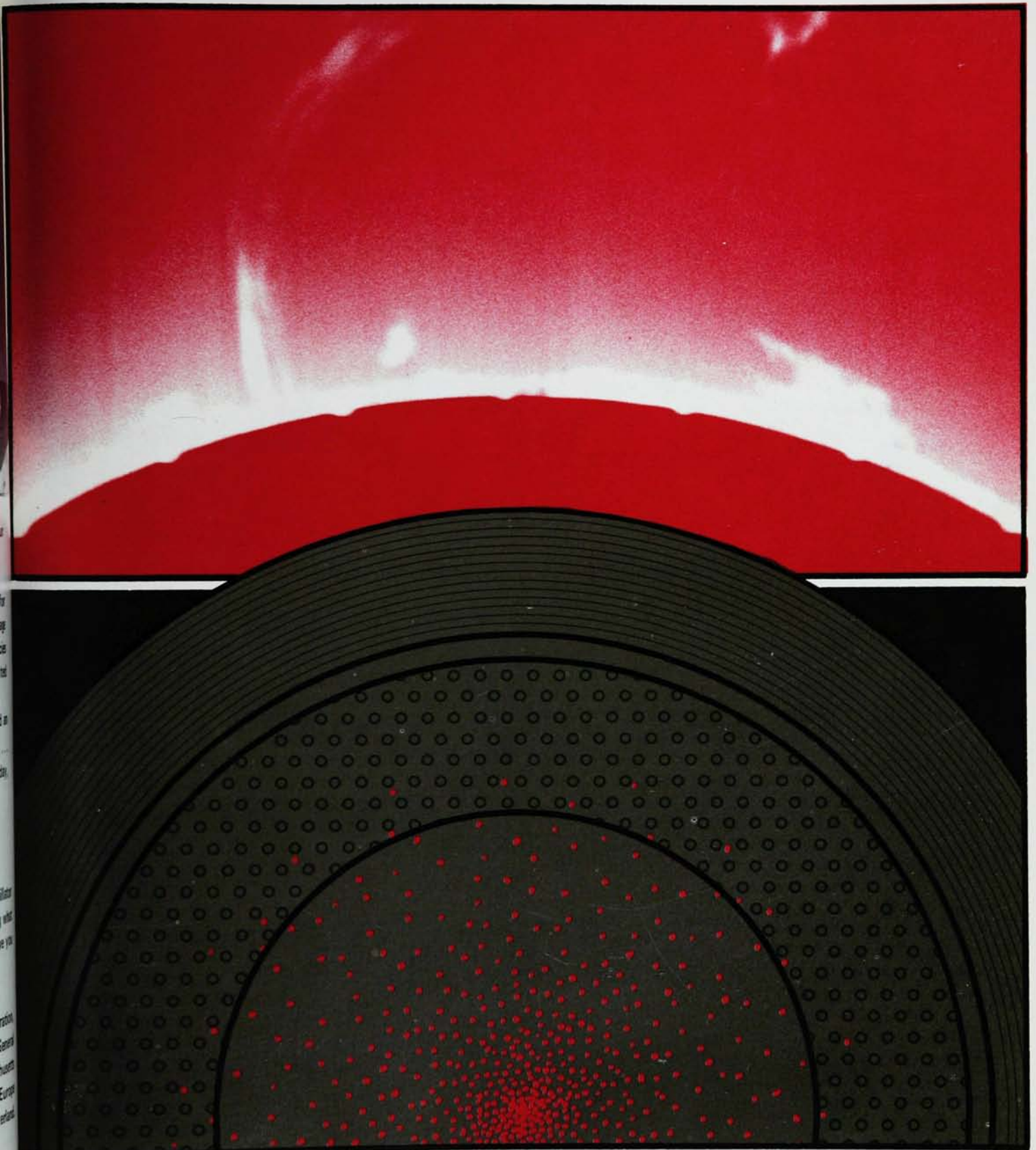


# PHYSICS TODAY

PLASMAS: PAGE 34



DECEMBER 1969

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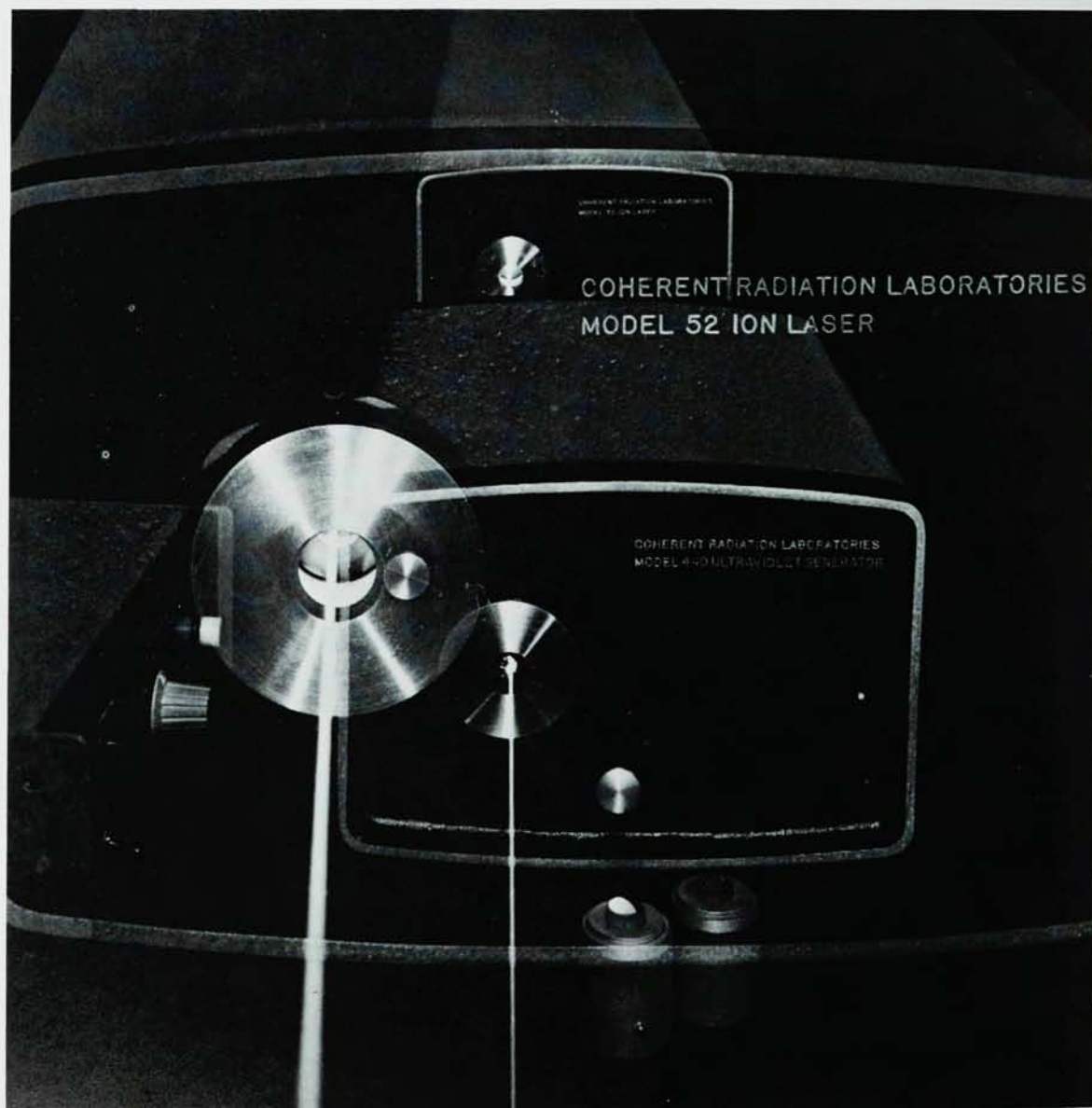
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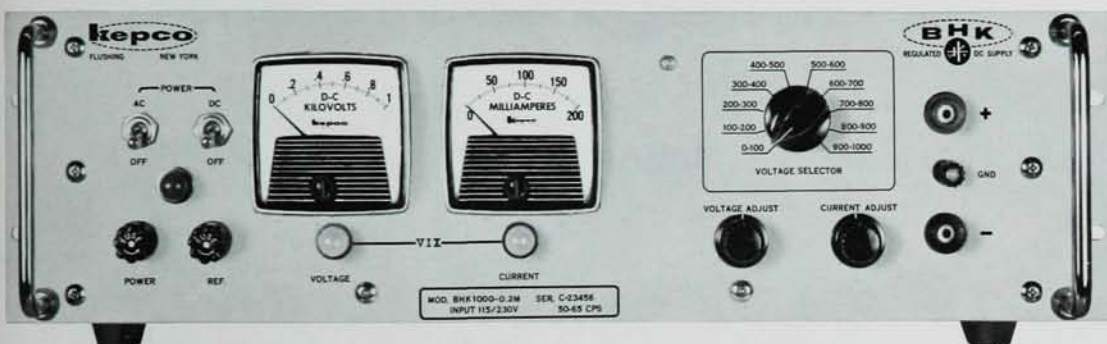




