Physics in 1975—new problems and insights

The retiring president of APS assesses future trends in employment and need for government support, and offers his own analysis of the new fundamental particles.

Wolfgang K. H. Panofsky

Following the tradition of past farewell messages, I will comment on the general health of physics; I also want to report, however, on a singular development that occurred during my term in office. This was the discovery of the new particles, an event that has generated a 20-year high in the level of excitement among high-energy physicists.

The physics profession is clearly undergoing dramatic changes in its patterns of activity. While it is still true that a majority of the members of The American Physical Society would characterize themselves as "academic," indicating some combination of research and teaching, it is now evident that this majority must soon become a minority. New opportunities for academic careers for physicists are withering and thousands of physicists have entered other fields, as shown in figure 1. A number of factors are responsible for this incipient change from academic to other professional activities: A decline in the birth rate, coupled with a decline in the percentage of young people seeking higher education, reduces the demand for college teachers; in addition, the economic misfortune of the universities and decreasing federal support further limit the demand for new faculty. There are only a few new permanent teaching positions in the Nation's universities, and there is little hope for change in this situation.

A shift from academe

Details of the transformation through which we are going is now becoming available. Figure 2 shows the growth and decline of the temporary and permanent faculties of the university and college physics departments. This graph, demonstrating the fixed size of academic faculties and the rapid saturation of tenured ranks, tells most of the story. The number of academic positions occupied by younger physicists is declining alarmingly. An examination of the age distribution of the faculties and other factors leads to the conclusion that the number of academic openings each year will be less than 300—and many of these will carry no hope of permanency; in fact, about 200 non-tenured faculty members are leaving academic life annually. Since more than 1200 physicists receive PhD's every year, and there are more than 10 000 graduate students still in the pipeline, this means that fewer than 10% of the current PhD candidates can look forward to lasting academic careers. It is worth noting here that the majority of the doctorates in this country are granted by the twenty most "prestigious" institutions, and that these institutions themselves have very few openings indeed; in fact, most of them are studying alternatives for retrenchment. Physics is not alone here—this situation exists in nearly all areas of academic specialization.

These conclusions are depressing to those who regard an academic career in physics as their exclusive goal. However, physics and physicists can and do contribute much beyond academe. It does not follow, therefore, that people should cease to enter the profession, or that present students of physics should conclude that most of their number will have to leave the field. What these figures mean is that physics is undergoing the same transformation from academic to broader-based professional activity as chemistry did many years ago. In contemplating this change it is important to remember that a combined teaching and research career is not necessarily synonymous with an academic position. Many academic positions offer little opportunity for research of any kind, while much of the work in government-supported laboratories and in industrial research laboratories would meet even the strictest standards for basic as well as applied research.

The clear implication is that a graduate student in physics must be aware of, and prepare for, the broader range of opportunities for the exercise of his professional talents than those that were once provided by the academic world. The delineation of these wider opportunities, and the development of educational programs to prepare physicists to meet them, remain two of the most pressing problems. Therefore, graduate departments should review both their advisory methods and their curricula to give students a less negative attitude toward applied work and a better preparation for pursuing it as a career. The Forum on Physics and Society of the APS recently sponsored a specialized conference dedicated to patterns in graduate education with this reorientation in mind.

Many physicists have left the traditional subfields of physics and many have found rewarding careers in other areas. The flow chart in figure 1 shows how, as of March 1973, about 24 000 PhD's in physics have chosen their occupations. Of these about 6000 doctorate physicists have found their individual ways out of what is conventionally called physics, but fewer than 1000 have left technical fields altogether. All this means that the large majority of physics graduate students have entered scientific–technical careers. These figures describe the patterns of the past, but how large are the future opportunities for PhD physicists for permanent, or at least continuing, positions in non-aca-

