# Popular and unpopular ideas in particle physics

**As particle physics becomes a deeper but more difficult field, there is too much emphasis on fashionable ideas and the search for anomalies, and too little reward for improving accelerators and detectors.**

Martin L. Perl

I finished writing this article while the 23rd International Conference on High-Energy Physics was taking place in Berkeley, California. It was a well-organized conference with a variety of theoretical talks, comprehensive plenary lectures and many reports on new experiments. Yet the atmosphere of the conference could be summed up in one word: waiting. The participants knew that no major experimental or theoretical advance in particle physics was to be announced at this conference. They listened to the experimental and theoretical results, but they were prepared to wait until the next conference for a breakthrough.

Much of particle physics is in a period of expectant waiting. We are waiting for the first results from the new accelerators—TRISTAN, LEP, the Tevatron Collider, the SLAC Linear Collider. We are waiting to see if new and quantitative predictions can be deduced from superstring theory, predictions that present experimental techniques can test. We are waiting to see if anomalous effects reported in the last few years will stand up to further experimentation.

The world's particle-physics community is industrious and adventurous in this waiting period. We are constructing new accelerators and their attendant detectors as fast as technology and funds will allow. We are improving experimental methods to get greater precision and to probe for anomalies in our standard ways of understanding elementary particles and the forces between them—the "standard model." We are extending theoretical calculation and speculation in all directions, connecting with astrophysics and cosmology. We are working hard to extend the revolution that occurred over the last two decades in particle physics. However, there are some small changes we can make to increase our chances of continuing that revolution; that is the subject of this article.

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## Deeper and narrower

It has become harder to make substantial progress in particle physics because we have redefined particle physics to be a deeper but narrower field. We concentrate on the deepest questions: What sets the masses of the particles? How can the four forces be unified? We attribute little importance to discoveries that we don't believe are central to the field. For example, 20 years ago the discovery of an additional hadronic resonance was an important event in our world; now such a discovery gains no recognition beyond a new entry in the particle data tables. There is nothing we can do to reverse this. The discoveries of the last 20 years have taught us that some measurements are central to particle physics but others are too peripheral or too complex to be of direct use.

This narrowing of the field has been harder on the experimenter than on the theorist. The theorist can extend his or her interests into general relativity or cosmology or the borderline between nuclear and particle physics. The experimenter, faced with long, complex and expensive experiments, must always ask, "Will this experiment probe into the heart of matter?" The narrowing of the field and the harsher measures of progress have led to two deleterious tendencies in particle physics. One of these tendencies, acceptable and understandable at first glance, but damaging on closer analysis, is the emphasis in particle physics on popular and fashionable ideas rather than unpopular and unfashionable ideas. (As figure 1 indicates, this problem is not unique to particle physics.) The other tendency is the addiction to the search for experimental anomalies.

## The fashionable and the popular

A theoretical concept or an idea for a model in particle physics may be popular and fashionable for good reasons. The concept may promise to explain a large set of data; it may point to new