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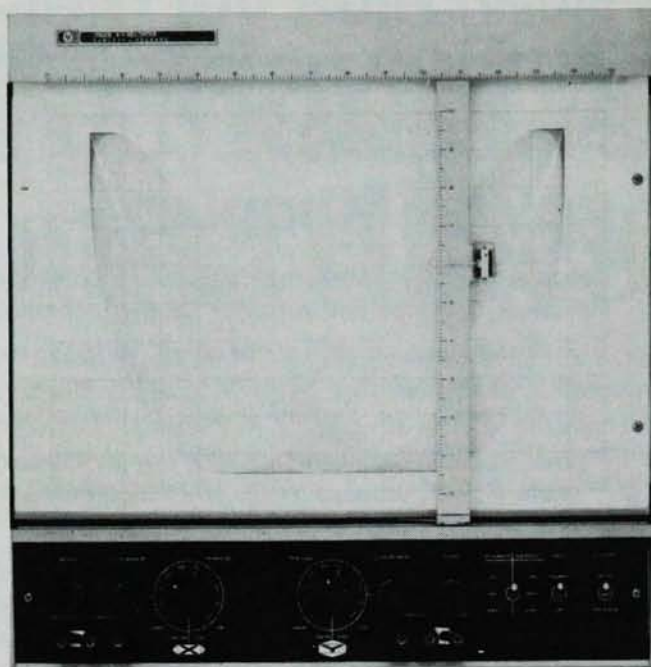
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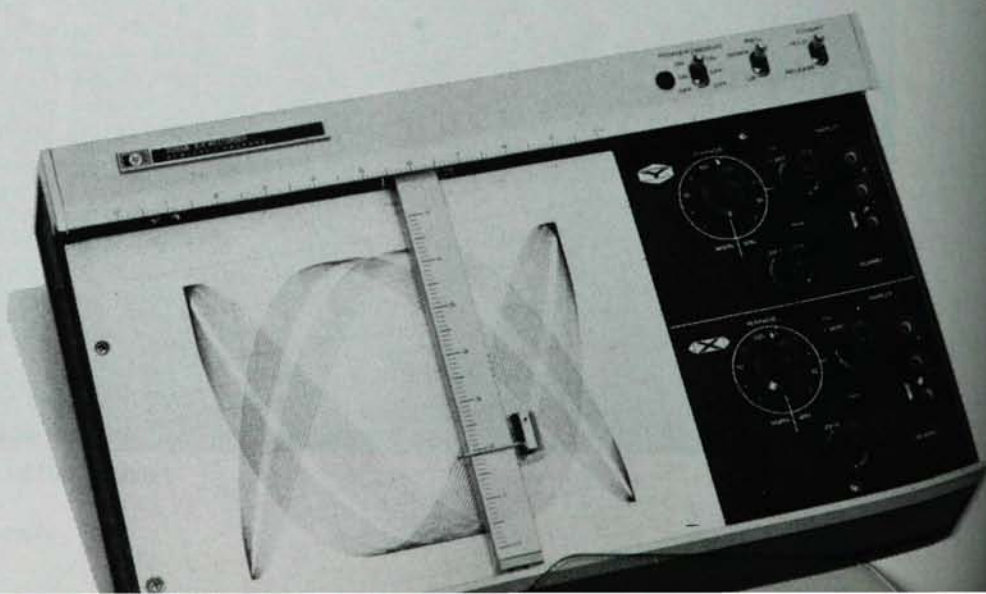
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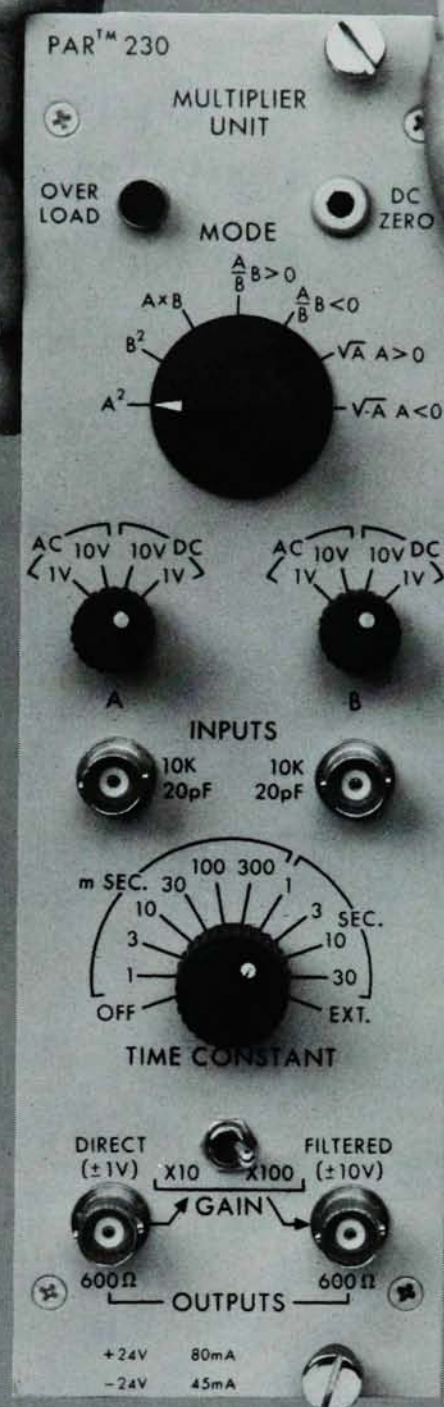
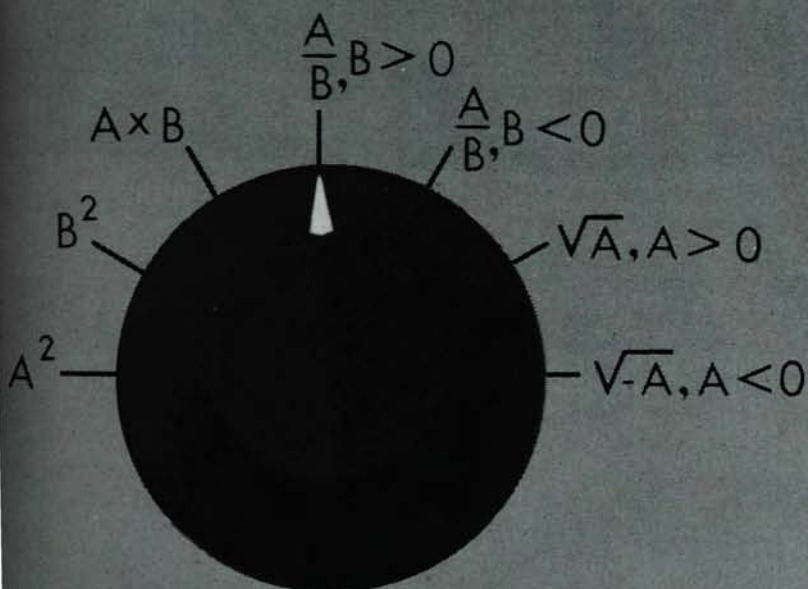
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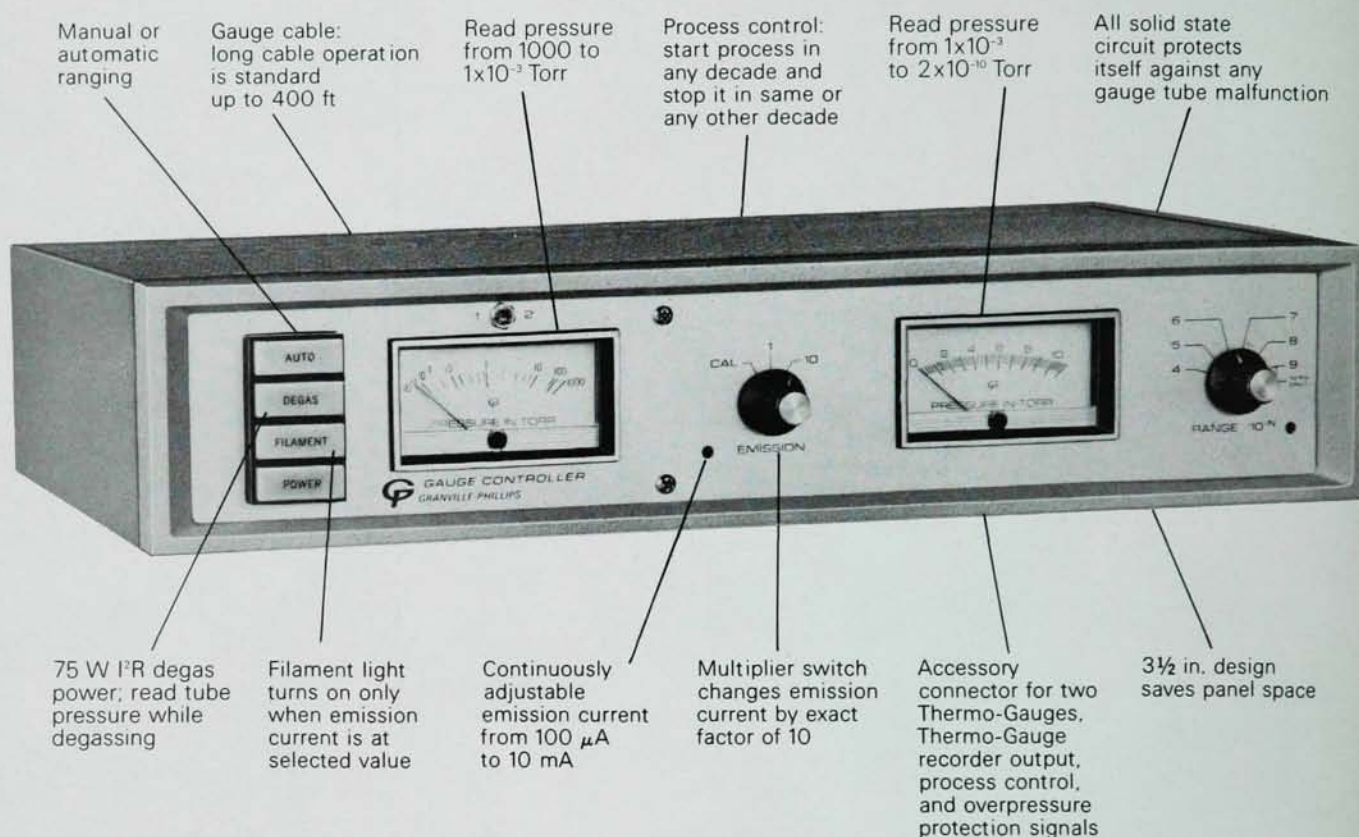
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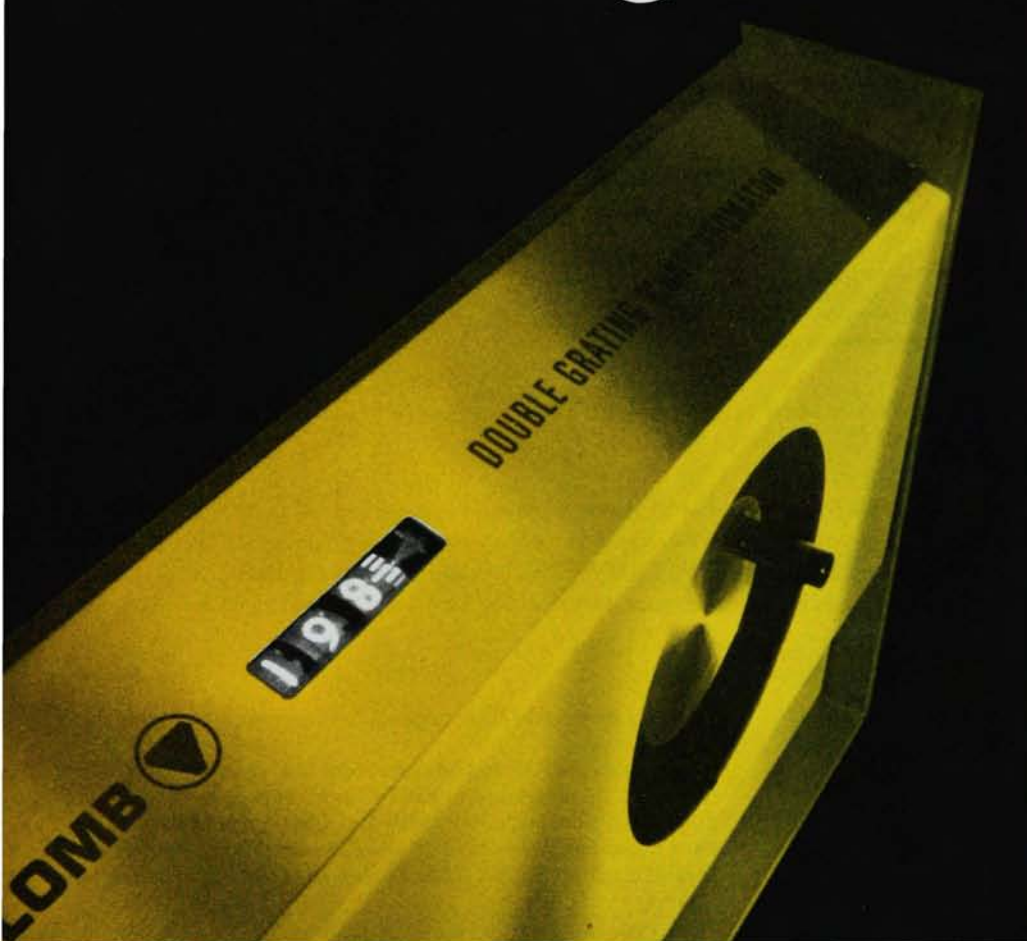
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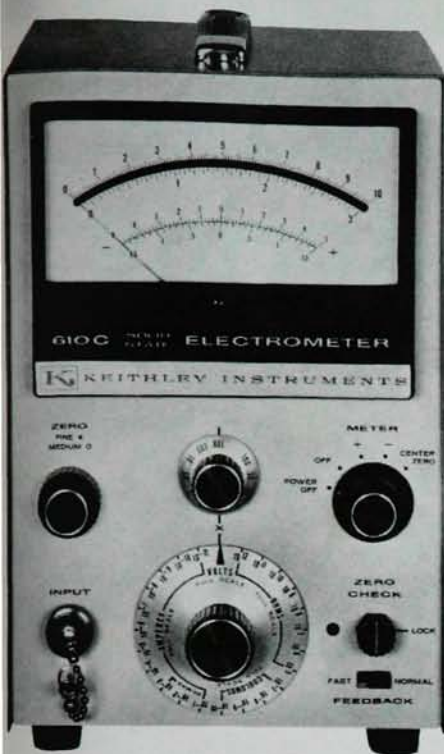
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## LETTERS