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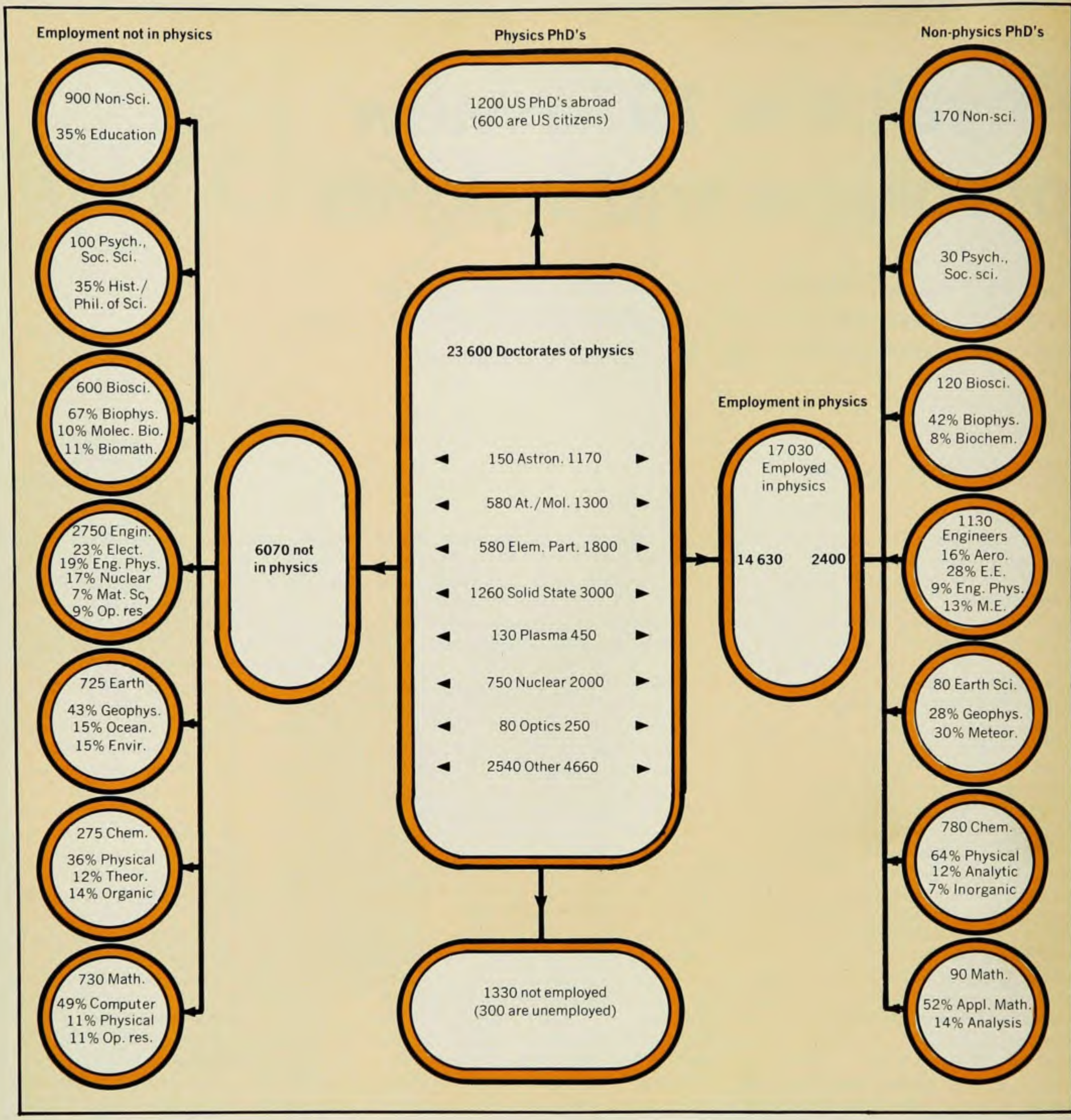


Where do physics PhD's go? This flow chart traces the movement of physicists into physics and nonphysics employment, abroad, and out of employment. The chart, valid to March 1973 with uncertainties of 5–10%, also shows an influx of nonphysics PhD's into physics jobs. It and figure 2 were composed by Lee Grodzins from data supplied by the NRC Commission on Human Resources. Figure 1

demic pursuits? There is probably *no more difficult time* to give meaningful answers to this question *than today.*

Future need for physicists

It is simply unclear at this time whether and when the large number of problems facing society, many of which have high technical content, will result in major new opportunities for professional contributions by physicists. While some may blame science and technology for some of the evils that presently beset society, it is evident that active and thoughtful contributions from these disciplines will be required if we are to provide an adequate quality of life for the population of this planet. There is no question that physicists can and must make contributions to the energy and environmental problems that confront us. The real question is where the responsibility for such work is to reside.

Let us contrast the situation of today with that following World War II. A rapid increase in productive activity in basic research, military development, space exploration and nuclear energy by increasing numbers of scientists and engineers after the war was made possible by an essentially monolithic social pattern: There was but one responsible consumer of such scientific and technical work—the Federal Government; and there was a well-defined set of suppliers—universities, certain institutes and laboratories, and selected industries. The responsibility for dealing with the problems of energy and environment and for developing new resources and managing their utilization is now shared among several elements of society—the Federal Government, local governments, industry and the citizenry itself. The result has been that society so far has not been able to coordinate effectively even a small portion of these efforts. How and how soon will society succeed in finding and sustaining centers of responsibility for attacking these problems? Without an adequate answer to this question it is all but impossible to assess future professional opportunities for physicists.

In meetings of the presidents of scientific societies with Federal officials we have urged more intensified analysis of this problem but so far the results have been meager. The national crystal ball remains cloudy.

The essence of the preceding remarks is that a new demand for physicists—but one outside the academic world—is sure to arise in the future, but specific predictions are non-existent so far. It is not clear that we will have *a sufficient number of highly gifted new physicists to meet these new needs* for physics outside of the academic environment, once additional positions are created. Many graduate schools are already finding exceptional candidates rare, and shortages of good post-doctoral applicants are reported in some areas. There are a number of reasons for this, including disenchantment with science or its technological consequences, and the apparent lack of identifiable, promising job opportunities.

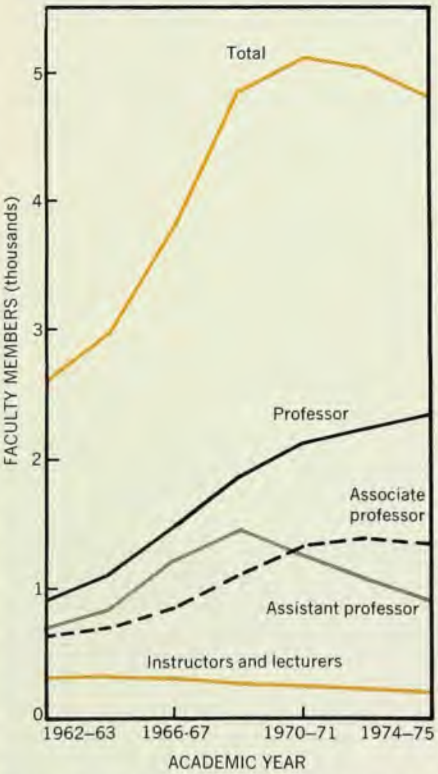
The Federal role in physics

In addition to the near-disappearance of new permanent academic positions for physicists and the lag of the elements of society in marshalling its scientific talent toward the solution of the new problems of energy and ecology, there is a third major element of concern: the pattern of Federal support for basic science. The shift from academic careers on the part of the majority of physicists is a necessity brought about by the birth rate and changing social patterns, by the economy and by changes in the general role of science in our lives. Yet the strength of basic science must be preserved if the continuous chain leading from new basic knowledge to cultural utilization is not to be broken. Since the academic world can no longer provide substantial support for research, and since no single segment of industry can be expected to support research the benefit of which it cannot predictably recapture, the responsibility for this aspect of the health of basic science in general, and physics in particular, falls squarely onto the Federal Government.

