Millikan Lecture 1989: The Einsteinization of Physics

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I see this award not so much for me personally, as for all of us, a recognition not for a person, but for an activity in which we are engaged together. I hope that you share with me the sense of achievement, and the pleasure of receiving it, in a field that does not always get its proportion of recognition.

I would like to thank the AAPT for the award, and for allowing me to participate in so many of its activities. Most specially I extend my thanks to the New Jersey Section, and to Yvette Van Hise, its president. This is the group whose friendship, loyalty, and support have put me here, and whose collective achievement is being honored today.

I hope that this also a recognition of what we are trying to do at Rutgers University. My colleague George Horton is at home teaching summer school. My colleague George Pallrand is preparing for a wave of teachers who will arrive next week, some of whom are still here today. Brian Holton is here. As Director of the Physics Learning Center that George Horton and he founded, he has participated in many innovations, as we try to care on the one hand for those who are best at physics and most eager to learn and, at the same time, work at that much more difficult task, to bring in some of those who in the past have been shut off from sharing with us in the riches and the wonders of the world that we try to describe.

Finally I want to thank Robert Millikan, who has shown that a research physicist can also contribute in many other ways. The books he wrote, and most notably his laboratory manual, helped to set the pattern for the teaching of physics of a whole generation.[1]

I am uneasy about invoking the name of the greatest saint that we have. Saints, by their nature, tend to be remote and inaccessible, and they are very difficult to use as role models. I hope I am not deluding myself to think that he would be the first to understand what I am going to say.

I am discouraged by the posters and T-shirts with Einstein's picture on them, not just because his likeness is so often distorted and caricatured, in the face of the fact that he was one of the most interesting looking and photogenic persons of the century. I want to go a step further, and look with you at the nature of the image of physics which he is asked to project.

You are familiar with what so often happens when you meet someone and you say that you are a physicist. The other person says, "You must be smart." You try to accept this judgment, more or less gracefully. Your new acquaintance may then add, "A real Einstein!" and you realize that you are not being paid a compliment at all. Rather, you are being told, "I don't know what you do. In fact, I can't know what you do." In the back of his or her mind may be, "I don't really want to know what you do." And even, "I don't need to know what you do."

I would like to get away from that image of physics. With or without Einstein, physics is too often perceived as being not only impossible to understand but also irrelevant.

Part of what I want to do today is to give some examples of important contributions to physics which have the common feature that their essence can be very easily understood. They are observations and experimental achievements rather than theoretical deductions, so direct and straightforward that most people can probably identify with the discoverers, perhaps to the point of imagining that they might themselves be able to participate in such a discovery. The work is therefore very different from that usually associated with Einstein. We will make a short tour, at the end of which we will come back to Einstein, with a renewed sense of the variety of his achievements.

Along the way we will also have the opportunity to reflect on what is valuable, and what is rewarded, and on the fact that the two are not always in simple correspondence to one another.

The first person that I would like to talk about is Jocelyn Bell (Fig. 1). Figure 2 shows the apparatus that she worked with, from a book[2] with this dedication: "To Jocelyn Bell, without whose perceptiveness and persistence we might not yet have had the pleasure of studying pulsars."

She is the person who discovered pulsars. The figure shows the antenna array that she not only used, but helped to build, in the process, according to her own account,[3] getting quite good at wielding a 20-pound sledgehammer. Figure 3 shows the record of some of the first observations of radio-frequency signals that represent the discovery of pulsars, which we now know to be neutron stars, emitting signals as they rotate. The figure shows some interference, of which there seems to have been a fair amount in the trace that she was analyzing, and, almost undistinguishable from it, the evidence for the first pulsar. What you see here recurred once a day, or about every hundred feet of chart. She not only realized that something was there, but she also determined that it came not every 24 hours but every 23

Fig. 1. Jocelyn Bell Burnell. (Sky and Telescope.)