### Losses to society

Although I have always admired your editorial comment for its breadth of vision and indeed its humanity, I must take exception to your remarks in the June issue concerning the current unemployment among physics PhDs. I can understand your irritation at suggestions made by some of these people that society owes them a living, but I doubt if these complaints are typical, and in any case, even if the unemployed were to misdirect their criticism, this misdirection would not mean that the blame for their unfortunate predicament was theirs alone. Thus, although I agree that society at large has more pressing concerns, I do suggest that a large measure of responsibility falls on the shoulders of the academic physics community.

Briefly, I claim that the graduate schools have no business turning out more PhDs than can reasonably be expected to find academic or other research employment (allowing, naturally, for the usual number of dropouts and *voluntary* transfers to other fields). Of course new PhDs "are equipped to do other jobs" of value to society. The point is that they were equipped for this five or so years ago and would have taken these jobs at that time if that is what they had wanted. As it is, feeling understandably frustrated, they will presumably give less of themselves in these jobs now than they would have done originally. There are two other losses which society suffers through the indiscriminating admissions policy of graduate schools: the service of these students during the period in which they were working for their (useless) PhDs, and the tax money spent in producing an unemployable elite.

And together with the loss to society must be considered the great personal distress of the individuals concerned. It is not simply a question of winding up with \$8 000 a year rather than \$14 000. Especially towards the end of one's PhD work a fairly intimate collaboration develops between student and research director. As the two sweat out their problems together, discuss them with other physicists, share the same moments of frustration and

satisfaction and travel together to physics meetings, the student is led by his professor into the circle to which he aspires, namely, the world of physics research. At any point the association can be terminated, should the student prove inadequate, but if awarding a PhD means anything at all, it must surely be regarded as a certificate of admission to this circle, at least to the point of a few probationary years. Although there can be no question of this admission constituting a formal contract, the new PhD who finds himself rejected from this community at the very moment of acceptance must surely feel cruelly betrayed. Obtaining his degree has required not only several years of financially unrewarding hard work but also a considerable emotional dedication to physics in general and his field in particular.

Confronted with this situation, your "oldtime answer . . . physics is tough . . . . If you want to work with us despite the drawbacks we will let you" is a cynical irrelevancy. For the point is that however good the student is, we will only let him work with us up to the completion of his PhD. After that he can take his chance on the market.

Probably my picture of the personal relationships involved between student and professor is somewhat idealized, but only insofar as graduate students have come to be regarded as slave labor, engaged only to serve the ambitions of expanding departments and the careers of individual professors. Therein, of course, lies the rub. As long as there were jobs, no conflict of interest arose and everyone was happy, but in this new situation the only honest thing to do is to reduce the output of PhDs, either by reducing the intake of graduate students or by raising the requirements. (The latter solution could take the form of demanding a certain measure of competence and experience in teaching, which would be suitably remunerated.) If whatever solution is adopted involves a cutback in research so be it: The loss is not likely to be irreparable.