Here, however, the recent patterns of funding threatens the health of research—there is not only a question of *levels of support* but of the *continuity and internal consistency* of that support. Effective research is a fragile enterprise. Inflation, the ever-increasing complexity of valid scientific work is increasing its cost. While many deplore this evolution, few would argue that a return to the good old days in which profound results could be obtained with simple apparatus is a realistic alternative.

It is often difficult to assess the true level of support given to *basic* research. Physicists contribute to a continuum of activities ranging from very pure research to the development of specific devices. It is unproductive to argue about the "logical division line" between basic and applied physics, or between science and development—in fact, one of the strengths of the science of physics lies in its basic unity. Yet, depending on how one decides to draw these division lines, one can get a large variety of answers for the cost of supporting physics; ultimately the costs of development and production dwarf those of more fundamental activities.

Let me illustrate the situation as I see it with data pertaining to elementary-particle physics. This is a field that lends itself to relatively unambiguous analysis. Since the situation is very similar in other areas, the pattern of particle physics can be considered as a typical sample of all of basic physics. Funding patterns in particle physics are



Faculty members in PhD-granting physics departments. The breakdown shows declines in the lower, and saturation in the upper, ranks with the total dropping since 1971. (Courtesy of Lee Grodzins.) Figure 2

particularly easy to analyze because 90% of its support comes from a single agency, the Atomic Energy Commission (recently superseded by the Energy Research and Development Administration) and because all costs for building and running accelerators and storage rings are fully carried by the program. In contrast, the real costs of space sciences, for instance, are frequently very hard to define, because one can adopt a variety of practices in assigning the costs of space vehicles that carry scientific probes.

Figure 3 shows the support pattern for elementary-particle physics plotted in real purchasing power; the graph shows the division among operating and equipment funds, and funds dedicated to new construction. It is evident that there has been a continual decline in real support since 1969. At the same time that new facilities such as the Fermi Laboratory near Chicago and SPEAR at Stanford were being built, older installations such as the Cambridge Electron Accelerator and the Princeton–Penn Accelerator were closed out. It is also clear and disturbing that *the fraction of total support dedicated to preparing for the future has been declining* since the peak of construction at Fermi Lab.

We must all recognize that support for physics ultimately depends on the collective judgment of society of the role of science in our culture and its contribution to our well-being. It is a difficult political decision to determine what level of support for science is "right" in terms of expected long- and short-range benefits, or in terms of basic cultural values. Once this decision is made, however, prudence demands that:

- there should be a reasonable expectation that a predictable level will be maintained for some years; and
- there should be a reasonable balance between the development of new facilities and the exploitation of older ones.

These conclusions may appear obvious, but lately the annual budgetary processes of government have operated in a manner entirely contrary to these needs.

Work in physics spans a long time between basic ideas and final results, and between the training of a new generation of physicists and their entering into a creative career; and high-energy physics is no exception. The time between the conceptual design of a new accelerator or storage ring and its impact on physics is a decade. The time to exploit a specific facility (without major upgrading in performance) tends to be one to two decades; after that period the usefulness of a facility tends to decline, while costs to exploit it remain high. These considerations lead one to conclude that about 25% of the total support for elementary-particle physics *should be dedicated to new construction or major upgrading* of facilities, if the field is to remain productive. This was the case from the end of World War II until the beginning of this decade. At the same time it is also true that the level of support defines the *degree of exploitation* of existing facilities; because of the large fixed costs, the pattern of declining funding has had the consequence that existing facilities are seriously underutilized.