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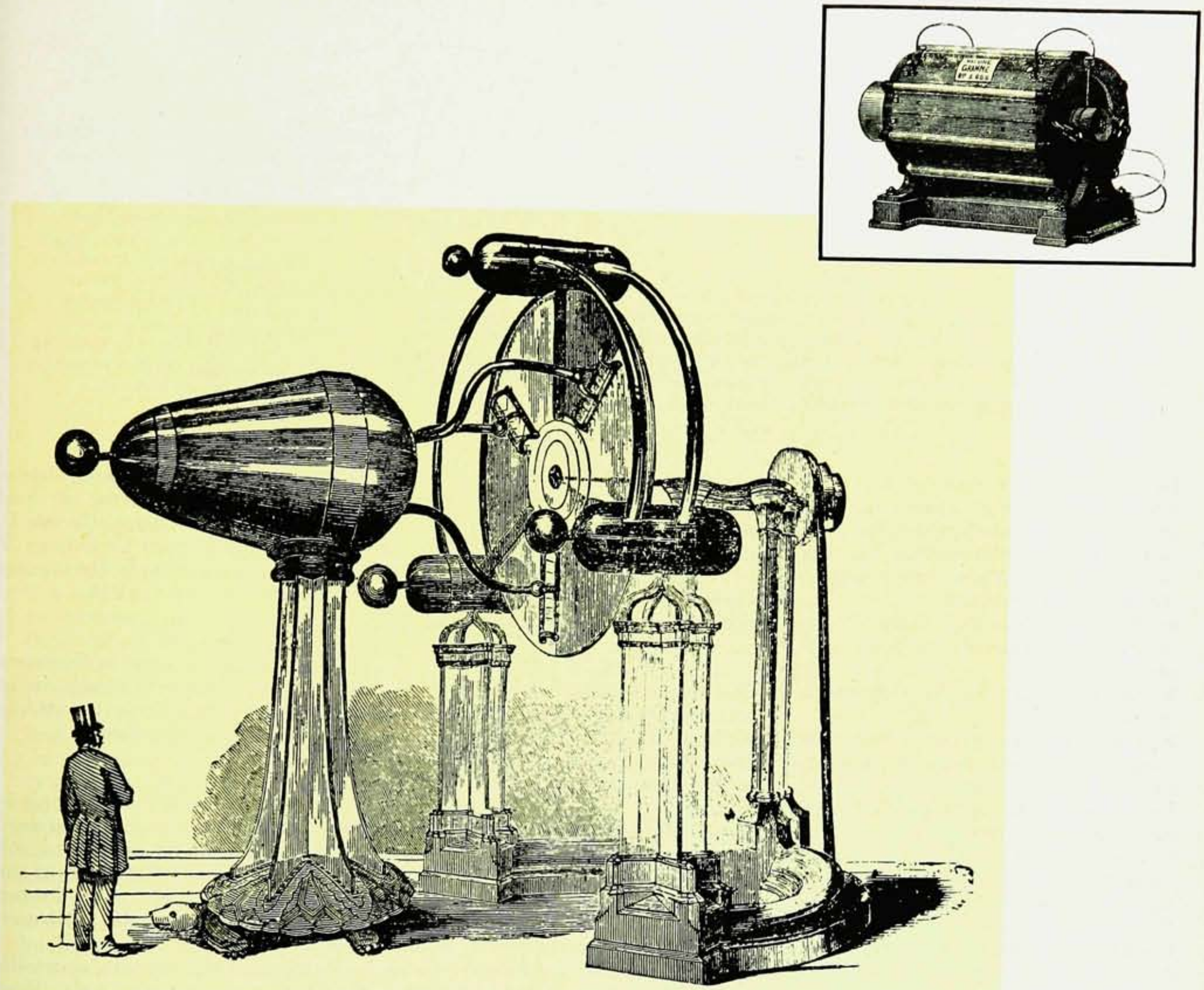


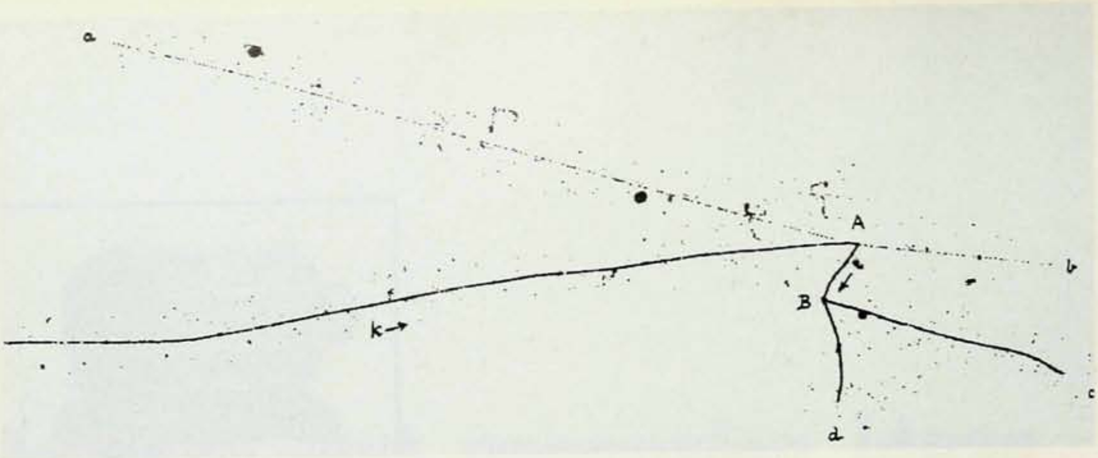
**Fashion and the future.** In the second half of the 19th century, large static-electricity generators, such as this machine with its 10-foot-diameter glass plate, made popular and dramatic displays. But the future of electromagnetic technology lay with the ungainly electric-current generator (inset). Paradoxically, once static-electricity machines had sunk into obscurity, the idea was reborn in the Van de Graaff generator. Figure 1

directions for experimental exploration; it may connect particle physics with other areas of physics. However, a concept may also become popular and fashionable simply because it gives the experimenter something to search for and the theorist something to calculate.