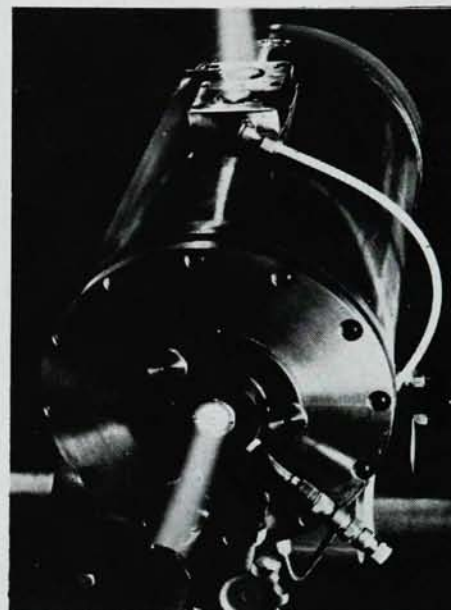
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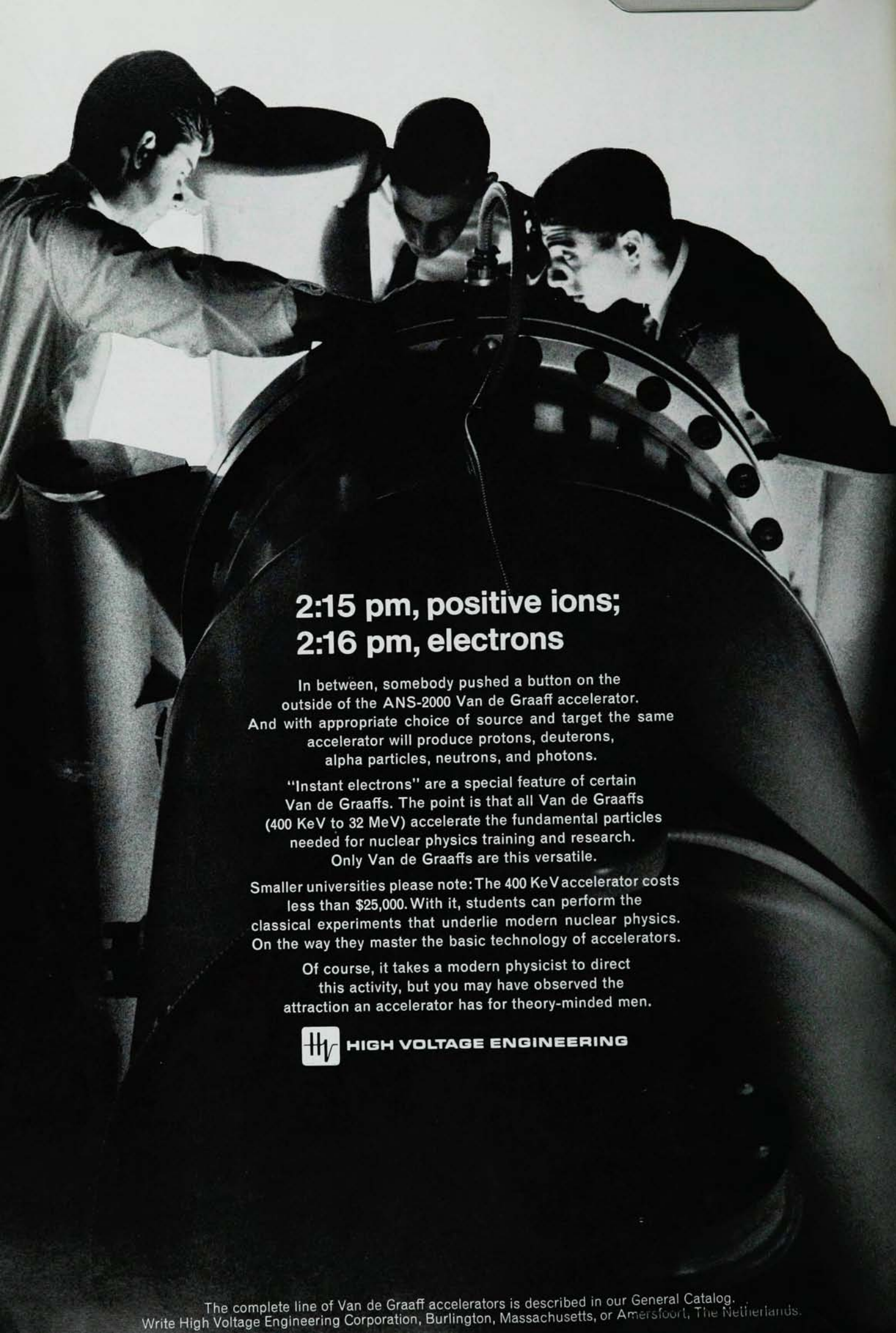
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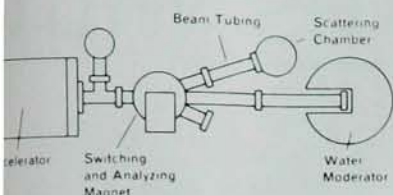
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## LETTERS

employ itself, I would certainly not hold this to be true for the bachelor or master degrees. On the contrary, the programs for these courses have been far too strongly oriented towards the student who will eventually go into research. Physics is an excellent training for the mind, and it is one of the scandals of our time that men in public life are, for the most part, scientifically illiterate. Physics departments have been sadly derelict in failing to develop rigorous undergraduate programs for those who will eventually do something else: economics, law, sociology, politics, etc. But once a man goes as far as the PhD in physics, it must be assumed that this is what he wants to do.

J. MICHAEL PEARSON  
*Université de Montréal*

### Unethical promise of jobs

William Silvert writes of (1) an "employment crisis," and (2) of a crisis . . . far deeper and more bitter than a matter of jobs (PHYSICS TODAY, August, page 9).

Regarding the employment crisis, it is hardly reasonable to expect any course of study to lead surely to well paid permanent employment. No institution can properly hold out such a promise to its students unless it has the power to enforce it. Lacking this power, such a promise is unethical. Unemployment is common among actors, playwrights, musicians, poets and composers, but they did not expect their studies to guarantee jobs. They studied for the love of the subject.

Beginning about 1950, many public statements appeared that alleged a "shortage" of scientific personnel—at first, mainly of engineers. This publicity began at about the time that the defense contracting business started to grow rapidly, on a cost-plus-fixed-fee basis. One writer suggested that such contractors made profits on the mere buying and selling of technical labor, the customer being the government. This has not been proved and is not provable, but it is a fair hypothesis. The allegations of a "shortage" were shown to be poorly justified, at best, as long ago as 1957, when the National Bureau of Economic Research published its book-length study, *The Demand and Supply of Scientific Personnel*. It is surprising that any highly skilled group, such as physicists,

should still believe official statements from any source as to the demand for its services, instead of drawing its conclusions independently from factual sources.

Silvert's second remark suggests deep and widespread disillusionment. But it fails to advance reasons for this second "crisis." In so failing, it becomes unscientific. This crisis clearly exists, but it is a symptom. The disease seems to be hidden. This disease is probably rooted in practices in industry and politics. Nobody seems to know what they are.

Students, at least, are in a position to search for the underlying disease, and to try to explain it. I hope that they will do so instead of merely reacting to pronouncements from still other sources. Persons in responsible positions are likely to be under pressure to protect and extend these positions as we all know, and so students may properly question their motives. What appears to be needed is the clear application of the human brain to the political problems that beset young physicists. They, able to think clearly, will always do better than specialists in the more pseudo sciences.

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### Manpower contradictions

In your August issue there is an apparent contradiction between the letters of William Silvert and the reply of Susanne Ellis, on the one hand, and the reply by Hugh Wolfe to Robert C. Johnson's letter on the other. The first letters complain about lack of positions for physicists. Wolfe complains of staff losses and difficulties in recruiting competent people. I have also heard that the National Accelerator Laboratory encounters recruiting problems.

The resolution of the contradiction might well lie in the areas of work for which young physicists strive and the editorial work that the American Institute of Physics can offer. However, it would be good to have a more detailed review of positions available and positions sought by applicants for jobs.

I know from first-hand information that many of the smaller colleges are eager to find good physics teachers, and I think there are also some job openings in national laboratories. On the other hand, I also know of some young physicists who had considerable difficulty in locating positions to their liking even though, in the cases about

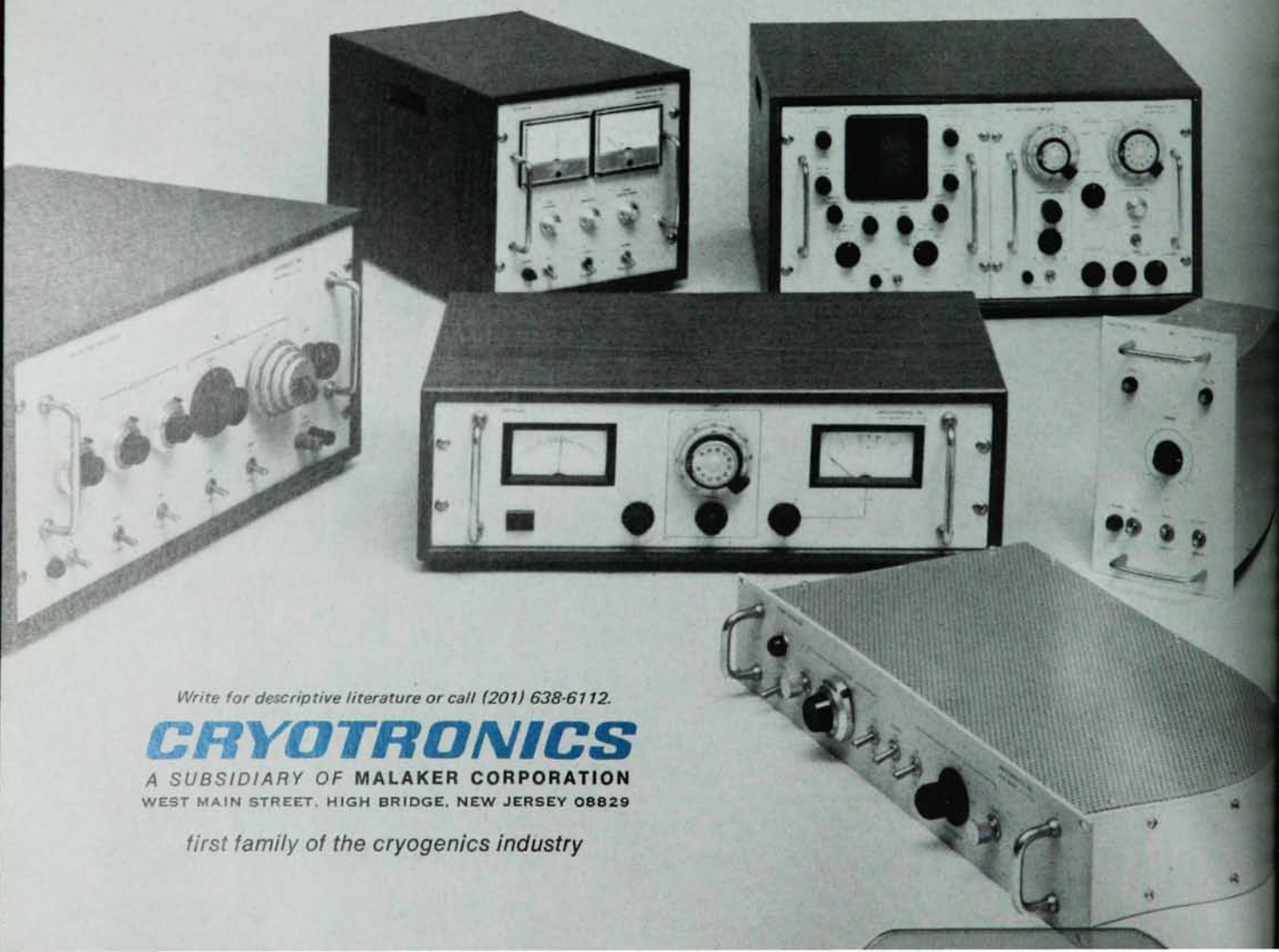


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which I know, they eventually succeeded. An article that would resolve the apparently conflicting statements of the letters would, I think, attract considerable interest.