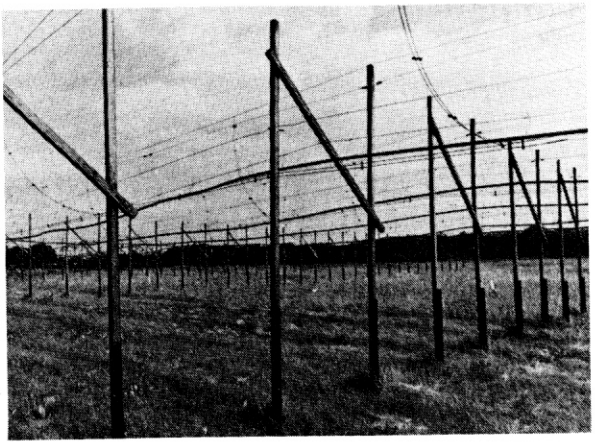
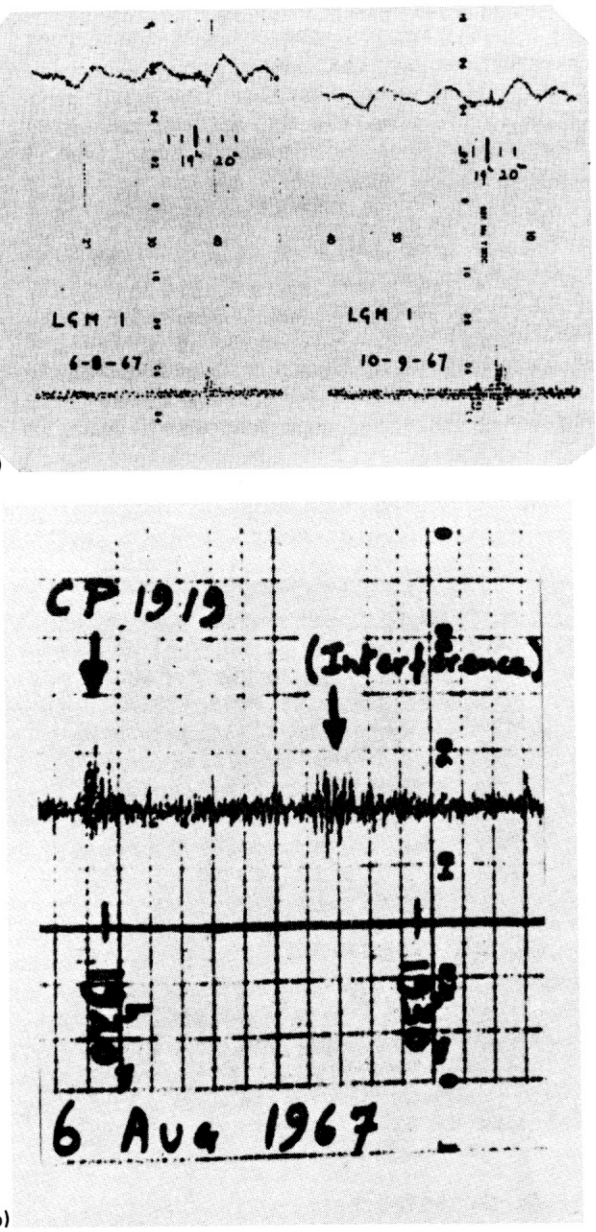


Fig. 2. Part of the antenna array at the Mullard Radio Astronomy Observatory in England with which pulsars were discovered. (From Ref. 2.)



(a)

(b)

Fig. 3. (a) Two records of "LGM1," the signal that turned out to be from a pulsar. Raw signal above and filtered output below. (b) Pulsar signal and interference. (From Ref. 3.)

hours and 56 minutes, a time interval that represents one sidereal day, in other words, the time of one rotation of the Earth as seen by someone not influenced by the motion of the Earth. You notice that she called the signals LGM, standing for "little green men," presumably those who were sending out the signals that she observed.

The astronomer Fred Hoyle comments on the discovery as follows[4]: "There has been a tendency to misunderstand the magnitude of Miss Bell's achievement because it sounds so simple—just to search and search through a great mass of records. The achievement came from a willingness to contemplate as a serious possibility a phenomenon that all past experience suggested was impossible. I have to go back in my mind to the discovery of radioactivity by Henri Becquerel for a comparable example of a scientific bolt from the blue."

