very tempted but I had heard bad things about places like Harvard and Yale keeping junior faculty for a number of years, and then not offering them tenure. So we went back to Oxford to try and get tenure there, before doing anything else.

Try Oxford

After a couple of years I did indeed get a tenure appointment in Oxford, and I started to build a research group of my own. Money was always a problem in England, both for equipment and for personal expenses. I thought I might do something about the research funds by getting a contract from the US Air Force, who had supported some of the work at Harvard and, to everyone's surprise, we were offered a contract to do just the kind of work I wanted to do. The people at Oxford weren't used to such contracts, and when the Air Force officer asked what their overhead rate was they didn't know what he meant – but they quickly came up with a figure, and accepted the Air Force money.

One nice perk that came with the Air Force contract was the ability to visit the States using MATS – the Military Air Transport Service – and not only that, they gave me a civilian rank equivalent to that of a colonel, which quite impressed my father-in-law, who had himself been a colonel in the British Army.

On the personal front it wasn't so easy. There were rigid salary scales linked to age and, since I was then only 29, I was near the bottom of the totem pole. So I thought I might be able to supplement my meager university salary with some industrial consulting. But British industry wasn't ready for such a proposal. After a great deal of discussion I was finally offered a consulting contract for just four days a year at £25 per day. We had started a family and we wanted to buy a house, and one hundred pounds a year wasn't going to make much difference, so we started to think about going back to the US again.

Back To America

We started by spending a summer at the General Electric Research Labs in Schenectady, and later six weeks at IBM in Yorktown Heights, and I was also being recruited by Bell Labs, but I really wanted to be in a good university with students and the freedom to follow my own ideas. Also I liked the idea of being free to interact with people in other labs all over the world and to travel widely – a passion I had even then.

So I sent a letter to Henry Fairbank, who was a professor in the physics department here at Yale, and whom I had met in Oxford when he spent a sabbatical year there, and I simply asked if there might perhaps be an opening for me at Yale. The timing couldn't have been better. This was in the spring of 1962 and there had recently been a big reorganization of Engineering at Yale, following a report that recommended that there should be more "Applied Science," and so they were looking for new faculty in just my sort of field. I came and gave a talk, and within a month I had a letter from Kingman Brewster offering a tenured joint appointment between the Physics Department and the new Department of Engineering and Applied Science. Of course I was delighted but I was also a bit nervous, because getting tenure at Oxford had been a big deal and, I knew that if gave that up, I probably wouldn't be able to go back. But we had very much enjoyed all our previous visits to the States, and the Nobel Laureate Willis Lamb had recently left Oxford for Yale, so I accepted and we arrived at here in February 1963.

Yale

At that time I really didn't understand the differences between Physics, Engineering and Applied Science. I knew I wasn't really an engineer, but everyone told me that what I had been doing all along would be just fine. So I set out to rebuild the research facilities I had had at Oxford. I told Kingman Brewster that I wanted to bring a lot of my home-built equipment from Oxford, and that they were happy to sell it for some $12,000 and after a few weeks a big 2-ton container duly arrived. But where to set up my lab? Brewster said it would take "two to five years" to construct a new building, which ultimately became Becton Center on Prospect Street – and actually turned out to take two plus five years – but in the meanwhile I had to find a home.

The only space available was a huge, almost empty hall at the back of Hammond Metallurgical Lab in Mansfield Street, where the School of Art now has its sculpture studios. The only items in that hall at that time were a large 5-inch naval gun and a small anti aircraft gun, – left over from a World War II training facility – much to the joy of our four-year-old son. But there was more than enough space left inside the hall to build a two story cinderblock structure with four large labs, and I set up my two tons of equipment there, aided by two former graduate students from Oxford, who had come with me as post-docs.

The university provided good startup funds, but there was very little in the way of infrastructure or support staff. So, I hired a very skilled machinist, who could make almost anything, a very efficient secretary to handle all the paper work setting up a lab and, together with Bob Wheeler and Dick Barker, we hired a chemist to make samples. Some of you may know Stanley Mroczkowski. I was able to get two sizable grants from Washington almost immediately and so, for seven happy years, I ran a more-or-less self contained research operation in our little cinderblock house at the back of Hammond Lab.