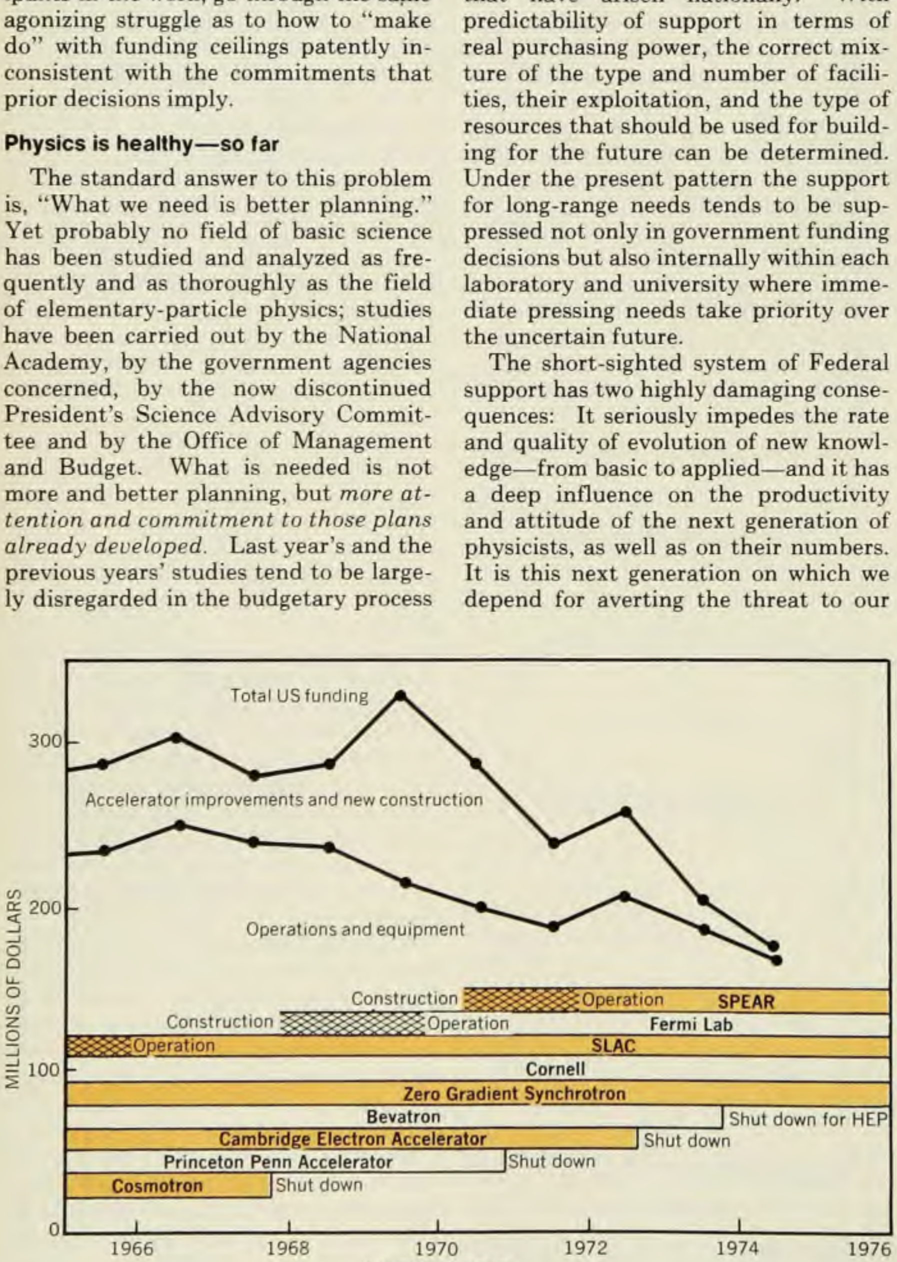
All this means that Federal support of high-energy physics has not followed an internally consistent pattern; furthermore, this remark applies throughout the physical sciences. Year after year officials of the responsible government agencies, together with the participants in the work, go through the same agonizing struggle as to how to "make do" with funding ceilings patently inconsistent with the commitments that prior decisions imply.

Physics is healthy—so far

The standard answer to this problem is, "What we need is better planning." Yet probably no field of basic science has been studied and analyzed as frequently and as thoroughly as the field of elementary-particle physics; studies have been carried out by the National Academy, by the government agencies concerned, by the now discontinued President's Science Advisory Committee and by the Office of Management and Budget. What is needed is not more and better planning, but *more attention and commitment to those plans already developed.* Last year's and the previous years' studies tend to be largely disregarded in the budgetary process of any current year. Thus we are *damaging both the long-range and short-range effectiveness* of the science: *failure to initiate new construction endangers work a decade hence* while at the same time *currently operating installations are seriously underexploited.*

The budgetary processes of the government do not, of course, permit formal long-range commitments. I do believe, however, that a meaningful pattern of "guidelines" could be adopted; their role would be that the burden of proof in deviating from such guidelines in future years would fall on those who were to make the change: An increase would require demonstration of some new and unusual physical principle or opportunity, and a decrease would have to rest on unforeseeable contingencies that have arisen nationally. With predictability of support in terms of real purchasing power, the correct mixture of the type and number of facilities, their exploitation, and the type of resources that should be used for building for the future can be determined. Under the present pattern the support for long-range needs tends to be suppressed not only in government funding decisions but also internally within each laboratory and university where immediate pressing needs take priority over the uncertain future.

The short-sighted system of Federal support has two highly damaging consequences: It seriously impedes the rate and quality of evolution of new knowledge—from basic to applied—and it has a deep influence on the productivity and attitude of the next generation of physicists, as well as on their numbers. It is this next generation on which we depend for averting the threat to our future brought about by the limited availability of ecologically safe energy sources and raw materials, and for devising methods for the world's population to conserve these resources. The current transition in the role (but not the nature) of physics and the uncertainties of that transition *imply a serious threat* to the future health of the science: We can ill afford *a major interruption in the evolution of basic understanding,* nor can we permit *the discouragement of a whole generation of physicists.*



The funding of high-energy physics in the United States, in FY 1975 dollars. Declines in allocations led to the shutdown of several facilities in recent years, as shown. Figure 3

The foregoing remarks are primarily a concern and a warning for the future, not an assessment of the past. *Today physics as a profession and as a source of new and basic insights remains healthy.* The average time interval between important new discoveries that have profoundly changed our perception of the nature of the physical world has not been increasing. I would take particular exception with those who claim that, while the technical complexity of physics experimentation has been increasing, the productivity in real results has gone down. Indeed, I should like to disprove that assertion by turning to a discussion of the almost explosive evolution of new information in the field of elementary-particle physics that has occurred during the last year or two.

Leptons probe hadron structure

One of the most powerful and successful methods of exploring the structure of the strongly interacting particles, the hadrons, to emerge in recent times has been the use of lepton beams. These lepton probes of hadronic structure can be charged (as in the case of electrons and muons) or uncharged (neutrinos). This approach uses a known or partially known force to probe *unknown* structures.