An example is the question of whether there are neutral leptons other than the neutrinos that are associated with the three generations of leptons—the electron, muon and tau—and, more particularly, whether there are neutral leptons that have mass. The known neutrinos present a simple experimental picture in which each neutrino is associated with its own charged lepton and each has a mass close to zero compared with the mass of the charged lepton. At present there is no confirmed evidence that the masses of the neutrinos are other than zero. Nor is there evidence that any coupling exists between a neutrino in one generation and the charged leptons in the other two generations.

In thinking about the possibility of additional neutral leptons, the simplest extension of the present picture is to consider the existence of additional generations of neutrino and charged-lepton pairs, with the neutrino having zero mass. In this minimum extension there would be no coupling between the new neutrino and any of the known charged leptons.

If you look through recent literature[1] on massive neutrinos, whether experimental or theoretical, you find that this model is not popular. On the contrary, popular models assume, or at least




**Kaon decay in an emulsion.** The decay $K^{\pm} \to \pi^{\pm} + \pi^{+} + \pi^{-}$ illustrates the importance of anomalies. The decay of a hadron into nothing but hadrons on a time scale many orders of magnitude slower than the usual time for hadronic interactions was an anomaly. It was eventually explained as the effect of a new, approximately conserved quantum number, strangeness. Track k is the incident K meson, which decayed at point A after nearing or reaching the end of its range. The particle going from A to B is identified as a $\pi^{-}$ meson, which produced a nuclear disintegration at B. The other two tracks coming from A are taken to be $\pi$ mesons, but the possibility that one or both are $\mu$ mesons could not be excluded by measurements in the emulsion. This picture is magnified; track k is actually about ½ mm long. (From reference 3.) Figure 2

hope, that the new neutrino will have nonzero mass. Most models also assume nonzero coupling between this new neutrino and the electron, muon or tau generation. Three ideas suggest the second assumption:

▶ The present standard model of elementary particles allows such coupling.

▶ Such coupling occurs between quark generations.

▶ A new symmetry principle would be required to prevent coupling between the lepton generations.

Furthermore, the assumption of nonzero coupling gives experimental and theoretical particle physicists something to work on. The experimenter can try to detect the new neutrino through rare decays of mesons or through its production and subsequent decay in electron–positron annihilation. The theorist can construct models that draw on the analogy with the coupling between quark generations.

Yet these fashionable nonzero-coupling models for massive neutrinos are in contradiction to other basic ways of thinking in physics. Usually we look for the simple solution, and if nature presents us with a simple and regular pattern, we try to understand that pattern, not complicate it. The introduction of nonzero coupling between lepton generations is certainly a complication. To first order there is nothing wrong with our working, experimentally or theoretically, with such nonzero-coupling models. However, to second order these models may be popular not because they are obvious or elegant or solve a mystery, but merely because they give us something to work on.

For the zero-coupling model, by contrast, there appears to be nothing new theoretically that can be said, and experimental work with it is difficult and indirect. Current experimental work is restricted to setting a limit on the number of small-mass neutrinos using the resonance width of the $Z^0$ or the cross section for the reaction

$$e^{+} + e^{-} \to \nu + \bar{\nu} + \gamma$$

Our experimental reach will improve drastically when LEP and SLC begin operation. Then we can make much better measurements of the $Z^0$ width and the cross section for the above reaction.

Two other examples of fashionable concepts in recent years are supersymmetry and axions. One or both ideas could be right, but in view of the continued lack of evidence for their validity, it is surprising how much experimental and theoretical work was, and still is, done on these concepts.