Graduate Students

That time was very exciting for me. I was completely in charge, I had my own support staff, plenty of lab space and good equipment and, most of all, very soon I also had some very good graduate students. The first one, Bob Birgeneau, finished his degree in less than three years and he is now the Chancellor of the University of California at Berkeley. The others have all done pretty well too.

Perhaps this is a good point to talk some more about the role of graduate students in our sort of work, and the relationship that I, and most of my colleagues had with our graduate students. We always worked as a very close team, and we would interact with each student every day. We would plan experiments together, I would try to be there when they were taking data, and we would spend hours discussing the results. And when it came to publishing papers with the results, the students would prepare a draft which I would usually tear apart, they would re-write it several times, and then I would take their best effort, re-write the whole paper and we would publish it jointly. It was a really close interaction, and I am proud to say that I am still in close contact with some three quarters of all my graduate students, and I consider several of them among my closest personal friends.

Science

I should also try to explain now just what it was we were trying to do during this time. The general theme was a search for "ideal magnetic materials." An "ideal" material would be one whose magnetic properties one could describe in terms a detailed theoretical model, and that model should explain not only the observed properties but also predict successfully some other properties that had not yet been studied. It turns out that many magnetic materials really cannot be described in quantitative detail because they are just too complicated, and while one can understand their properties in a general way, any detailed prediction of specific properties is not possible. So why should that matter?

There are at least three different reasons why this sort of detailed understanding is important.

First, there is the purely intellectual joy of finding a system for which "it all works out" – that the experiment agrees with the theory. When scientists talk about "understanding" anything, what they really mean is that they *think* they understand something so well that if they *were* to make some new measurement they could predict with certainty and accuracy what they would find. And finding that just feels good.

But a related reason for building confidence in model predictions is that it can lead to the development of new materials with specific properties of interest for practical applications. Most of you are familiar with an application of one class of magnetic materials – the magnetic hard disks that store information on your computers. We didn't actually work on any of those materials, but the general theoretical understanding of magnetic materials is one of the factors that has made it possible to produce disks that can store more and more information.

But I also had another, very important reason for wanting to study ideal magnetic materials. If one works on a system for which one can make detailed predictions, one has a very good tool for graduate students to learn a critical approach to research. If one has good reason to predict what is going on, one has to be very careful in how one performs new experiments, and how one interprets the results. It either all comes out right, and that's good, – or the experiment has gone wrong somehow, and that's bad – or one finds a really significant discrepancy between the predictions and the results that forces one to revise the model, – and that's interesting. In each case one learns something that can't just be swept under the rug, and I like to think that all my students finished their dissertations having learned to think really critically and honestly.

Anyway, we went on doing this kind of research for seven years in Hammond Lab, and then in 1970 we moved to the brand new Becton Center on Prospect Street and, after a short struggle to get everything working again, enjoyed the carefully designed facilities there.

Administration 1

But only three years later my life started to change. Bob Wheeler had become chairman of Engineering and Applied Science and he asked me to take on the job of Director of Graduate Studies. There was a lot of unrest among the more than 100 students at that time. Since Engineering doesn't teach many large service courses, graduate students in Engineering are generally not supported by teaching appointments, and most of them are usually supported by individual faculty research grants. When money becomes tight, as it did in the early 1970's, and faculty find they can't – or don't want to – support students, that can become a big problem. Anyway, as DGS it was my job to sort out many such problems and I found that I actually quite enjoyed doing that, and after three years things were running very much better.

Undergraduates

But just then there was another major change in my life. One of my senior colleagues, George Schulz, died suddenly of a heart attack, and someone had to teach his undergraduate course in Modern Applied Physics. Believe it or not, I had been at Yale for 13 years, and I had never been asked to teach an undergraduate course. I had had undergraduates doing research projects in my labs, and I had been happily teaching advanced graduate courses and seminars, but now I suddenly had to take over an undergraduate course for juniors. At first that was quite a shock, but I managed to survive and, for the first time, I got an insight into the undergraduate program at Yale. That turned out to be an important preparation for the next phase, which came only a few months later.