The paper, when it came out, had six names on it, and Hoyle declares[4] that the finding had been kept secret for six months while others "were busily pinching the discovery from the girl." A Nobel prize was given for this work. It went, however, not to Miss Bell, but to her Professor, Anthony Hewish.

Even so, Miss Bell came out better than the person who first observed superconductivity. Gilles Holst didn't even get his name on the paper, and when he finally got a degree it was not from Leiden University where the discovery was made.[5]

The next story is also about an unexpected astronomical discovery. It is similar in some respects, quite different in others. As you will see, it also raises questions about the proper apportionment of credit and reward.

It concerns two people who were working for the telephone company. They were, in fact, interested in communication. Figure 4 shows Arno Penzias and Robert Wilson

Fig. 4. Penzias and Wilson inside their antenna. (Courtesy of AT&T Archives.)

inside the antenna which they were refurbishing, a large and sensitive version of the horn antennas now seen on microwave communication towers all over the country.

In order to test and calibrate their antenna they pointed it toward outer space and, to their annoyance, observed a small amount of background noise.

This is, of course, a situation familiar to anyone who has ever worked with electrical signals. You try to make sure that you have grounded your apparatus properly, that you have shielded it from interfering radiation, and at some point you decide that you have done all you can, and you accept and live with whatever background noise remains.

Penzias and Wilson did not do that. They were so sure of the excellence of their antenna that they did not accept the noise as irreducible. What they observed was not all that much, but it was equivalent to the amount of radiation emitted by a body at a temperature of about 3 K. And no matter what they did to their antenna they could not go below that, even though their very detailed and profound knowledge of its characteristics led them to expect that that should be possible.

They talked with their colleagues about the possibility of signals which they might not have eliminated, but came up empty. Eventually someone suggested that they talk to a group of physicists at Princeton University, less than an hour's drive from their laboratory in Holmdel. Indeed, at Princeton Roll and Wilkinson were setting up an antenna to look for radiation from outer space, and when they, with their colleagues Dicke and Peebles, saw the data that Penzias and Wilson showed them, they said, presumably with a mixture of excitement and disappointment, "we think we know what you found."

They explained that they were just about ready to start to search for radiation with precisely the characteristics of that observed by Penzias and Wilson, predicted to remain from what we think of as the moment of creation of the universe as we know it.[6]

Penzias and Wilson were pleased that there seemed to be some kind of rational explanation for their observations and submitted a paper[7] of about one page to the Astrophysical Journal, entitled "A measurement of excess antenna temperature at 4080 Mc/s". It was accompanied by a longer paper[8] in which Dicke, Peebles, Roll, and Wilkinson described what they believed to be the origin of the radiation, which turned out to be correct, as far as we can tell, resulting in a Nobel prize for Penzias and Wilson.

The third example is about an experiment—this one done by Einstein, at least under his direction and according to his ideas, in 1915. You may remember 1915 as the year when he finally came to grips with the general theory of relativity. In addition, however, he tried to respond to H. A. Lorentz, who had commended his son-in-law W. J. de Haas to his care. The outcome was a fascinating and fundamental experiment that has gone down in history as the Einstein–de Haas experiment.

It arose from Einstein's consideration of Ampère's suggestion, almost a century earlier, that all magnetism was the result of electric currents. Einstein wanted to test this hypothesis and try to obtain direct, mechanical evidence for the existence of the currents.

The idea of the experiment is to suspend an unmagnetized iron cylinder and to magnetize it by switching on an external magnetic field produced by a coil surrounding the cylinder. If, indeed, the magnetization of the iron is associated with "amperian" currents, i.e., with circulating charges having angular momentum, then the iron cylinder as a whole should experience a mechanical torque at the moment of magnetization.

They did the experiment and they observed the effect. A person who was particularly pleased with the result was Bohr. He had, after all, declared just a few years earlier that there should be persistent currents, electrons moving around nuclei without radiation or other dissipation of energy. He had every reason to believe that this was the nature of the currents observed by Einstein and de Haas, confirming the existence of electron orbits in stationary states.