The first remedy for the overemphasis on the fashionable and the popular in particle physics is for the old hands in the field to regain their balance and judgment. Those of us who watched the rise and fall of Regge poles in the 1960s should be cautious about supersymmetry and axions in the 1980s. The second remedy is for the new hands to be more skeptical: "If the old hands know so much, why don't they know the answer?"

## Anomalies

In the past decade the particle-physics community has become addicted to the search for experimental anomalies. This addiction is useful to a certain extent but is sometimes injurious. Anomalies have played an honorable and important part in the history of particle physics:

▶ The strange particles were discovered as anomalous events in cloud chambers and emulsions. George D. Rochester and Clifford C. Butler's V particles turned out[2] to be the strange particle that we now call $\Lambda$:

$$\Lambda \to p + \pi^{-}$$

▶ The four-track events[3] of R. Brown and her coworkers were anomalous, as figure 2 explains. These events are now known to be the decay

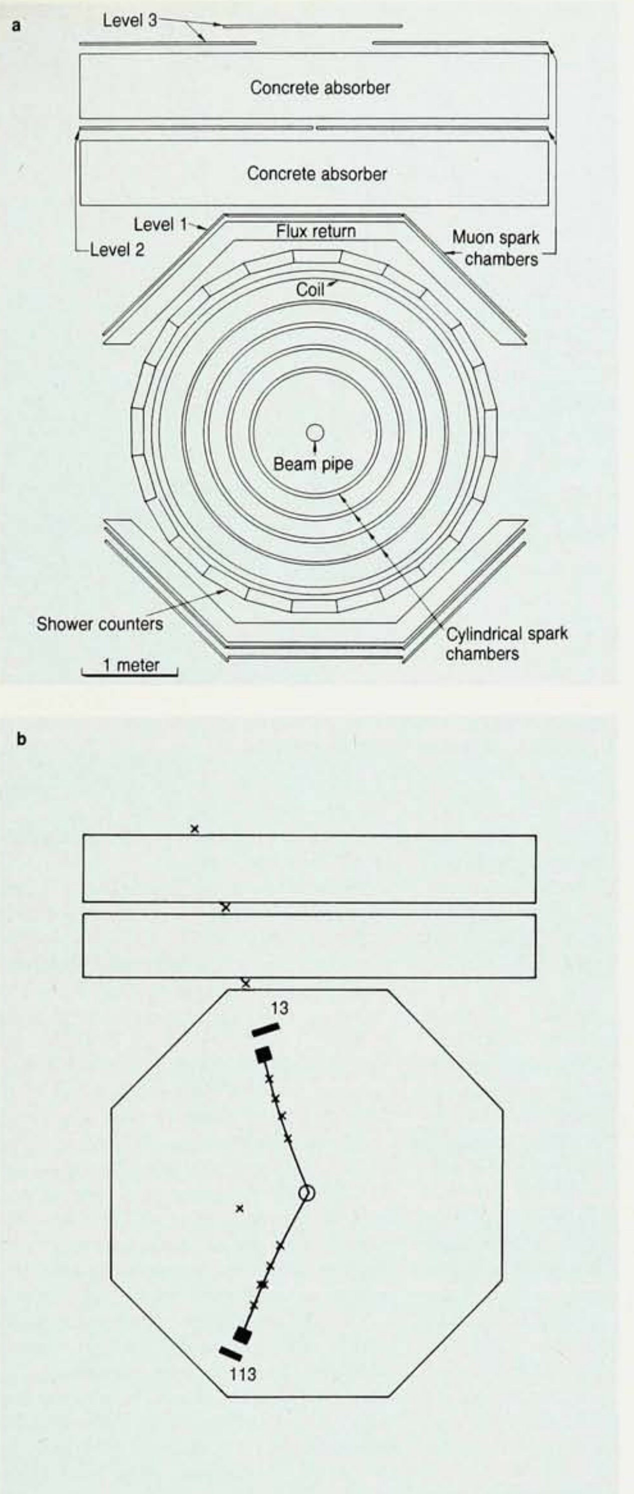
$$K^{\pm} \to \pi^{\pm} + \pi^{+} + \pi^{-}$$

▶ An anomaly still unexplained in spite of two decades of more and more precise measurements is CP violation in K-meson decays. The decay of the long-lived neutral K meson to two pions, for example, violates CP conservation.