Chairman 1

Charles Walker, who had succeeded Bob Wheeler as chairman, suddenly resigned in the summer of 1976, after only two years as chairman. The details of his fight with the administration aren't important here, but Kingman Brewster found himself suddenly needing another chairman, and he called on me. I mentioned before that the graduate students had been in a state of unrest, – well, the faculty were also in a state of unrest and I realized that taking over as chairman would be quite a challenge. I negotiated a number of conditions before agreeing to take on the job, one of which was that there should be an extensive external review of Engineering and Applied Science at Yale, and Kingman agreed.

The main problem was that the faculty in the department of Engineering and Applied Science had widely divergent points of view, and it was almost impossible to get consensus on anything. There were chemical engineers, electrical engineers, and mechanical engineers, and then there were a lot of more recently appointed applied scientists. So any time a proposal for a new appointment came up, or there was a suggestion to revise the undergraduate or the graduate program, three of the four main groups tended, almost automatically, to be against most proposals.

After a lot of negotiation, we finally agreed on a review process for the department, first among ourselves – and that wasn't easy – and then with the Administration. So, after more than four years of discussion and five different review committees, Bart Giamatti, who had succeeded Kingman by then, had a lot of recommendations to consider. He finally decided to split the department into four separate programs, each with its own chairman, much as it had been before the School of Engineering was abolished nearly 20 years earlier. Bart was adamant that he didn't want a School again, and he also didn't want a Dean, but he realized that the four programs needed some coordination, and so he needed someone to manage the whole enterprise. And guess whom he asked to be chairman of the new "Council of Engineering?"

Chairman 2

So I started my 9th year as a fairly busy administrator, and for the next three years we created a new and unique structure for Engineering at Yale, – one that only a few weeks ago (now 27 years later!) finally morphed into what it should have been all along, a School of Engineering and Applied Science with a Dean to run the whole enterprise. But in 1981 we were happy to have four autonomous programs again, and a new era started.

Fading Research

Needless to say my own research suffered a lot during those twelve years of more or less full time administrative work, and when I tried to get back to research I found it difficult to raise funds again. I was invited by the University of Karlsruhe in Germany to come and spend some time with their research group in magnetism, and they nominated me for a Senior Humboldt Award to support my visits.

That was nice because one of the perks of the Humboldt awards in those days was the use of a brand new BMW 528 in which I could drive on the Autobahn at 130 mph and visit the vineyards that are all around Karlsruhe. It was interesting to experience a classical German university system, with its strict hierarchy and large numbers of students and assistants, but I really didn't warm to doing research again. The drive to write "one more paper" had dimmed over the years, and I started to look for other things to do.

Non-Science Undergraduates

It was again Bob Wheeler who got me started on a new track. For some time he had been organizing a course for undergraduates who were not majoring in the sciences, and who were looking for a course to satisfy the Group IV distributional requirement. The course was called "Perspectives on Technology" and it was team taught by a number of faculty members, and he asked me to join. I had previously enjoyed my one experience of teaching undergraduates, – but this was quite a different challenge.

Most of the students who were looking for a course to satisfy the distributional requirement didn't know much about science but, what was worse, many of them really didn't want to know much about science. So it was quite a challenge to get them interested, and even more of a challenge to get them to work. I tried to tell them about such things as crystals, lasers, solar cells, fiber optics, and the global positioning system, all with a lot of live demonstrations, and some of the students really enjoyed seeing how practical things work, but others just couldn't care less. One had to draw satisfaction from those who appreciated what we were trying to show them and not feel hurt by those who were totally unappreciative, if not actually rude. Anyway, I taught parts of that course, in various versions, until I retired.

