retical side of the sciences. But as brilliant and convincing as his observations and his astrophysics were, his research into the fundamental laws of physics never carried conviction. Eddington was the first man in England to learn relativity. The experience transformed his outlook, and almost immediately he set about to finish what he perceived Einstein had begun. In 1921, he published his own version of a unified field theory. Other papers followed, slowly, but with increasing rapidity. Toward the end of his life, as he devoted more and more time to the unification of general relativity with Dirac's quantum mechanics, he drifted farther and farther from the mainstream of physics.

There is no real mystery as to why Eddington's ideas met with such ambivalence during his lifetime; but to understand the reactions of contemporary scientists, one must consider briefly what Eddington was trying to do. His goal was simple: He wished to deduce the fundamental laws of physics, as known in his day, from axioms that no one could self-consistently object to. By fundamental laws, he meant laws such as Einstein's field equations and Dirac's equation. The axioms that led to these laws came from a careful examination of the ways in which we viewed the world. Eddington's goal was quite reasonable and falls within a tradition that included Hilbert, von Neumann, and many mathematical physicists of today. What distinguished Eddington was that he believed he had completed such a deduction. Unfortunately, his scheme was obscure and out of step with his times. During the very years in which Eddington was trying to deduce a comprehensive view of the universe, physics was exploding with new facts and new theories that led to still more new facts. Eddington's work was ignored because the vast majority of physicists were uninterested in pursuing ideas that did not lead to *new* physics.

The other reason that Eddington's ideas met with such reluctance is psychological. During the twenties and thirties, Eddington was a premier popularizer of the physical sciences. Not only did his books discuss the recent advances in physics, they also fit them within the framework of his deductive scheme. One can easily understand the resentment some of Eddington's contemporaries must have felt upon seeing unproven assertions delivered in public with an aura of authority befitting England's leading astronomer.

Many years have gone by since the controversies Eddington raised in the philosophy of science died away. Now, Paul Nawrocki (March 1981, page 81) and A. J. Coleman (December, page 72) herald Eddington as the unsung progenitor of modern particle physics. Many of Eddington's ideas are remarkably modern. But even where he was right, he was right for the wrong reasons. Eddington was trying to build a deductive system in which every statement was vital. Make one serious mistake and the structure crumbles. Eddington's prediction that the fine structure constant was exactly the reciprocal of 137, his equation of state for a massive white dwarf, and his strong force potential of $e^{-r^2/k^2}$ are well known but hardly the only examples of where Eddington's deductive structure contains serious gaps. If certain ideas look appealing when examined individually, we must remember the context from which they were drawn. The world Eddington thought he had deduced no longer exists; it has been replaced by one that is infinitely richer and more complex. Eddington had a prescient view of physics, but the history of science teems with people who had the right idea at the wrong time and in the wrong place. Out of phase with history, their achievements effectively cancel each other out. It is a reflection of Eddington's true greatness and breadth of interest that he is still so revered despite the neglect of his later work.

Eddington was the first second-year student to win the Cambridge Mathematical Tripos. In her biography of Eddington, A. V. Douglas relates how he paid tribute to his former teacher Sir Horace Lamb by saying that "while he now knew what it was to be treated as something of a lion his ambition was to become something of a Lamb." Quiet, modest, and almost painfully shy, Eddington sought neither the uncritical adulation nor the unwarranted neglect that seem to be his fate. He was born a hundred years ago this December. As we celebrate his centennial, we may do him no greater honor than to recognize and to continue his work on stellar models. We may do him no greater disservice than to turn him into what he was not, thereby casting him like Daniel into a den of lions.

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*1/82*

## Organic superconductivity

D. Jérome's comment following M. Revzen, A. Ron and J. Zak's letter in September (page 104) might be misread. This comment did not wish in any way to minimize the work of the Jerusalem group in the superconductivity of chain compounds, which we both consider a significant contribution to the field. Moreover, we wish to acknowledge M. Weger's initial impetus and inspired contribution to the development of the research on organic conductors during his fruitful scientific collaboration with the Orsay group.

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## Advice to lecturers

The fact that you have thought it necessary to print Darrow's article "How to address the APS" (December, page 25) three times in thirty years should lead you to question the effectiveness of your approach to improving conference presentations. Can preaching really teach teaching? Your evidence suggests that it cannot. Let me add some further evidence to support this conclusion. The Royal Institution in London—famous for its lectures—has published a little volume 1, the preface of which states that "the only way to learn to give a good lecture is by experience—often bitter at first. But some of the more excruciating moments can be avoided by giving attention to the hard-worn practical wisdom of fine lecturers." And yet, I have experienced such excruciating moments occasionally even at the Royal Institution.