Searches for experimental anomalies can be injurious in several ways. First, too much emphasis on searching for anomalies may pull us away from another path to progress in physics—the steady accumulation of data that gradually forces a change in how we view the physical world. Second, too much emphasis on the importance of experimental anomalies may have bad effects on the young women and men entering particle physics. They become distracted by a rush of talks and papers on possible evidence for a new axion, or for a new heavy lepton, or for a fifth force. Excitement in the field is fine, but it should not distract young physicists from learning the craft of physics. In the learning of any craft, one needs time to learn the methods, to develop one's own thoughts, to build confidence in one's skill.

I am not advocating that the experimenters suppress evidence for experi-

Detector and particle tracks. a: The Mark I detector, sketched in cross section. This was one of the first large-solid-angle, multiple-purpose particle detectors. b: An electron–muon event resulting from the decay of a pair of tau leptons. The track at 7 o'clock is due to an electron, as indicated by the large amount of electromagnetic energy deposited in the shower counter. The energies are given in arbitrary units. The track at 11 o'clock is due to a muon, as indicated by its passage through the iron flux return and the two blocks of concrete. Figure 3

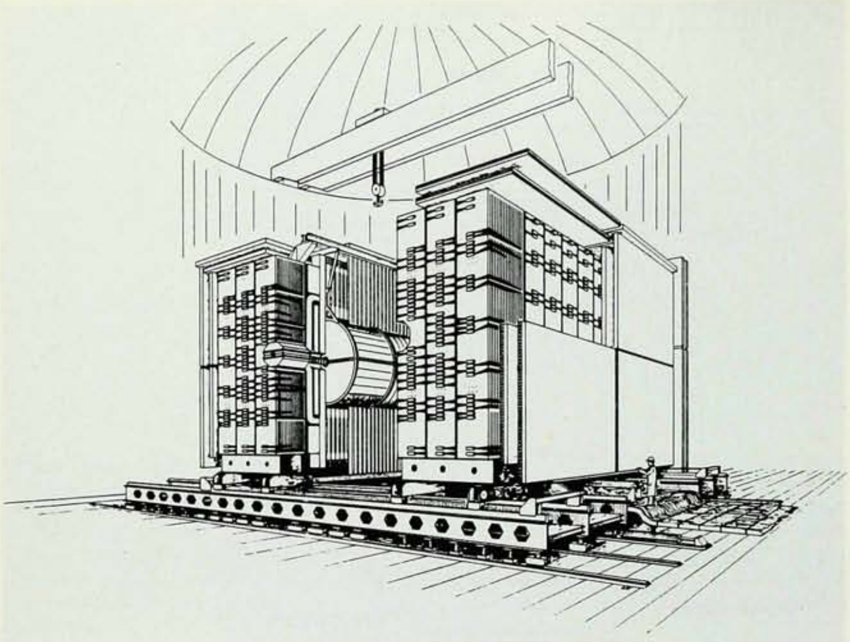



mental anomalies. On the contrary, it is the duty of an experimenter to talk about or publish an experiment; this is crucial to the life of science. The injurious secondary effect is the positive feedback that occurs between experimenters and theorists. This feedback occurs because theorists, being human, are eager to explain new experimental results and to explain them first. Experimenters, also being human, are equally eager to interact with the theorists and provide them with the data they need for their theories. The cure here—and we need only a second-order cure—lies within the power of the journal editors and conference organizers: Damp the positive feedback by limiting the number of published explanations of new and untested data, by spacing the talks on new and untested speculations based on preliminary data and by waiting before organizing conferences on new areas.

Fashion in theories can have an effect similar to that of addiction to the search for anomalies. Many people bemoan our increasingly mathematical and speculative particle theories, and their distance from present experiments. I don't much like the separation either, being a poor mathematician and having been an engineer before becoming a physicist. But there is nothing to be done. Nature dictates the correct theory; if it is mathematical and remote we still have to live with it. My concern is with the effect on experimental directions of fashionable, but unproven, speculative theories. As I have discussed[4] at greater length elsewhere, we need experiments that cover broad areas, experiments that simultaneously acquire data on known phenomena and make wide searches for new phenomena. Experiments based on speculative theories and with narrow goals teach us little if the answer is no—only that the theory is wrong or, more likely, that the parameters in the theory need adjustment. Again it is the time and energy of the young experimenter that are most at risk. One may argue that the young have infinite time and energy, but half-decade or longer experiments use up time, even for the young.[5]