So much for the qualitative part. It is also easy to calculate the angular momentum quantitatively. An electron with charge e moving in a circular orbit with period T represents a current e/T. For an electron with speed v in an orbit whose radius is r, $vT = 2\pi r$ and the current is equal to $ev/2\pi r$.

In modern (SI) units the magnetic moment is defined as the current times the area of the orbit and is therefore $\frac{1}{2}evr$. The angular momentum is mvr, and the ratio of the magnetic moment to the angular momentum, the gyromagnetic ratio, is $e/2m$.

It doesn't matter how big the radius is, or how fast the charges are moving, so that the gyromagnetic ratio is a rather fundamental quantity. An important part of the experiment is therefore to do it quantitatively, so as to be able to deduce from it an experimental value for the gyromagnetic ratio.

They did this and, according to the account given by Abraham Pais in his marvelous biography of Einstein,[9] they made two determinations. One led to a value for the gyromagnetic ratio of 1.02 times the expected result, or, as we would say today, a "g value" of 1.02, the other to a value for g of 1.45.

At this point Einstein and de Haas did something quite strange. They tried to estimate the probable experimental uncertainty and decided that it was about 10%. They concluded that the value 1.02 was right, and threw out the other one.

A good deal of the literature on saints, in the various religions with which I am familiar, describes the parts of their lives when they sinned. It is important, and sometimes helpful, to remember that this is characteristic for at least the more human saints.

The irony is, of course, as you know, but as Einstein could not know, that $e/2m$ is not the appropriate value of the gyromagnetic ratio. He could not know that while the magnetism of iron is indeed the result of charges with angular momentum, it is not the angular momentum of the orbital motion of the electrons, but that other angular momentum which electrons always have, the "intrinsic" angular momentum which we like to call "spin."

Even more ironically, it turns out that a proper relativistic calculation leads to the result that for this case the gyromagnetic ratio is e/m and not $e/2m$. The result which they threw out is, therefore, while not within 10% of the expected value, at least closer than the value which they kept. Not only that, but if they had persisted, and taken their result more seriously, they might have considered the possibility that there was a discrepancy, and perhaps discovered the spin angular momentum about a decade before it was actually contemplated and discussed as a result of spectroscopic evidence by Goudsmit and Uhlenbeck.

My object in describing some aspects of the achievements of Jocelyn Bell, of Penzias and Wilson, and of Einstein and de Haas is partly to illustrate the wide variety of the possible contributions to our knowledge and understanding. In part it is also to hint at the fact that it is possible to describe some of these great achievements at many levels of detail and sophistication. I want to show the relatively easy accessibility of the nature and ideas underlying some of the most fertile observations and experiments, with examples that rarely find their way into our fundamental courses.

I regret that physics has become almost proverbially inaccessible to the majority of the population. I wonder whether we can learn from other fields, where teaching is less abstract, and where contact between student and subject is more direct and more easily established.

Consider music. Suppose that musicians were to teach as, to some extent, we teach physics. They might start by saying "First you have to learn to draw five equally spaced parallel lines." After that they would describe circles and other figures to be put on or between the lines and call them "notes." They might go on to describe scales and the laws of sequencing and combination before eventually showing the possibility of a sensory interpretation involving sound and tunes.

There are other differences between the fields. Musicians tend to be much more open and inclusive than we are. A jazz musician does not look up or down on a classical musician or vice versa. They also seem to be more aware and accepting of the different kinds of people whose efforts contribute to the field in important, even essential ways. There are performers, interpreters, critics, teachers, instrument makers, historians—there is even an honored place for the musical amateur. In physics we act as if composing were the only activity worthy of the professional and valued by the community.

When I was a graduate student the recommended text on mechanics was the one by Whittaker.[10] Whittaker's book is famous for the fact that it has no diagrams. The author presumably considered those to be just a sop to the lazy and unimaginative, who could not look at a set of equations and see through them the phenomena that he was trying to describe.