Chairman 3

But I am getting ahead of myself. In the summer of 1990, Liz and I were in England when an urgent phone call came from Yale: would I take on the chairmanship of the department of Applied Physics? I was totally unprepared for that. I had assumed that my colleague Richard Chang was next in line, but it turned out that he had developed some serious health problems, and when he was asked by Benno Schmidt to become chair he said he really couldn't do it. So I embarked on yet another term as an administrator, but this time of a much more coherent department of Applied Physics.

But it was not to be a smooth ride.

Ever since the creation of the Section of Applied Physics, when the old Engineering and Applied Science department was split in 1981, there were ongoing efforts on the part of the Physics Department to absorb Applied Physics into Physics, and use some of our faculty slots for "real" physics. There were many in the Physics Department at that time who just couldn't see the importance of subfields such as Solid State Physics, Plasma Physics or Laser Physics, and they also couldn't see the importance of such fields to Engineering as a whole.

All of this came to a head during the now infamous "Restructuring" proposals during 1991 and 1992. I was very much involved with the efforts to fight these very ill-conceived proposals – and, as some of you may recall, – we won, and the "Restructuring" effort was totally abandoned!

In retrospect, I like to think that one basic problem then was that some members of the Restructuring Committee, and also some of our colleagues in the Physics Department, really didn't understand what modern engineers and applied scientists do. Finding new applications for lasers is in every way as challenging intellectually as finding a new elementary particle, and both of these fields are important for any modern university. If anything, lasers may be more important for students going out into the real world.

Levin and Bromley

But all of this began to change in 1993 when Rick Levin became President and, even more a year later, when Allan Bromley returned from Washington and took over the coordination of Engineering. He insisted on getting the proper title of Dean of Engineering, in place of Chairman of the Council of Engineering, and he changed the name of the Council to "Faculty of Engineering."

But he also liked to delegate, and so he invented the new job of Director of Educational Affairs for Engineering and, guess whom he asked to take on the position . . . . . ? One of the things he wanted me to do was to take charge of the preparations for the next accreditation visit, which for Engineering is a big deal. It happens every six years and it requires an immense amount of data collection and organization. At the same time, he also asked Rick to reappoint me as chairman of Applied Physics for another term. So I was once again in a full time administrative position. Teaching my part in the course for non-science majors and a course in quantum mechanics for applied physicists and engineers filled the rest of my time, and any thought of research was finally squashed completely.

Director of Educational Affairs

But my five years as Director of Educational Affairs were very interesting because the profession of Engineering was undergoing a major transformation nationally and, indeed, globally. The organization that accredits engineering programs, and is very serious about the process, was about the change the criteria significantly and I got to learn all about their new approach to engineering education.

In a word, they had come to recognize that there are many ways to approach engineering education, and that the old rigid rules about how much of this and that students had to learn were no longer appropriate. What they wanted to do instead was to assess whether each program was doing successfully what it had set out to do. At Yale we had been saying that for more than three decades, so all our programs passed the next accreditation visit with flying colors.

Science and Engineering

In the process, I finally came to understand the real difference between science and engineering. In both science and engineering you follow some ideas to find out what happens when you try to do something new, but in engineering you first have to set a specific goal. In engineering you have to design an approach to get there, whereas in science you tend to let the results dictate the next step. Both can be valid academic activities, but there is a big difference in how these activities can be perceived.

Some people think that science is purely academic, because it probes the laws of Nature, and that engineering is not academic, because it aims towards practical applications. But in many ways engineering is really a more rigorous discipline, because you have to say at the outset what you are trying to achieve, and it is certainly a good preparation for many careers in the real world. Anyway, my involvement with the accreditation process finally completed my conversion from pure science to real engineering education.

Consulting

Actually, from a practical point of view, I had been involved with real engineering from the time I first came to Yale. As I mentioned earlier, I had had contacts with General Electric and IBM while we were still in England, and as soon as I arrived at Yale I converted those contacts into regular consulting arrangements, first with IBM, and then, for the next 27 years, with General Electric. I would go to their labs for one or two days a month, and discuss with different people what they were doing. I found that consulting involves a lot of listening to other people's problems, and one learns a lot by just listening. It was always casual, but it was often quite stimulating, and very different from the kinds of things we were doing in our own labs at Yale.