## Large detectors, large accelerators

Many people also bemoan the great increase in the cost, complexity and size of accelerators and particle-physics experiments. We have no choice with respect to accelerators. Almost all recent progress in particle physics has



**The UA1 detector** at CERN, which produced the $Z^0$ event shown in figure 5. Note the large size of the detector as compared with the person shown standing at lower right in this sketch, or even as compared with the Mark I detector, shown in figure 3a. Figure 4

been made by building higher-energy and more diverse accelerators and colliders. In contrast, in recent times we have learned much less about elementary particles from nonaccelerator experiments, and it is not for lack of trying.

Sometimes there appears to be a choice in experiment: for example, many small particle detectors at an accelerator instead of a few, large-solid-angle, multiple-purpose detectors. However, large-solid-angle, multiple-purpose detectors have two great advantages. First, there is the obvious increase in the efficient use of accelerator time and apparatus when one detector collects data for many experiments at once. Second, a multiple-purpose detector is more powerful than the sum of its components. One can study reactions that cannot be studied by small experiments, and one can do measurements and searches beyond those conceived by the builders of the detector.

For example, we discovered[6] the tau lepton with one of the first large-solid-angle, multiple-purpose particle detectors, the Mark I at SPEAR. We used the reaction sequence

$$\begin{aligned} e^+ + e^- &\rightarrow \tau^+ + \tau^- \\ \tau^+ &\rightarrow e^+ + \nu_e + \bar{\nu}_\tau \\ \tau^- &\rightarrow \mu^- + \bar{\nu}_\mu + \nu_\tau \end{aligned}$$

The Mark I, shown in figure 3, could identify both electrons and muons. If we had used two separate detectors in separate experiments—one detector sensitive to electrons, the other to muons—it would have been much harder to find the tau.

Modern large-solid-angle, multiple-purpose detectors are bigger and more complex than the Mark I detector, but they also have much greater ability to acquire and analyze data. The UA1 detector, used for the discovery[7] of the W and $Z^0$ particles, is an example (see figures 4 and 5). Such detectors are the only way we have to study most of the processes that occur in very-high-energy particle collisions. Whether or not we like the cost and sociology associated with the building and use of such detectors, we have no choice as a community: These detectors are essential for progress in particle physics.

The necessity for large accelerators and large detectors impels us intellectually and practically to find ways to improve these devices, increase their experimental reach and minimize their cost, size and complexity. What can we do to stimulate technical progress and invention in accelerators and detectors? I'll address this question for detectors; the answer for accelerators is analogous.

### Publish what or perish?

Our reward and recognition system for the experimenter—the PhD, tenure, grants—is based mostly on the publication of experimental results. A graduate student's most useful achievement may be building an efficient trigger system for a multiple-purpose detector, but the faculty will award the PhD for one more measurement of the lifetime of a D meson, B meson or tau lepton. An assistant professor in a collaboration may be valued for his or her ability to design drift chambers, but this physicist's tenure request must contain a substantial list of the collaboration's published results along with a complicated explanation of who did what. In considering a grant request from an experimenter, the physics staff at a funding agency is most interested in the experimental strength of the applicant's group, but a list of publications of experimental results is still needed for the files.

We need more powerful detectors, more compact detectors, more efficient detectors and cheaper detectors, yet the reward system gives little direct recognition for contributions to such goals. This is perverse because it is the physics community itself that sets up the reward system by setting the standards for experimental achievement in particle physics. It is also perverse historically, because the traditional view of an experimental physicist is someone whose work designing and building apparatus is inextricably mixed with the collection and analysis of data.