All of us who deal with physics have, to a greater or lesser extent, learned to do what Whittaker expected, and we tend to forget that just looking at a page with equations may not give everyone the emotional charge that we get from it.

Again there is a parallel in music. A composer, probably any musician, can, to some extent, look at a page of music and feel, not just imagine, but feel, something of the music that it represents.

The most famous and remarkable example is that of Beethoven. Figure 5 shows him three years before his death, at a time when, as far as we know, he could hardly hear anything. This was one year before he composed the string quartet, opus 130, of which you see in Fig. 6 the manuscript of the fifth movement, the Cavatina, a wonderful piece of music which, presumably, he never heard except in his mind.

For most of us the emotional impact comes only with the listening, and it is not so different when we deal with physics: We have to find new ways of communicating with those for whom the equations are not as transparent as they are for us.

As we look out on the sea of faces of our students I hope we can catch ourselves when we see that we are about to be pedantic and exclusive or dogmatic. I hope we can forget for a moment the scales, the laws, the abstract equations, and not lose sight of the music.

The music can come in many ways. For some it will come in the drama of the persons involved, for some in the historical context of a development or achievement. It can be in the philosophical structure, in the examination of that which is learned and that which is unsaid, the questions which remain, the answers which are untouched. Certainly also, but sparingly, in the abstract beauty of the equations and the theoretical structure. Most importantly in the direct communion with the phenomena themselves.

If we can broaden our outlook and methods perhaps we can become better at including those whom we so often fail to reach: the fledgling, the tinkerer, the reader, the amateur, those who are turned off by the arrogance of knowledge, those whose expectations have been diminished by failures that were expected of them.

Fig. 5. Beethoven in 1824. Chalk drawing by Stephan Decker. (Historical Museum of the City of Vienna.)

Fig. 6. Manuscript page for the fifth movement ("Cavatina") of the quartet, opus 130, by Beethoven, composed in 1825. [Staatsbibliothek, Berlin.)

Fig. 7. Einstein on sailboat, 1936. (Niels Bohr Library, American Institute of Physics.)

If they are not nurtured by us, who will need or want the interpreter and the practitioner? Where will be the pool from which the composer will come?

Next week I will meet a group of high-school teachers and participate in work with them on semiconductors and transistors, lasers, magnetism, superconductivity, and resonance. Will they be ready to become composers in these fields? Probably not. But perhaps they can hear, and help to transmit to their students, some of the music.

There was a time when physics needed to be rescued from engineering. Physics courses emphasized gadgetry, machines, and miscellaneous applications. We have succeeded beyond our expectations. Physics is held in respect, even awe, but it has removed itself into castles surrounded by hostile moats. Those inside take only sporadic interest in the world outside—the world that must support, regenerate, and repopulate the castle; the world that itself needs the achievements, the products, the insights of those within. We must break down the barriers, build bridges over the moats, open up the structures, the castles, where we live and where we build and work.

Einstein is often pictured as an old man looking at a blackboard filled with incomprehensible equations. I prefer to think of him as he is in the picture shown on Fig. 7 as a much younger, vigorous man on a sailboat. And if he stood on the beach and looked out into the unknown, he was firmly on and of this Earth as he tried to find the most direct path toward an understanding of its phenomena.

He heard and understood the music as only a great composer can.

As we try to follow him, I hope we remember that there are many ways to hear the music, many ways to open the ears and minds, to touch the hearts, of those who are in our care.

[1]Alfred Romer, "Robert A. Millikan, physics teacher," Phys. Teach. **16**, 78 (1978).

[2]Richard N. Manchester and Joseph H. Taylor, *Pulsars* (Freeman, San Francisco, 1977).

[3]Jocelyn Bell Burnell, "The discovery of pulsars," in *Serendipitous Discoveries in Radio Astronomy*, edited by K. Kellermann and B. Sheets (National Radio Astronomy Observatory, Green Brook, WV, 1983), p. 160.

[4]Nicholas Wade, "Discovery of pulsars: A graduate student's story," Science **189**, 358 (1975).