EUGENE P. WIGNER  
Princeton University

### Personal ivory towers

As a physicist turned engineer (by choice) I could not help commenting on two things in PHYSICS TODAY. First the job shortage for PhDs. It exists because some people got the idea the US owed them a personal ivory tower—equipped with secretaries, technicians and an unlimited supply of money. Now the coach has turned into a pumpkin; the horses are mice, and a cold cruel employer asks, "What can you do for the corporation?" I reply it is just about time that Alice returned from Wonderland.

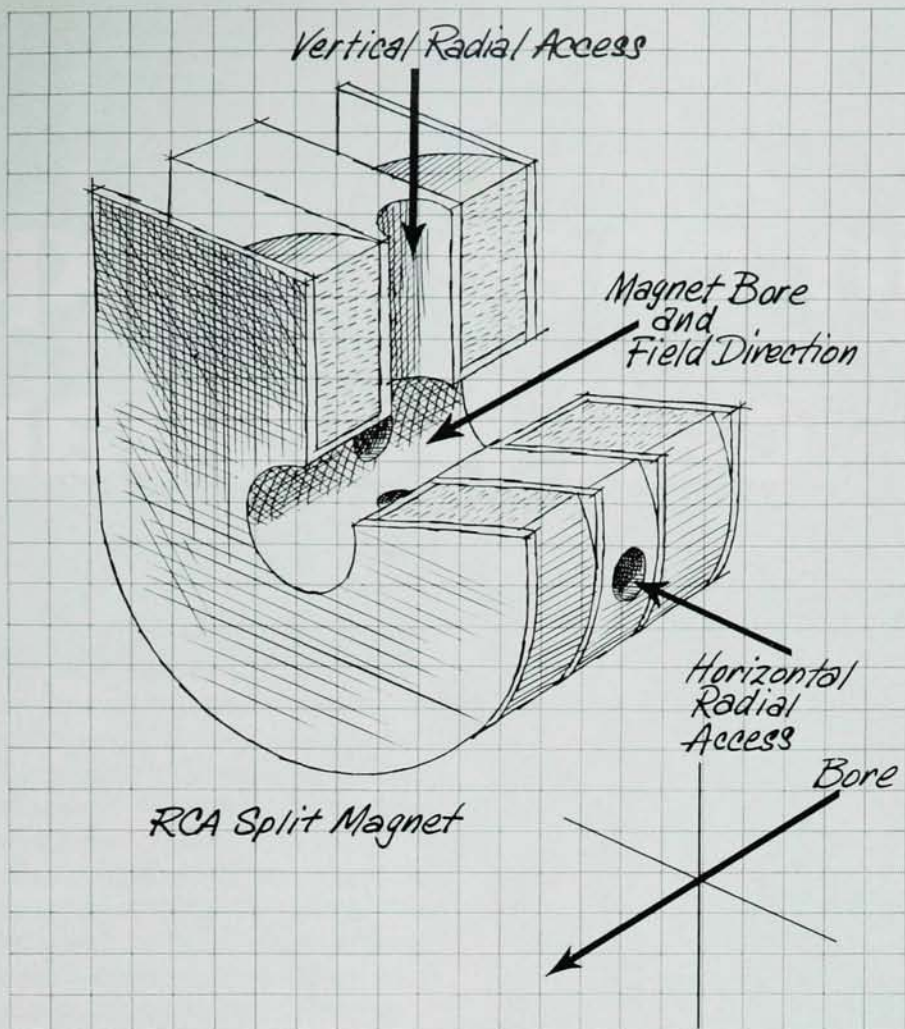
Second, the journals of the American Physical Society. They are excellent. Try submitting a paper to some engineering journal. Six months later (honestly) you get a letter saying, "We regret that the reviewer..."

STUART A. HOENIG  
University of Arizona

### Modifying lunar atmosphere

The lunar atmosphere (vacuum) is a source that has become available to mankind only within the last few years. It appears likely that studies of dual gas near the moon's surface will provide useful information concerning the history and composition of the body. It is possible that the moon will find important use as a support for large infrared and ultraviolet scopes, thermionic devices and other apparatus that requires high vacuum for operation. Perhaps it is worthwhile to point out that this environment may be changed appreciably in the process of lunar exploration—that in particular some consideration should be given to the effects of injection of large amounts of rocket gases into that environment.

A typical manned landing module must exhaust 5000 pounds of gases, mostly water and carbon dioxide in roughly equal molar amounts with measurable amounts of heavier hydrocarbons. At a mean temperature of 300 K the vertical distances required for 1/e reduction of atmospheric pressure on

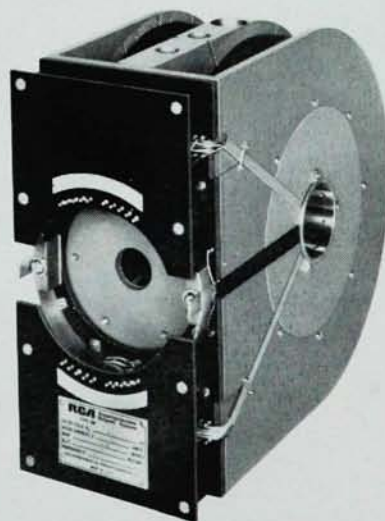


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the moon are  $8.2 \times 10^6$  cm for water and  $3.4 \times 10^6$  cm for carbon dioxide. These gases thus expand into effective volumes of  $3.1 \times 10^{24}$  cm<sup>3</sup> and  $1.3 \times 10^{24}$  cm<sup>3</sup>, respectively. To within an order of magnitude, the pressure rises to be expected due to ejection of this amount of gas are  $2 \times 10^{-13}$  torr for water and  $5 \times 10^{-13}$  torr for carbon dioxide. Pressures of this magnitude are measurable with commercially available equipment.

Simple estimates of typical escape times for these gases indicate that they will remain for at least several thousand years. We may then expect to modify the total lunar environment irreversibly, and only partly predictably, each time a rocket lands there. Only if the natural background pressures of water and carbon dioxide are several orders of magnitude larger than the above values will our perturbations of these quantities be unimportant.