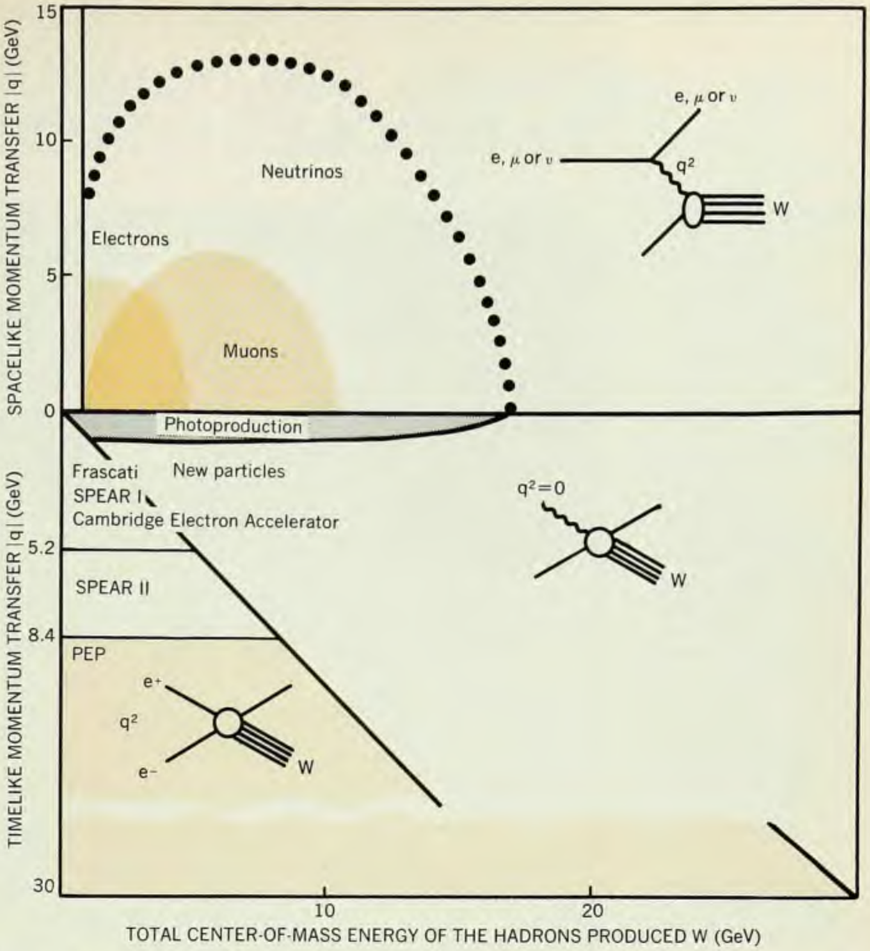
Quantum electrodynamics stands unique among all the formulations of physical laws in that its validity in describing the electromagnetic interaction has been verified over intervals ranging from several Earth radii to near 10^{-15} cm. In the case of the charged leptons, this precise knowledge allows us to investigate the electromagnetic charge structure of the hadrons.

For the neutral leptons, the neutrinos, the existence of a phenomenologically successful low-energy description of the weak interactions allows us to probe the weak charge structure of the hadrons. Conversely, these experiments can be used to test the validity of quantum electrodynamics and the behavior of the weak interactions at higher energies.

The interactions of leptons with hadrons can now be studied over a wide range of kinematic variables. The regions presently accessible are shown in figure 4. The electromagnetic interaction of charged leptons with hadrons has been most extensively investigated in electron-scattering experiments at SLAC and in muon-scattering experiments at Fermi Lab. The momentum transfer from the lepton to the hadron is spacelike (corresponding, in lowest-order electrodynamics, to a photon with negative mass-squared). The neutrino interactions also probe the structure of hadrons in the spacelike region, but do so through the weak force.

More recently, evolving from the lower-energy installations at Stanford, Orsay and Novosibirsk, it has become possible at SLAC, CEA, DESY and Frascati to extend our electromagnetic probes of hadrons into the timelike region, through the successful development of colliding electrons and positrons. The resulting annihilation of an electron and a positron produces, to the lowest order in the electrodynamic interaction, a single virtual photon with positive mass-squared.

This virtual object then materializes into those states, mostly hadrons, that are allowed because the well-defined quantum numbers (odd charge conjugation, spin one, odd parity) of the virtual photon are conserved. At relatively low annihilation energies, this one-photon assumption is amply justified by experiment, and hadron production is dominated by the known vector mesons. The earlier experiments at Novosibirsk, Frascati and Orsay have given valuable insight into the widths and decays of the ρ, ω and ϕ mesons and have been used to study ρ–ω interference. At higher energies, the annihilation cross section appeared to smooth out into a continuum of hadron states, and experiments at the Cambridge Electron Accelerator measured this apparently smooth cross section at two points, at center-of-mass energies of 4 and 5 GeV. Subsequent data at SPEAR added a large number of additional measurements in this same energy region.



Lepton–hadron interactions have been studied in both spacelike and timelike regions. Electrons and muons scatter from hadrons electromagnetically, neutrinos through the weak force. The timelike region is explored with colliding electron–positron beams. Figure 4

The annihilation cross sections

This was the experimental situation until late last year. Before I go into what occurred at that time, it is worthwhile to examine the basis of our theoretical understanding of the annihilation process prior to the new discoveries. An early suggestion, which requires the validity of the one-photon mechanism, was that, in converting into hadrons, the photon coupled directly and electromagnetically to the postulated pointlike constituents of hadrons. At collision energies exceeding the value at which coherent resonant states can be found, the total hadronic cross section becomes proportional to the incoherent sum over such pointlike interactions and is proportional to the sum of the squares of the electromagnetic charges of the constituents. It is customary to compare this hadron-production process with the reaction

$$e^+ + e^- \rightarrow \mu^+ + \mu^-$$

Since this latter process presumably involves purely electrodynamic forces among point-like particles its cross section is known; one uses the ratio R of the hadronic cross section to that of this process.