Our recognition through publication also is counterproductive. As soon as graduate students begin their work in experimental particle physics, they learn that the most prestige comes from publishing in *Physics Letters B* or in *Physical Review Letters*. When did either of those journals publish a paper on particle detectors or accelerator

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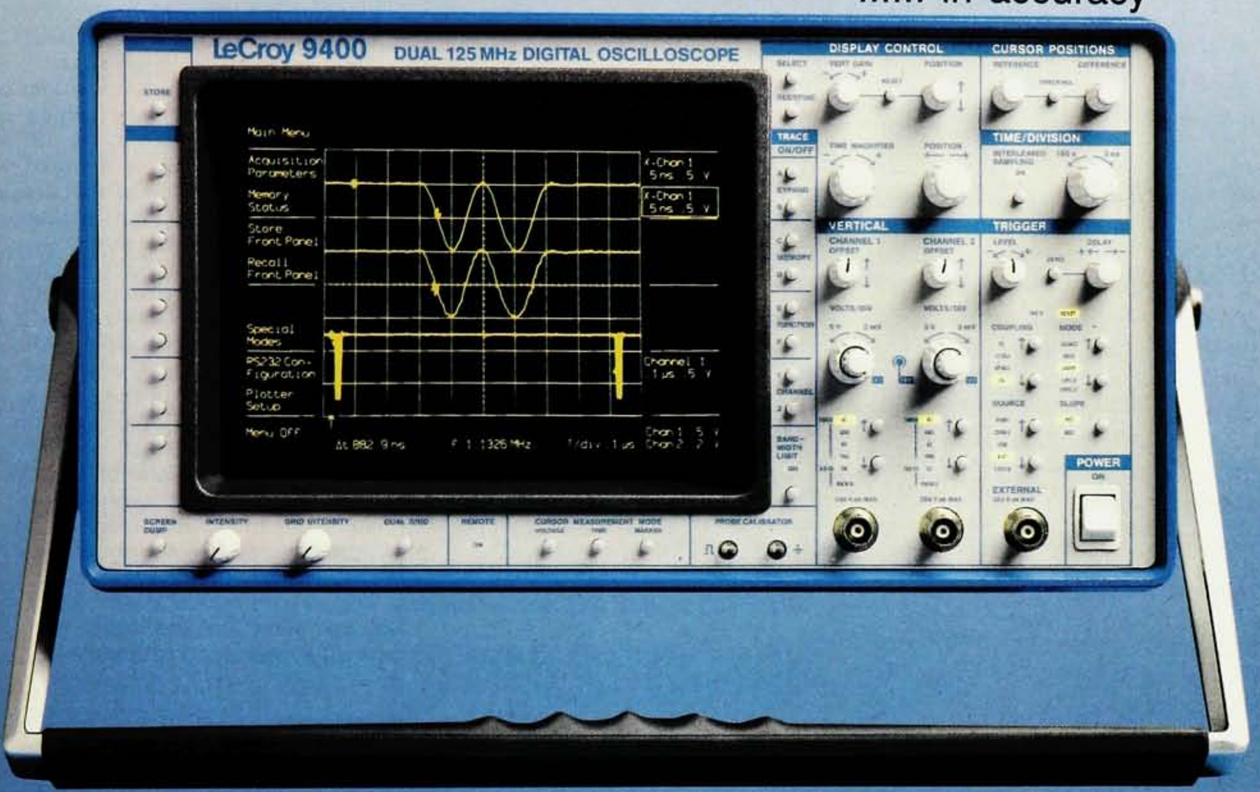


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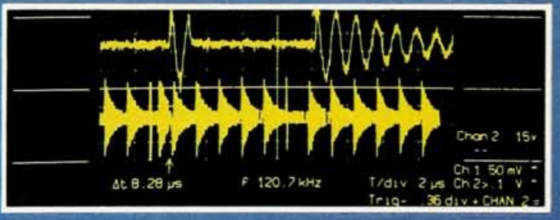


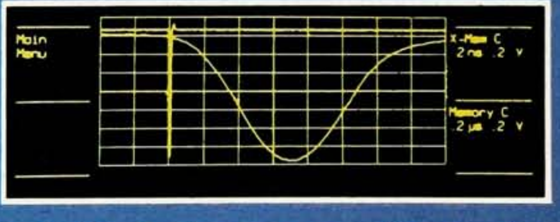


*Top: Dual Zoom and time cursors are applied to measure delay between double pulses with 100 ps resolution and 0.002% precision.*

*Middle: Channel 2 is segmented in 15 partitions of 2,000 words each. Expansion of event 3 appears on top.*

*Below: A 10 ns wide pulse is digitized with 5 GS/s interleaved sampling speed. Expansion to 2 ns/div shows outstanding time and screen resolution.*