JOHN O. STONER JR  
University of Arizona

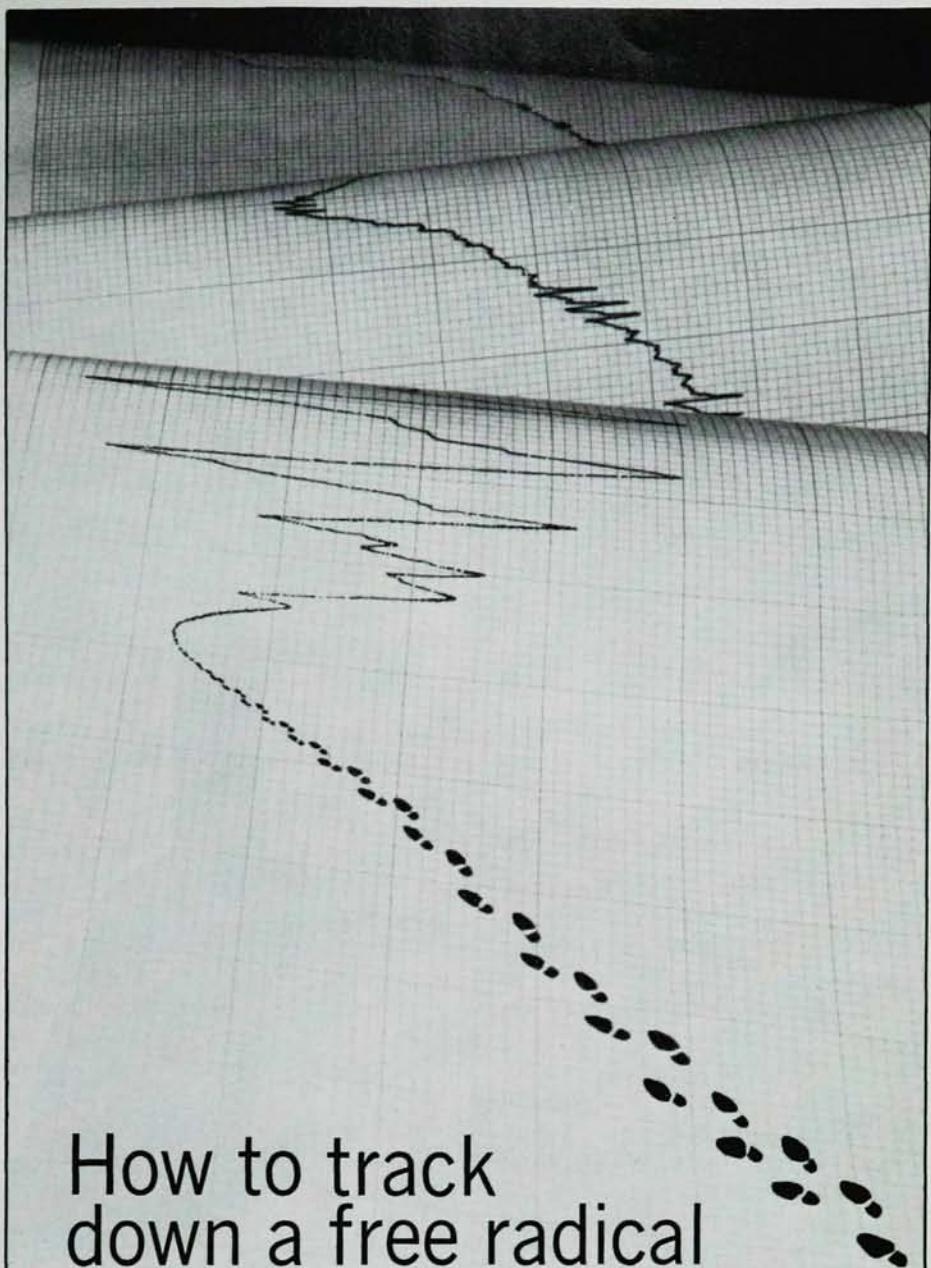
#### Emily Wolf and register

I enjoyed the article "The National Register Looks at Manpower" in the October PHYSICS TODAY. In one statement, though, it is in error.

At the request of Henry A. Barton, then director, and Wallace Waterfall, then as now secretary of the American Institute of Physics, I organized the register in November 1953. I employed Sylvia Barisch, your senior author, in March 1954 as one of my part-time coders. I remained in charge of the register until 1960, when it was transferred to the newly formed Education and Manpower Division. Mrs Barisch had been named supervisor in May 1959.

EMILY WOLF  
American Institute of Physics

**CORRECTION:** The editors apologize for two typographical errors in Don B. Lichtenberg's October review of *Paradoxes in the Theory of Relativity* by Yakov P. Terletskii. The word "comparable" was substituted in the last sentence, which should have read, "... the theory of relativity is *compatible* with dialectic materialism." The first equation in the fourth paragraph should have read  $\gamma = 1/(1 - u^2/c^2)^{1/2}$ . □



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## Physicists can paint doors

PATHDAFRELEDO was the name of the project: "paint those damned freight elevator doors." It started when Dan J. Zaffarano, chairman at Iowa State, decided that everything but the doors was just fine in the new physics building. The contest he initiated stimulated 85 entries from faculty, students and employees. Six were chosen for the six sets of doors. Lo, one of the winning designs (entered with num-



**FISHING DRAGON** won first prize for Klaus Ruedenberg and daughter Ursula.



**ZAFFARANO AND DAUGHTER** made one of six designs that went on doors.



**"END ALL WAR"** ceramic was first member of a growing sculpture court.



**SCRAPBOX SCULPTURE** was a surreptitious graduate-student contribution.

bers and not the artists' names) was that of Zaffarano and his junior-high daughter Elisa. Then at a "Slingathon" the designs were transferred to the doors.

First prize went to Klaus Ruedenberg, professor of physics and chemistry, and his daughter Ursula for "Charlie in Minnesota," a fantastic dragon fishing with his tongue in a fantastic fish pool. "Clyde" is a huge psychedelic frog that opens his mouth when the doors open. All other entered designs are now framed and decorate the building hallways.

Not only painted doors are part of the art scene at Ames. A courtyard between old and new parts of the physics building will soon have a new office complex on one of its other sides. It became a sculpture garden and acquired "End All War," a ceramic showing red flames of war rising from a green prairie and reducing civilization to a black cinder. Challenged, some graduate students got busy and surreptitiously put up a rival on dedication morning. "From Chaos

## New in nuclear power

"A tiny pellet, small enough to fit into a thimble, can light your home for three years," says a General Electric ad that I hear repeatedly on my favorite station, "the radio station of the *New York Times*." That doesn't sound much like huge cranes handling giant fuel elements, heavy shipping

to Beauty, Symbolizing the Wonderland Trip Toward the PhD" was an abstraction made from the cryogenics scrapbox with some round-the-clock work. Chief solderer, it turned out, was Durkee Richards, now a PhD physicist at 3M Research.

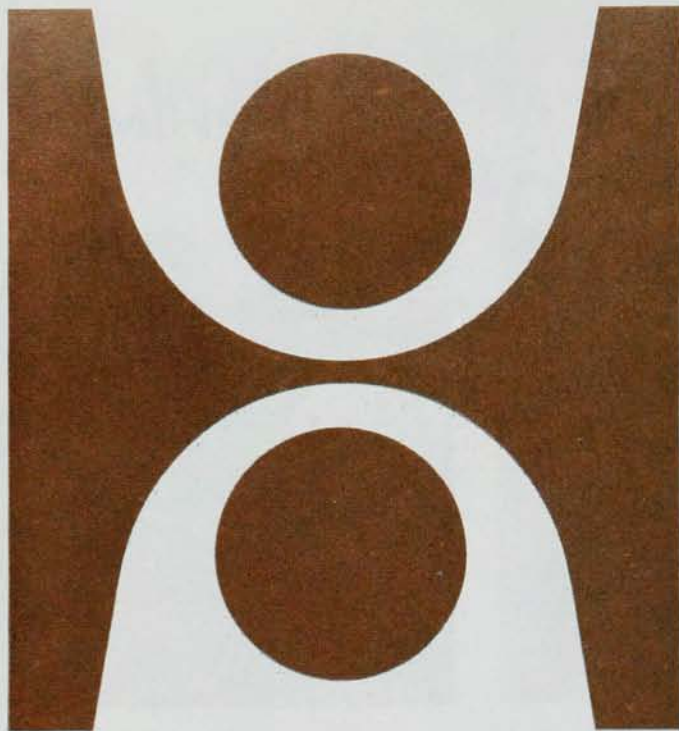
"What is all this about?" I asked Zaffarano. "I always thought you physicists were dedicated specialists quite unable to apply yourselves productively to anything but the subjects of your PhD theses." It seems I've been given the wrong picture.

"Physicists are creative people whose need for expression often transcends even publication in *The Physical Review*," was Zaffarano's answer. "Since our daily working environment seems to consist mostly of blackboards, vacuum pumps, dewars, magnets and racks of modular electronics, we thought it appropriate to observe through our new windows that the worlds of living things, abstract forms and color also provide outlets for research and creativity to intelligent people."

containers to shield against radioactivity and pressure vessels weighing hundreds of tons. You physicists at GE must have come up with something new in the way of tiny fuel pellets.

Somebody somewhere is confusing me, and I hope that the confusion is unintentional. □





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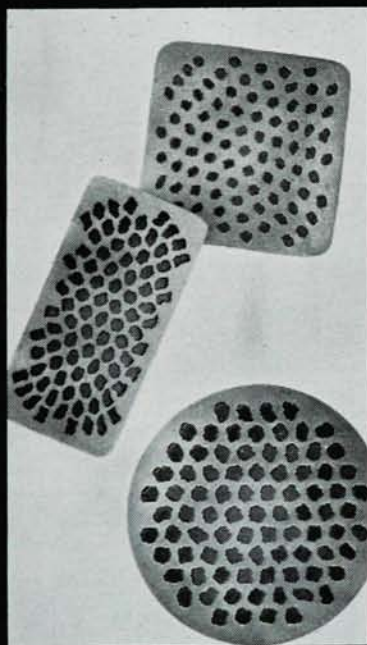