It was the experimental behavior of R as a function of energy that first drew serious attention. It appeared to rise linearly to a value of perhaps five at the higher energy available at the Cambridge Electron Accelerator and SPEAR. This was not in good agreement with the theoretical expectation that it should be approximately constant and relatively small. This expectation was based on the simple fractionally-charged quark model, in which R should have the constant value of two-thirds. While the simple quark model had been spectacularly successful in explaining the systematics of hadron spectroscopy, its apparent failure in predicting R was only another in the list of its defects. Indeed, these other difficulties gave some clue as to how to modify the model and perhaps eliminate such problems, including that of failing to predict R.

The first of these difficulties was theoretical in nature. According to the general theorem concerning the connection between spin and statistics, the elemental fermion quarks should be in a fully *antisymmetric* ground state. However, the quark model gives successful predictions concerning the spectroscopy of hadrons only if *symmetric* ground states are used. Several theoretical suggestions for removing this embarrassment were made, ranging from the invention of a special kind of statistics for quarks in bound hadron systems to the assignment of a new quantum number, called "color." This number enlarges the family of quarks from one to three triplets, each with a different color. Thus construction of hadrons from colored quarks removes the apparent violation of the exclusion principle, because states antisymmetric in the color quantum number can be constructed. There are at least two variants of this model: one with the original fractional charged quarks of Murray Gell-Mann and Kazuhiko Nishijima, and a second scheme proposed by Moo-Young Han and Yoichiro Nambu

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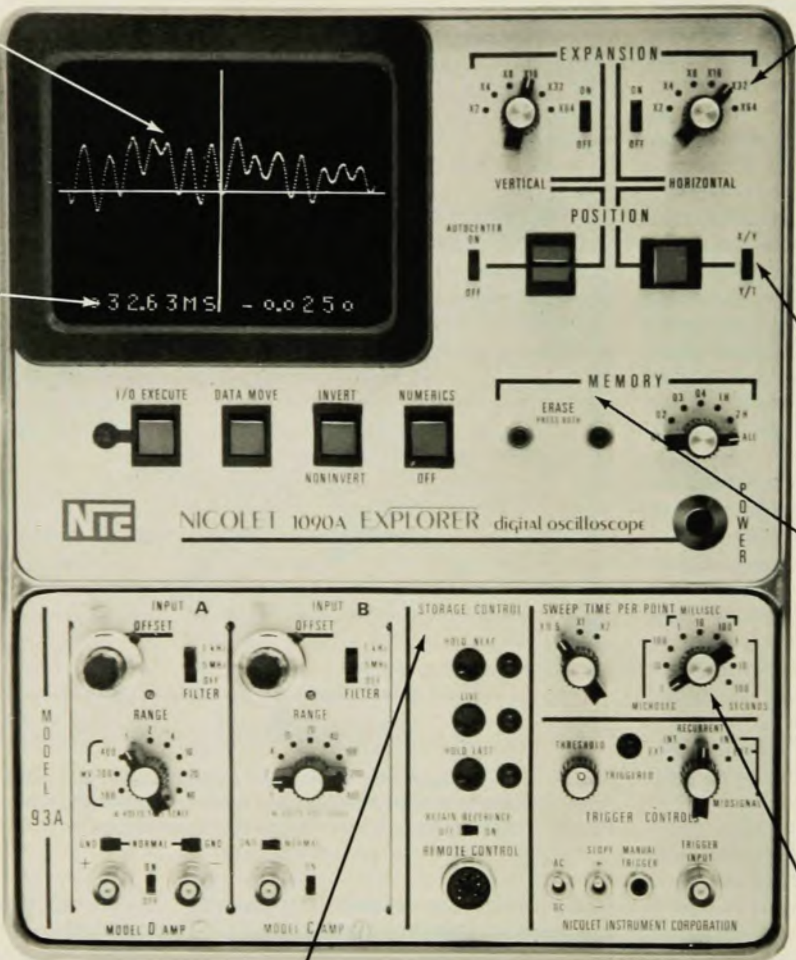

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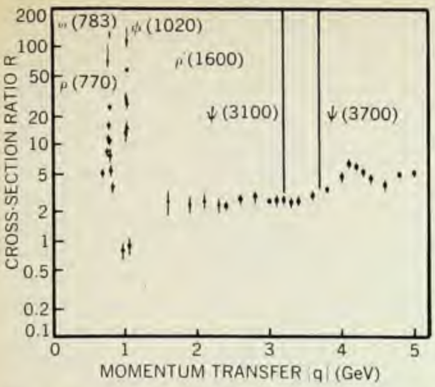

The ratio of cross sections for hadron production from e^+–e^- annihilation to that for muon-pair production for the same process versus total CM energy. Shown are vector mesons that enhance this ratio; the new particles (too narrow to display width here), and 4.1-GeV enhancement. Figure 5

with integrally charged quarks. The importance of this suggestion to our discussion is evident because an increase in the number of quarks involved in the annihilation process leads to an increase in the value of the ratio R. In the model of Gell-Mann and Nishijima, R is expected to be two, while in that of Han and Nambu, R would be four.