The real problem is that most university lecturers do not lecture well because they lack certain skills and this lack may even be thought to be endearing. This last point is illustrated in the following quotation from an article on Professor Siegbahn, which celebrated his Nobel Prize.[2]

> He was one of the key invited speakers . . . and I well remember his talk. Professor Siegbahn made the mistake *we all make when we get carried away by our subject* (my emphasis). He spoke for almost a full hour about an aspect of his apparatus and, when it became apparent that time was running out, suddenly discovered that there were some 40 slides of data he wanted to show and had not come round to. Much to the Chairman's annoyance, he then spent a good ten minutes whipping through what in effect was the main part of his talk at a rate of some 15 seconds per slide.

The acquisition of competent lecturing skills—like that of any other skill—requires training, and as long as university teachers do not consider such training necessary, inadequate lectures and conference presentations will be the rule rather than the exception. As I am not hopeful that this situation will change soon, I am looking forward to

# letters

reprints of Darrow's article in 1991, 2001, . . .

### References


1. G. Porter and J. Friday (eds), *Advice to Lecturers*, The Royal Institution, London (1974).
2. J. P. Connerade, *Physics Bulletin* **33**, 24 (1982).



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*2/82*

## Repression in Turkey

In an editorial (January 1974, page 128) you invite physicists to use the pages of PHYSICS TODAY for matters to be brought to the attention of physicists, and you mention the magazine's worldwide circulation. I am not by any means certain that scientists would appreciate learning of the following, but I feel a duty to write it as it is.

In the period 1962–64 I was visiting as a physics lecturer at the then internationally funded Middle East Technical University, Ankara, Turkey. Seventeen years later I have received a communication from a former physics graduate student (living now in Cambridge, England) who desires my help. Because I think it reasonable for academics to realize to what extent repression is practiced upon physicists (and others) outside the USSR, I quote from her letter:

> My sister, who is an experimental physicist, was a lecturer at Adana University until seven months ago. That is when she was arrested and detained for 90 days. During that period she was kept without food for 3 days and subjected to harsh interrogation (although not actually tortured). I now have documents compiled by Amnesty International with details of much worse cases of harsh treatment and torture in Turkey. Some actually die after such treatment. All this happens during detention, when no one, not even a lawyer, is allowed to visit. She was charged with being (formerly) a member of the Communist Party; yet she was only a member of a progressive women's organization. She is now in the civilian prison run by the military, together with 120 women, most of whom are political prisoners, the rest ordinary criminals. They are in a ward designed for 40 people; three in one bed. I managed to see her in prison behind wires when the temperature was 40° C in the shade. We ourselves couldn't bear the heat without taking showers every so often. Her husband is finding it hard to accept that they should be treated in this manner. He is a professor of physics and was the dean of the faculty until her arrest; he resigned in protest. It is the military authorities who are in charge of all appointments or sackings. Recently they also put the former Prime Minister Ecevit in prison for criticizing the regime's decisions. We have been writing to scientists and to Amnesty International groups, but she is still in jail.

Last year I was notified of a Fulbright Teaching award to the USSR, but I have decided, for many reasons, against taking it. Also, happy as my memories are of the wonderful and friendly people of Turkey who would always enjoy the chance of a chat over a cup of "cay," I do not believe that any of us should fail to object to the above kind of repression.


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*2/82*

## Wrong Brillouin

Alfred Kastler has called my attention to the fact that my article on Rayleigh scattering in January (page 42) incorrectly attributes the Brillouin effect to Marcel Louis Brillouin, the father of the actual dicoverer, Leon Brillouin. While my original manuscript referred to the paper by L. Brillouin [Ann. Physique (9) **17**, 88 (1922)], this was removed in the editing process, and the name of the wrong Brillouin was inserted. I did not see proofs of the published version, and might not have caught this error anyway, as I did not notice it until Kastler wrote to me.

It might be of interest to quote from Kastler's letter:

> The father, Marcel Louis Brillouin, born in 1854, was Professor of Theoretical Physics at the Collège de France in Paris where he retired in 1932 and died in 1948 at the age of 94. On the well known picture of the participants of the first Solvay conference 1911 (see, for example, the "Einstein Centenary" Volume, 1979, edited by A. P. French, page 146) he is sitting on the table, the second from the left, between Nernst and Solvay, Max France in 1940 at the invasion of the German army, living in the USA till his death in 1969. It is Leon (and not Marcel) who has made the theory of light scattering by the thermo-elastic waves in Planck staying behind him. His son Leon Brillouin, born 1889, succeeded his father in the chair of the Collège de France in 1932. He left