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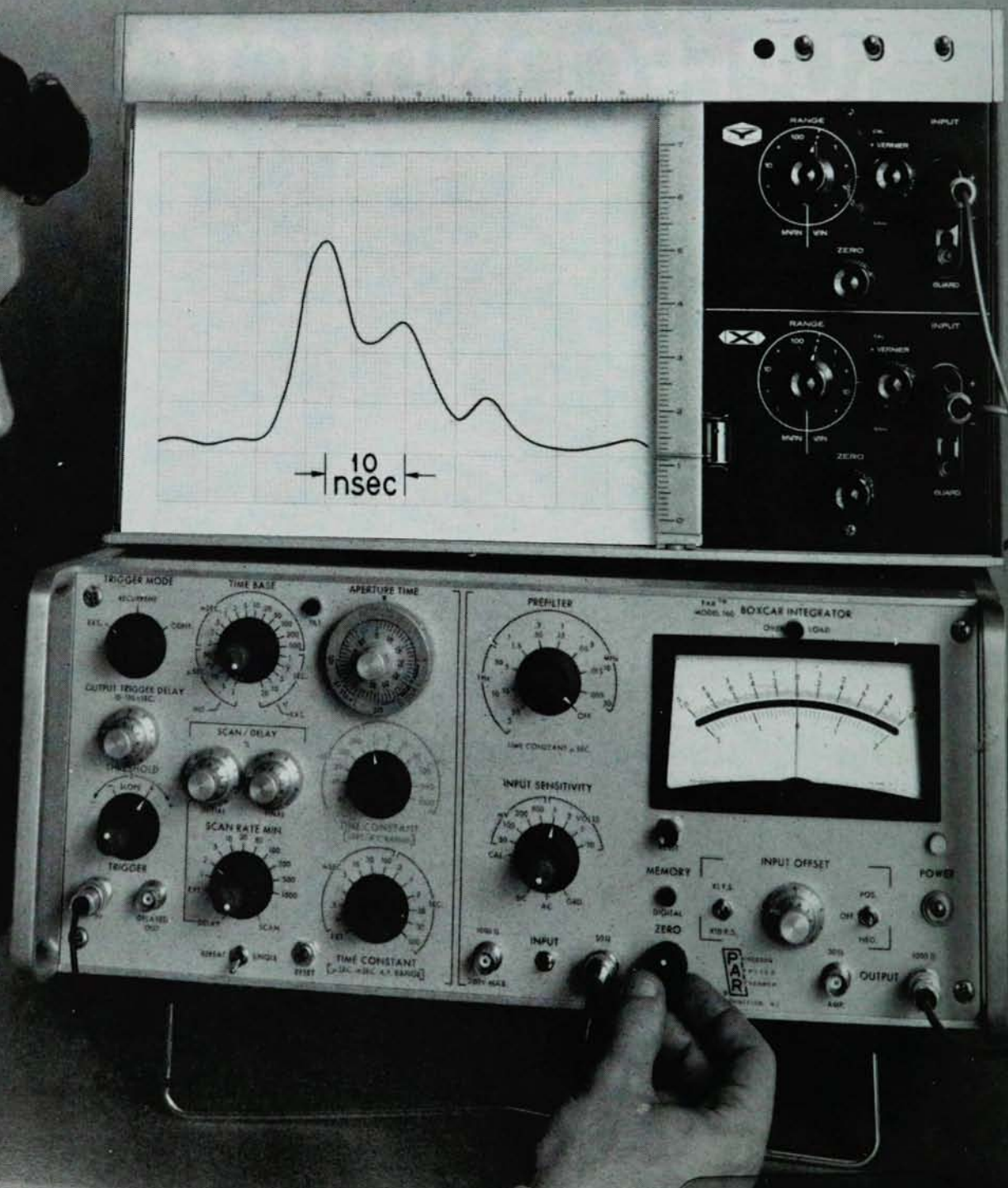
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## NUCLEON-NUCLEON SCATTERING

An atomic nucleus can be considered a set of two-nucleon systems.

What are the forces between these pairs?

How do protons and neutrons differ and how are they similar?

Studies during the past three decades have given some answers and indicated which new kinds of experiments are likely to be most useful.

MALCOLM H. MAC GREGOR

DETERMINATION OF THE fundamental law of force between two nucleons has occupied many physicists for the past three decades. Because the proton and electron have obvious similarities (elementarity, spin of  $1/2$ , equal-but opposite electric charge, Fermi statistics, antiparticles) the derivation of a nuclear "Coulomb's law" would seem to be a just reward for working in this area. As we have rather slowly and laboriously learned, however, simplicity appears to be inversely proportional to some power of the coupling constant.

Indirect means can be used to learn about the forces between two nucleons. An atomic nucleus, composed of protons and neutrons, can be reasonably treated as a collection of interacting two-nucleon systems. From the overall behavior of the nucleus, certain properties of the nuclear force, such as its range and the statistics it obeys, can be adduced. Direct information, however, is obtained only by scattering one nucleon off another. Analysis of these scatterings will be the subject of the present discussion.

### Early history

The most distinctive feature of the nuclear force, its very short range, was deduced by Ernest Rutherford in 1911. Modern studies of nucleon-nucleon interaction were initiated in 1932 when the neutron was discovered<sup>1</sup> and high-voltage particle accelerators first produced nuclear reactions.<sup>2</sup> Some of the

most crucial discoveries were made very early, as often happens. By studying the binding energy of the alpha-particle Eugene Wigner<sup>3</sup> in 1933 confirmed that nuclear forces have a short range and are very strong. Werner Heisenberg<sup>4</sup> and Ettore Majorana<sup>5</sup> pointed to the repulsive-core concept when they invoked exchange forces to explain the stability of nuclei against collapse. In 1935 Hideki Yukawa<sup>6</sup> predicted that the nuclear force should be mediated by exchange of a virtual meson with a mass of roughly 100 MeV.

Nucleon-nucleon scattering occurs within the constraints imposed by invariance under time reversal and conservation of angular momentum and parity. For a given total angular momentum  $J$  the proton-proton system has five independent ways in which the intrinsic spins and the orbital angular momentum can couple together. These alternatives are shown in figure 1. The two possibilities listed as amplitude 5 in figure 1 are equivalent; one is the time-reverse of the other, and we are assuming time-reversal invariance. The antisymmetry of the proton-proton wave function when combined with the conservation of angular momentum and parity prevents mixing of singlet ( $S = 0$ ) and triplet ( $S = 1$ ) spin states.

If we assume that the proton and neutron are isotopic states of the same particle that differ only in the  $I_z = \pm 1/2$  projections of their isotopic spin,

then the neutron-proton wave function must be antisymmetric. In this case, we have the same five scattering amplitudes in spin space as we did for proton-proton scattering. Scattering, however, now occurs in two isotopic spin states ( $I = 1$  and  $I = 0$ ); so the direct product gives ten independent neutron-proton scattering amplitudes. The  $I = 1$  amplitudes as measured in proton-proton and neutron-proton scattering should be identical in all but electromagnetic effects. This is



Malcolm H. Mac Gregor served in the US Navy at the end of World War 2 before obtaining his BA (in mathematics), MA and PhD (both in physics) at the University of Michigan. In 1953 he joined the Lawrence Radiation Laboratory at Livermore, where he is now a research physicist. Mac Gregor's work has included both experiment (beta decay and neutron scattering) and theory (nucleon-nucleon analysis). He is also a graduate research adviser and occasional lecturer at the University of California, Berkeley, with research interest in elementary-particle structure.



the *charge independence* hypothesis. The weaker *charge-symmetry* hypothesis specifies that, apart from electromagnetic effects, proton-proton and neutron-neutron forces are equal.

Nonconservation of isotopic spin is indicated by the unequal masses of charged and neutral pions and of the proton and neutron, the nonlinearity inherent in superimposing nuclear and electromagnetic forces and the differing anomalous magnetic moments of proton and neutron. Fortunately for us here, these are all rather small effects. Assumption of charge independence is, we shall see, indispensable for analysis of existing neutron-proton scattering data.

Because nucleon-nucleon scattering occurs simultaneously in five independent spin states, it is necessary to analyze five kinds of scattering experiments at a given energy simultaneously to determine the elastic-scattering matrix at that energy. This fact makes a definitive experimental determination of the scattering amplitudes a formidable task.

#### Observed spin and isospin dependence

Observations pointing both to the greatest complication in the nuclear force and to its greatest simplification occurred in 1936. The deuteron is a neutron and proton bound in a triplet spin state. From the deuteron structure we can infer the cross-section magnitude for triplet neutron-proton scattering at zero energy. The observed neutron-proton scattering, which is  $1/4$  in the singlet state and  $3/4$  in the triplet state, is much larger than this value. Thus, as Wigner<sup>7</sup> pointed out, singlet neutron-proton scattering must be much larger than



GREGORY BREIT (left) and EUGENE WIGNER. This famous resonance is shown at the Gainesville, Florida international nucleon-nucleon conference in 1967. —FIG. 2

triplet neutron-proton scattering; this work established that nuclear forces, unlike simple Coulomb forces, are spin dependent.

Further evidence for spin dependence of the nuclear force was soon forthcoming. In 1939, measurements of the magnetic moment and electric quadrupole moment of the deuteron<sup>8</sup> showed that tensor forces, leading to a D-state admixture in the S-wave ground state, are present. Analysis of high-energy proton-proton scattering data by Kenneth Case and Abraham Pais<sup>9</sup> in 1950 showed that spin-orbit components are also present in the nuclear force. The conclusion that we have today, which is borne out by studies with a variety of potential models, is that nature has taken full advantage of the freedom in nucleon-nucleon spin space afforded by the invariance principles; spin dependence of nuclear force is as complicated as

it is allowed to be.

The greatest and perhaps only simplicity in nucleon-nucleon scattering occurs in isotopic-spin space. In 1936, Gregory Breit and Eugene Feenberg<sup>10</sup> analyzed low-energy neutron-proton and proton-proton scattering and showed that the singlet-S nuclear phase shift (the  $I = 1$  scattering) is the same for both processes to within a few percent, thus experimentally establishing charge independence. Many subsequent experiments during the past third of a century have substantiated the usefulness of the charge-independence approximation. It is interesting that Breit and coworkers<sup>11</sup> in 1968 were the first to introduce a nucleon-nucleon phase-shift analysis in which charge independence is no longer strictly assumed. Breit's work in nucleon-nucleon interactions has spanned the entire modern development of the subject (see figure 2).

#### Nucleon-nucleon amplitudes

As we have seen, the proton-proton system has five complex amplitudes. If we eliminate one overall phase factor, specification of these amplitudes at one energy and angle requires nine numbers and hence nine independent experiments. If, however, measurements are made over all angles from 0 deg to 90 deg at one energy, then unitarity relations<sup>12</sup> relating real and imaginary parts of the amplitudes can be formulated. The result is that, in principle at least, five kinds of experiments at one energy and *all angles* suffice to specify the proton-proton scattering matrix at that energy.