A second defect in which the quark model apparently contradicted experiment is a more detailed one: Experimentally there appears to be a selection rule that forbids the weak interaction from coupling states differing in strangeness but not in charge. You may remember that what is known as *neutral* currents, observed first at CERN and then at Fermi Lab and Argonne, are processes in which *neither* charge nor strangeness change under weak interactions. Since the simple quark model involves hadrons with quarks carrying the quantum number of strangeness, it is difficult to forbid strangeness-changing processes that leave the charge unchanged. The additional selection rule required to prohibit or at least impede strangeness-changing neutral currents led Sheldon Glashow and James Bjorken to introduce a fourth quark, which carries a new quantum number that they called "charm." This, added to the original quark model in a manner parallel to the strange-quark model, is contrived to cancel the strangeness-changing neutral transitions. The predicted value of R of the "charming and colorful" model is $3\frac{1}{3}$.

These quark additions were invented in response to the basic difficulties indicated; no further or more direct evidence has as yet been obtained. We are of course facing the fundamental difficulty that all quark hunts—to observe such objects in their free states, either produced at accelerators or occurring in nature—have been unsuccessful. Therefore quarks either do not exist, or they are too heavy, or their dynamics of binding prevents their existence in a free state.

In addition to this rise of the observed hadron-production ratio to uncomfortably large values, detailed examination of the final hadron products produced in electron–positron annihilation also exhibits distributions and ratios among charged and neutral fragments that are difficult to reconcile with a simple electromagnetic-production mechanism. In fact, a superficially striking similarity emerged between the distributions of secondary fragments produced in electron–positron annihilation on the one hand, and ultrahigh-energy proton–proton collisions on the other. This appears surprising at first sight because proton–proton collisions involve states of many angular momenta and are dominated by the strong interaction.

The new particle

In November a new series of striking events added to these still unresolved puzzles. At that time the discovery was announced by the MIT–BNL group of collaborators working at Brookhaven, and the SLAC–LBL collaboration working with the storage ring at SLAC, that a very sharp peak was observed corresponding to an electron–positron state of effective mass 3.1 GeV.

The discovery of these heavy objects sent shock waves through the high-energy physics community. If the new particles are hadrons—an assumption to be treated with caution—the existence of peaks this narrow appears to indicate that a selection rule is acting to prevent the decay of these objects into lighter hadronic fragments on a time scale of 10^{-23} seconds. To further complicate things, a second peak was soon discovered at SPEAR at a mass of 3.7 GeV. Figure 5 shows the total composite results on the low-energy and high-energy annihilation cross section into hadrons.

Let us enumerate the experimental facts observed in connection with these peaks.

- The 3.1-GeV object decays not only into hadrons but also, with a rate 16 times lower, into electron–positron pairs and an approximately equal number of muon pairs.
- The second object, near 3.7 GeV, is observed only in the $e^+ + e^-$ annihilation process and not in proton–nucleon collisions at Brookhaven energies, within the sensitivity of the apparatus used by the MIT-BNL group. It has also been observed in direct photoproduction at SLAC.
- A cascade transition involving the decay of roughly one-third of the 3.7-GeV objects to the 3.1-GeV object is observed to proceed through emission of two charged pions. Transitions accompanied by the emission of one or more photons do not occur with an observable cross section.
- If the production of these objects proceeds via electrodynamic forces rather than by some form of direct interaction, then the observed peak is presumably dominated by one-photon exchange. Under the assumption of electrodynamic production the spin–parity of these objects is therefore most likely to be 1^-, corresponding to a vector particle.
- A direct determination of the spin of these objects has been made by observing the interference between the muon-decay channel of the 3.1-GeV state and the muon pairs produced in an electron–positron collision by direct electromagnetic production. Observations are in agreement with the 1^- spin assignment.
- The decay width of these objects can be inferred theoretically but has not been measured directly. In the Brookhaven experiment the observed width is controlled by the experimental resolution of the apparatus; in the SPEAR electron–positron annihilation experiments the width is controlled by the energy inhomogeneity of the annihilating beams, which is controlled in turn by the quantum fluctuations of the emitted synchrotron light. With known energy width of the ring, and after radiative corrections are applied, the observed line shape is consistent with zero width.
- Detailed balance allows us to connect the annihilation cross section for production of the new objects to their decay rate into lepton pairs. Conservation of probability and an experimental separation of leptonic final states from hadronic ones then allows us to extract the partial width for decay of the new objects into lepton pairs. This partial width for the 3.1-GeV object is about 6 keV. Since the branching ratio into leptons is about $\frac{1}{16}$, a total width of about 70 kilovolts may be inferred. This is an extremely small width for such a massive state.
- Despite early reports to the contrary there is *no* front–back charge asymmetry for final-state leptons in the annihilation process. An asymmetry would result if the couplings of the new objects to leptons were violating parity.
- In a search for other narrow objects, annihilation cross section has been scanned up to a center-of-mass energy of 7 GeV. No other *sharp* structures have been found. However, the total annihilation cross section does exhibit a broad peak near 4.1 GeV. Observation of this structure is not in statistical disagreement with the earlier, less precise analyses of the measurements in this region.
- Apart from the cascade process in which the 3.7-GeV object decays into that at 3.1 GeV, there is no spectacular variation in the hadron composition of

the final state as a function of energy. As one passes through the peaks, the kaon fraction decreases somewhat.

▸ The question of whether these objects may be photoproduced is of particular interest. If they are vector mesons, like the ρ, ω and ϕ, then they will mix with the photon and be produced diffractively by photon beams on matter. Photoproduction has been measured at Fermi Lab and SLAC. The cross sections are rising from threshold to 20 GeV and then move slowly to Fermi Lab energies; the values are uncomfortably small for the simple description given here.