At GE, for instance, I had very early contact with the development of magnetic resonance imaging, and I also became quite involved with the development of the materials used in stealth technology to make military planes invisible to radar. These projects involved interesting science, but there were certainly very definite goals to be achieved. So, in a way, I've had one foot in engineering for a long time, even though I started my career very much as a physicist.

Retrospect: The Early Research

Throughout my talk I emphasized the more personal aspects of my trajectory. I omitted details of the scientific work that that my students, colleagues, and I published in the years before I was deflected into administration and undergraduate teaching, but now that this is to be published, I would like to add a brief retrospect. During that time we published more than 150 papers, most of which were concerned with a detailed understanding of the quantum mechanical properties of magnetic materials with localized magnetic electrons, primarily rare earth compounds. Crystals of these materials were often made for the first time in our labs.

To explain these magnetic properties there are three principal effects to understand. One is the effect of the *non*-magnetic environment of individual magnetic ions, the so-called crystal field. In a theoretical paper published in 1962 we showed how a wide variety of results could be explained in terms of a single parameter that could be determined experimentally. This paper has been cited more than 1500 times, and it is still being cited now. Crystal field effects also explained the macroscopic anisotropy of ordered magnetic materials, and a very simple explanation of that mechanism, published in 1957, is also still being cited widely.

The second effect to understand is the detailed nature of the interactions between pairs of magnetic ions. These interactions can be studied by a variety of experimental methods, including thermal, magnetic, optical, acoustic and microwave measurements. In some carefully selected materials they turn out to be very simple, and can be determined quite accurately. Such materials can then be used to study the third aspect of understanding magnetic properties, the macroscopic, cooperative behavior as a function of temperature and applied magnetic field.

The most interesting features that can be observed in such studies are singularities in the magnetic and thermal behavior that signal phase transitions. Many theoretical models describing phase transitions have been studied, and from our work we were able to identify a number of materials that closely approximate to one of the simplest, the Ising model. This allowed a number of critical tests of theoretical predictions. The material we studied most intensely was dysprosium aluminum garnet, $Dy_3Al_5O_{12}$, (commonly called DAG), which turned out to be a "fruit-fly" for the study of magnetic phase transitions.

Almost all our work entailed detailed studies of specially prepared materials, including other garnets and crystalline rare earth hydroxides. For this we had to build unique chemical synthesis facilities that then also provided samples to other research groups around the world. In the course of our work we also developed several useful new experimental techniques for measuring magnetic materials at very low temperatures.

All of our work was part of a broader movement to make new magnetic materials with specific characteristic properties. Many such materials are now widely used for practical applications, including electric motors, radar components, computer data storage, and medical YAG lasers. We ourselves never worked on any such devices, but I have always enjoyed consulting with my colleagues in industry who were working towards such applications.

But, in retrospect, probably the most important result of our research was its effect on our graduate students, who gained experience in dealing with a variety of complex problems, and almost all of them went on to amplify our efforts in industry and academia with marked success.

Other Interests

So far I've been talking only about the professional aspects of my intellectual trajectory, but throughout my life I have had other interests that I think should be mentioned, at least briefly. As I indicated at the beginning, my formal education, both at school and at Oxford, was really quite narrow, which left a lot of other things for me to learn. Thinking about how I became interested in other fields I realized that the big factor was again the influence of other people, just as it had been with my teachers, students and professional colleagues.

For example, it was my school friend Bernard who first interested me in chamber music and opera, and it was the opportunity to interact with colleagues at meetings all over the world that encouraged my interest in architecture and other cultures. My wife, Liz, got me interested in art and the spoken word, and my experiences as a child made sure that I was always keenly aware of politics. I always tried to make sure that I lived in an environment in which one could interact with people in all sorts of fields and, for the last 45 years, Yale has certainly been such an environment. The most recent highlight of the Yale experience has been the Koerner Center, which Liz and I have appreciated immensely. Maybe I'll end up civilized after all.