[5]For a personal account, see H. B. G. Casimir, *Haphazard Reality* (Harper & Row, New York, 1983).

[6]R. A. Alpher and Robert Herman, "Reflections on early work on 'big bang' cosmology," Phys. Today **41** (8), 24 (1988). See also David T. Wilkinson and P. J. E. Peebles, "Discovery of the 3 K radiation," Ref. 3, p. 175 and Robert W. Wilson, "Discovery of the cosmic microwave background," Ref. 3, p. 185.

[7]A. A. Penzias and R. W. Wilson, "A measurement of excess antenna temperature at 4080 Mc/s," Astrophys. J. **142**, 419 (1965).

[8]R. H. Dicke, P. J. E. Peebles, P. G. Roll, and D. T. Wilkinson, "Cosmic black-body radiation," Astrophys. J. **142**, 414 (1965).

[9]Abraham Pais, '*Subtle is the Lord...*' (Oxford U. P., New York, 1982), p. 245.

[10]E. T. Whittaker, *A Treatise on the Analytical Dynamics of Particles and Rigid Bodies* (Dover, New York, 1944), 4th ed.

Peter Lindenfeld: Recipient of the Robert A. Millikan Lecture Award

The Robert A. Millikan Lecture Award is given annually for "notable and creative contributions to the teaching of physics." The Awards Committee of the American Association of Physics Teachers is pleased to present the 1989 Millikan award to Peter Lindenfeld, Professor of Physics at Rutgers University. Historically there has been some emphasis on contributions to experimental or observational physics and to skill in lecturing for this award, although other contributions and factors are considered as well. Peter Lindenfeld is eminently qualified on all scores.

Since receiving his Ph.D. in 1954 from Columbia University, Peter advanced rapidly through the ranks at Rutgers to become Professor in 1966. He turned out to be that rare combination of distinguished researcher and inspiring teacher. In his field of superconductivity and other properties of metals he has published 65 papers and has supervised 19 Ph.D. theses, an important but often unrecognized form of teaching and mentoring. Many former graduate and postgraduate students of Peter's occupy key positions in physics research and administration today. Peter has taught courses at all levels with equal enthusiasm and effectiveness, students regarding him as a superlative and caring teacher. He seems to evoke the best from his students by making the subject matter fascinating and conveying his love of the subject without sacrificing depth or rigor.

Peter has contributed to the advancement of teaching in many other ways. He has presented papers at professional society meetings on teaching—related topics, published papers in the AAPT journals, engaged in significant educational and curriculum development both within and outside his university, served on important committees of the AAPT and the APS that deal with educational matters, and on the editorial board of *The Physics Teacher*. Peter has been on the executive committee of the New Jersey section of the AAPT almost since its inception and is regarded as the father figure to whom New Jersey high-school physics teachers turn for advice and encouragement. He is the editor of the Newsletter of the College–High School Interaction Committee and is part of a group developing "physics modules" on subjects including semiconductors, lasers, magnetism, and superconductivity for use in high schools. He is chairman of the executive committee of the Center for Mathematics, Science and Computer Education at Rutgers University and was the originator there of the degree of Master of Science for Teachers.

Peter has received prizes for devising a solar calorimeter and for writing about "Radioactive Radiations and Their Biological Effects." In 1988 he was awarded the Warren Susman prize at Rutgers for excellence in teaching. He is a Fellow of the American Physical Society and serves very effectively as a bridge between the community of research physicists and the physics teaching community. This is exemplified by his present work, which includes the development of a new method for precision thermal measurements of high-temperature superconductors and the preparation of experiments and materials for summer institutes for high-school teachers.

Peter complains that he has too little time for writing, for exhibitions of his photographs, for cooking, for mycology, for playing the recorder, and for the continued study of languages, which in the past have included Japanese and classical Arabic.

We are proud to honor him today as the 1989 Millikan Lecturer. The title of his lecture is "The Einsteinization of Physics."

Robert Resnick
Chair, AAPT Awards Committee
29 June 1989