▸ The final-state hadron distributions are consistent with isotropic production. This statement is true even for the decay products of the 3.7-GeV object, including the pions from the cascade transition. This is consistent with the conclusion that the spins of the 3.1- and 3.7-GeV objects are the same.

▸ The angular distribution of final-state leptons is consistent with the decay of spin-1 objects.

▸ The decay pattern of the 3.1-GeV object appears to conserve strangeness. This and the photoproduction results above indicate that these particles are almost certainly hadrons.

▸ Final states leading to an even number of pions appear to be absent from the decay of the 3.1-GeV object. This fact and detailed fits of specific events indicate that the G-parity of this particle is odd.

Interpretations

Let me now turn to the question, What is the meaning of the new data? The honest answer is that there is no one interpretation that is even remotely convincing. Many theoretical suggestions are current, none of which necessarily correspond to what will eventually become an accepted interpretation. Let me list some of these suggestions:

1. Are the new particles the intermediate vector boson projected to be the carrier of the weak interaction many years ago?

2. Are the new particles the bound states of the "charmed" quark and its antiparticle, the conjectured existence of which I discussed above?

3. Could the new particles be quark–antiquark bound states in which the quarks involved have different colors?

4. A combination of (2) and (3).

Each one of these conjectures has its positive points but also its defects. One can only state at this time that the "charmed" quark interpretation is simplest and perhaps the least defective. It is likely that the correct answer will be the unstated item 5: "something else." These theoretical discussions are already extensive and I will highlight only some of the arguments relating to the above conjectures.

The best that may be said of the hypothesis that the new particles are weak intermediate vector bosons is that the narrow decay width then has a simple explanation, since the weak bosons would lack strong interactions. However, gauge theories that unify the weak and electromagnetic interactions require a specific mass (much larger than 3.1 GeV!) for the neutral member of the "weak" boson family. The neutrino experiments from Fermi Lab also require a charged boson with mass in excess of 10 GeV. Moreover, unequal masses for the neutral and charged members of this family are difficult to reconcile with the apparently slow variation of the ratio of "neutral" to "charged" currents from CERN to Fermi Lab energies. Beyond this, it would be difficult to explain why there are two states or why the higher mass states decay into the lower with the properties known to be true of this process. The photoproduction cross section would also be difficult to explain.

Are they hadrons?

When we turn to the more conservative hadronic explanations, our largest problem is with the small observed width. Hadrons usually decay into other hadrons with a width of order 100 MeV and they decay electromagnetically with a width of about 1 MeV. An interesting exception, to which we might turn for guidance, is the vector meson ϕ, which decays with a total width of about 4 MeV into other hadrons, primarily into a kaon–antikaon pair. The limited phase space for the last reaction is responsible for the narrow width, so the real problem is to suppress all those decays into lighter hadrons that would have larger phase space and give a greater width. For instance, the lack of decay into three pions is difficult to explain. In the quark model, where the ϕ is a quark–antiquark bound state of strange quarks, it is necessary to invent a selection rule, Zweig's rule, which says that quarks inside a given hadron cannot annihilate each other. This somewhat mysterious rule then prohibits the strange quarks inside the ϕ from getting rid of their strangeness by mutual annihilation. The result is that a particle containing only *strange* quarks decays strongly into *strange* particles and is otherwise inhibited from decaying. If the strange particles into which it might decay are sufficiently massive, the width will be small.

We may argue by analogy with this somewhat *ad hoc* explanation of the ϕ decay, that *if* the new particles are made of quarks carrying some new quantum number, and *if* the hadronic states analogous to the kaons that might carry this new quantum number are high enough in mass, then we might be able to inhibit the decays of the new particles. The earlier inventions of color and charm are both candidates for this new quantum number, but in general one must work rather hard to get a small enough width with either choice. This mechanism also has the inescapable consequence that when the new particle with charm or color inside decays, it must decay into states that contain some of these new additions to the quark family an appreciable fraction of the time. As a result, the experimental predictions as to the decay mechanisms of the new objects made by either the charm or color explanation differ substantially. In particular, the colored-quark explanation would predict that a large fraction of the decay modes of the new objects into hadrons would be accompanied by gamma rays and that decays purely into hadrons are forbidden. However, this prediction is difficult to test experimentally, because the photons can be of very low energy. The reaction that might distinguish charm *versus* color is the cascade transition from the 3.7-GeV to the 3.1-GeV object. The existence of this two-pion transition is difficult to explain in the color scheme but very natural in the charm explanation.

Both of these schemes would predict that many other new objects, or even whole spectroscopies, should occur in nature; in particular, the explanation of the new particles as charmed objects predicts that a charged counterpart of the discovered neutral objects should be observed by its characteristic decays. Hunts for such objects are going on in many laboratories but have thus far been unsuccessful; all of these undertakings involve considerable experimental difficulty. An appealing game that is, however, quite unsupported by further experimental facts to date, is to interpret the 3.1- and 3.7-GeV states as radial excitations of a quark–antiquark combination; that is, as a beginning of a new spectroscopy analogous to the hydrogen atom. However nostalgic this interpretation may be, it is a large extrapolation from only two lines in this spectrum!

In conclusion one can only state that these new phenomena are clearly opening a whole new chapter in investigation and insight into the structure and the nature of elementary particles. Although public understanding has unfortunately not kept pace with these developments, the excitement of the new discoveries has been shared by many, and has traveled far beyond the particle-physics community. Elementary-particle physics is still a wide-open field of inquiry to those interested in the fundamental basis of our existence.

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This article is a slightly condensed version of the address of the retiring President of The American Physical Society, presented 30 January in Anaheim, California. □