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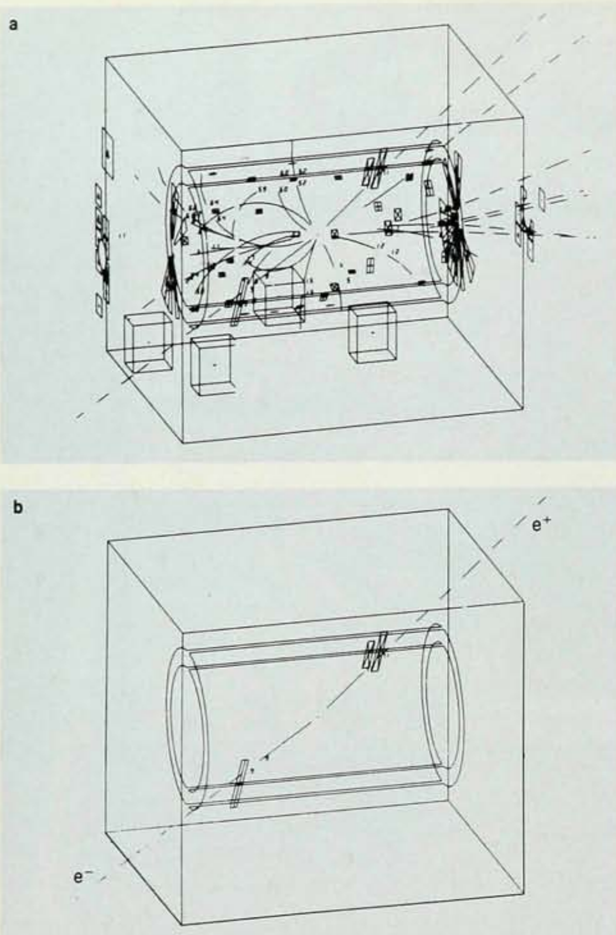


**Event containing a $Z^0$ particle,** found by the UA1 detector at the CERN proton–antiproton collider. The computer drawing in **a** shows the tracks of all the particles produced in the event. In **b** the tracks of the electron and positron produced in the event are shown by themselves. This electron–positron pair comes from the decay of the $Z^0$. Figure 5

research?

We are dependent on governments, large institutions and universities for places to work and for money to do our work. However, our reward and recognition system operates within our own small institutions: our departments, our laboratories, our journals. We can certainly change the reward system.

### Inventions

I have made suggestions for the practice of experimental particle physics that can increase the rate of progress in the field. While these are suggestions for second-order changes, there is the chance for a first-order change. A scientist can make major advances when deeply immersed in research that is supported and rewarded. Immersion in a subject—even obsession with it—is almost always a necessary condition for a major discovery. I would like our community to give full support to experimenters absorbed in accelerator and detector research, to support them on an equal basis with those absorbed in exploring the standard model of particle physics or looking for new particles. This will stimulate inventions in the apparatus of particle physics, inventions that may be necessary to continue the revolution in the field.

I conclude with two quotations. Donald Glaser, deliberately setting out to improve particle detection, invented the bubble chamber, which revolutionized particle physics in the 1950s. In his words:[8]

> I became interested in trying to devise new experimental methods for investigating the physics of elementary particles in 1950, not long after the new "strange particles" had been discovered in cosmic rays. . . . Greatly stimulated by these developments, I began to wonder whether it would be possible somehow to speed up the rate of observing the strange particles and their interactions. . . . There was therefore a great need for a particle detector of high density and large volume—tens to hundreds of liters—in which tracks could be photographed and scanned at a glance, and in which precision measurements of track geometry could be made.

Today we could well use inventions in particle detection as far-reaching as was the bubble chamber in the 1950s.

Similarly, invention is required to build a new generation of ultra-high-energy accelerators. Burton Richter summarizes[9] the requirements:

> These machines will have to have much higher energy than is available today and will have to be built at a cost that the taxpayers of the country (or perhaps the world) will be willing to bear. In the past the scientific community has come up with new techniques of acceleration when the progress of science required it and when the cost of the old techniques, extrapolated to higher energy, became prohibitive.

The more popular and rewarding it is to work on new techniques for acceleration and detection, the sooner we will have the needed inventions.

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