Because all experiments contain sta-

| Amplitude | Spin S | Orbital Angular Momentum I |               |
|-----------|--------|----------------------------|---------------|
|           |        | Initial                    | Final         |
| 1         | 0      | $I = J$                    | $I = J$       |
| 2         | 1      | $I = J$                    | $I = J$       |
| 3         | 1      | $I = J + 1$                | $I = J + 1$   |
| 4         | 1      | $I = J - 1$                | $I = J - 1$   |
| 5         | 1      | $I = J \pm 1$              | $I = J \mp 1$ |

SPIN-SPACE NUCLEON-NUCLEON amplitudes for total angular momentum J. Intrinsic spin and orbital angular momentum can couple in five ways. —FIG. 1



tistical and other uncertainties, overspecification of the scattering matrix is desirable. If we do proton-proton measurements at energies above the pion-production threshold (280 MeV), then the unitarity relations are lost; so nine experiments are again required to specify the elastic-scattering matrix. In practice, inelastic effects are small up to 450 MeV, and accurate phase-shift analyses can be made up to that energy.

The neutron-proton system has five complex amplitudes for each of two isotopic-spin amplitudes. Because neutron-proton measurements from 0 deg to 90 deg and from 90 deg to 180 deg can be considered as independent experiments, five measurements over the angular range from 0 deg to 180 deg are enough to specify the neutron-proton scattering matrix for energies below 280 MeV. Unfortunately, neutron-proton data are often of limited statistical accuracy and often include only a few scattering angles. Also, neutron-proton experiments sometimes involve deuterium as a neutron target.

This use makes a substantial and somewhat controversial correction necessary to remove the effect of the spectator proton that is contained in the deuteron.

Thus, except at lowest energies, attempts to analyze neutron-proton data by themselves<sup>13</sup> have been unsuccessful. If, however, proton-proton and neutron-proton data at the same energy are available, then the proton-proton data can be analyzed to give the  $I = 1$  amplitudes, charge independence can be invoked to apply these to the neutron-proton scattering and the neutron-proton data can then be analyzed to give the corresponding  $I = 0$  amplitudes. An analysis of this type was first published in 1961.

Neutron-neutron experiments are difficult; so few of them have been done. The main effort here has centered on the final-state neutron-neutron interaction that is produced when deuterium is bombarded with neutrons or pions. Results indicate agreement to within 1% with the concept of charge symmetry and to within a

few percentage points with the concept of charge independence.<sup>15</sup>

### Nucleon-nucleon experiments

As we have seen, in the most general case nine proton-proton experiments are needed to specify the elastic-scattering matrix at one energy and angle. Not surprisingly, it turns out that nine independent spin-space experiments can be simultaneously defined.<sup>12,16</sup> Experiments were first done by scattering protons once ( $\sigma$ ), twice ( $P$ ,  $C_{NN}$ ,  $C_{KP}$ ), and three times ( $D$ ,  $D_T$ ,  $R$ ,  $R'$ ,  $A$ ,  $A'$ ). Figure 3 describes these observables. The recent development, however, of polarized proton beams and polarized targets has enabled experimenters to reduce the number of scatterings by one and to improve greatly the accuracy and comprehensiveness of the experiments. This "second generation" of experiments is just now starting to have an important impact on nucleon-nucleon work.

Fairly complete sets of nucleon-nucleon data exist at 25, 50, 95, 142, 210, 330, 425 and 650 MeV. These energies correspond, naturally enough, to energies of existing cyclotrons. It is interesting that, their primary mission of measuring nucleon-nucleon scattering fulfilled, some of these cyclotrons are now being scrapped.

### Phase shift analyses

One major difficulty in analyzing nucleon-nucleon data is that we have so little theoretical guidance. Scattering amplitudes are essentially unknown functions of energy  $E$  and scattering angle  $\theta$ . The conventional way of dealing with this situation is to expand the scattering amplitudes in terms of angular-momentum states

$$a(E, \theta) = f(E) g(\theta)$$

The  $g(\theta)$  are known functions that depend on the spin, orbital angular momentum and total angular momentum ( $S$ ,  $l$ ,  $J$ ) of the system. The  $f(E)$  are unknown functions of energy and are expressed in the following unitary form

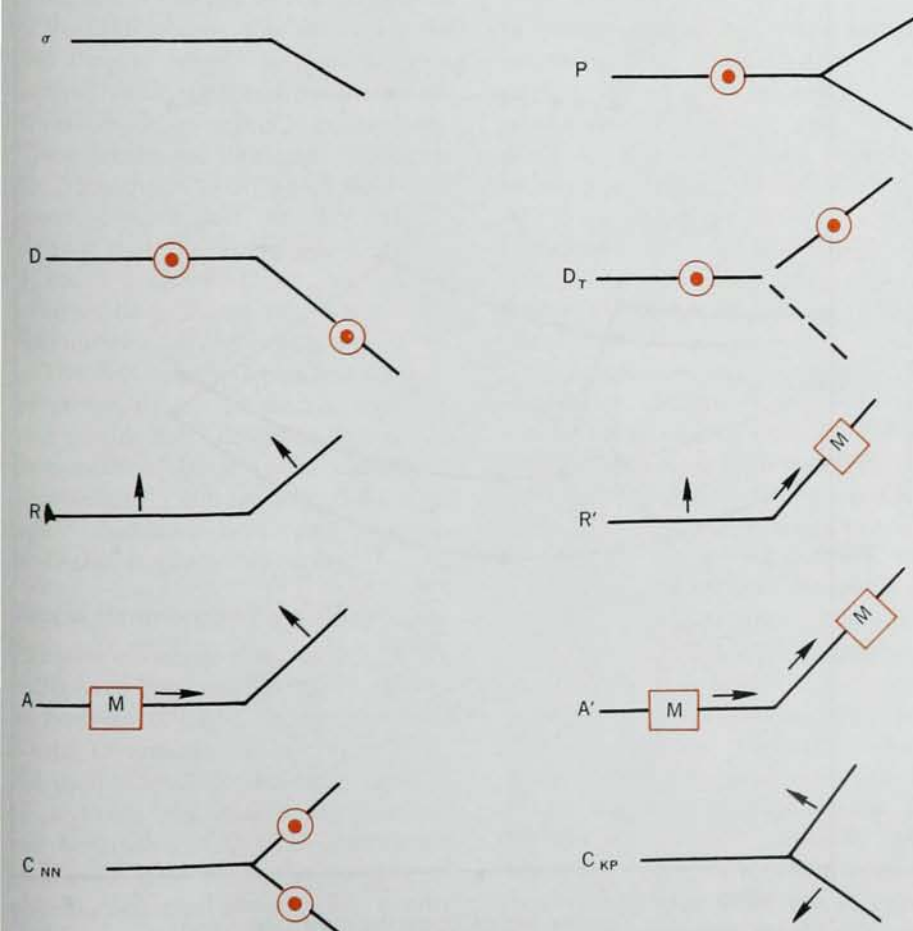
$$f(E) \propto e^{i\delta(E)}$$

where the phase shifts  $\delta(E)$  must of course carry labels  $S$ ,  $l$ ,  $J$ .

The spectroscopic form for the phase shifts<sup>17</sup>

$$\delta(E) \equiv {}^{2s+1}I_J(E)$$

is probably the prevalent notation today; notation used by the Yale group is very similar.<sup>18</sup> For the nuclear-bar



POSSIBLE NUCLEON-NUCLEON EXPERIMENTS. Laboratory-frame diagrams show polarization component to be measured; a dot indicates a vector out of the page, and M indicates 90 deg precession in a magnetic field. —FIG. 3



phase shifts,<sup>17</sup> as used for example in our work at Livermore, the lowest states of the proton-proton system are:  $^1S_0$ ,  $^3P_0$ ,  $^3P_1$ ,  $^3P_2$ ,  $\epsilon_2$ ,  $^3F_2$ , . . . , where S, P, D, F, . . . correspond to  $l = 0, 1, 2, 3, \dots$ , and where  $\epsilon_l$  is the mixing parameter.

The phase-shift decomposition of scattering amplitudes has several advantages: because a few low- $l$  phases dominate the scattering the number of free (phenomenological) phases can be kept reasonably small; physical information can be inserted by using effective-range low-energy limits for S-waves; theory can be inserted by calculating the small, high- $l$  phases from the one-pion-exchange Feynman diagram and the observed energy dependence of the phase shifts can be used to test theoretical models. The outstanding disadvantage of phase-shift formalism is that the equations are nonlinear.

Calculation of phases directly from experiment or of potentials directly from phases has proved to be impossible. It is necessary instead to go in the other direction. This means that we can determine a set of phase shifts only by making least-squares fits to the data, and we can determine parameters of a nuclear-force model only by making least-squares fits to the phases or to the data directly.

Phase-shift analyses can be made at a single energy (actually a narrow energy band), or over a whole range of energies. For a range of energies we must choose a set of parameters that express the energy dependence of the phase shifts, and these parameters are then varied to minimize the least-squares-sum  $\chi^2$ . The only two groups to carry out large-scale energy-dependent analyses have been Breit's Yale group and the Livermore group. Energy-independent analyses have been carried out at several laboratories.

To determine phase shifts one selects a set of phases, calculates the corresponding observables, determines the least-square sum  $\chi^2$  for a fit to the data and then varies the phases to minimize  $\chi^2$ . If the data are complete, statistically accurate and self-consistent, a unique solution (set of phases) results. In a typical analysis, 1000 to 2000 data may be included in the  $\chi^2$  sum. The variable parameters, which include both phase-shift coefficients and data-normalization constants, can number 100 or more. Thus selection of a method to minimize the param-

eters is a nontrivial part of the problem.

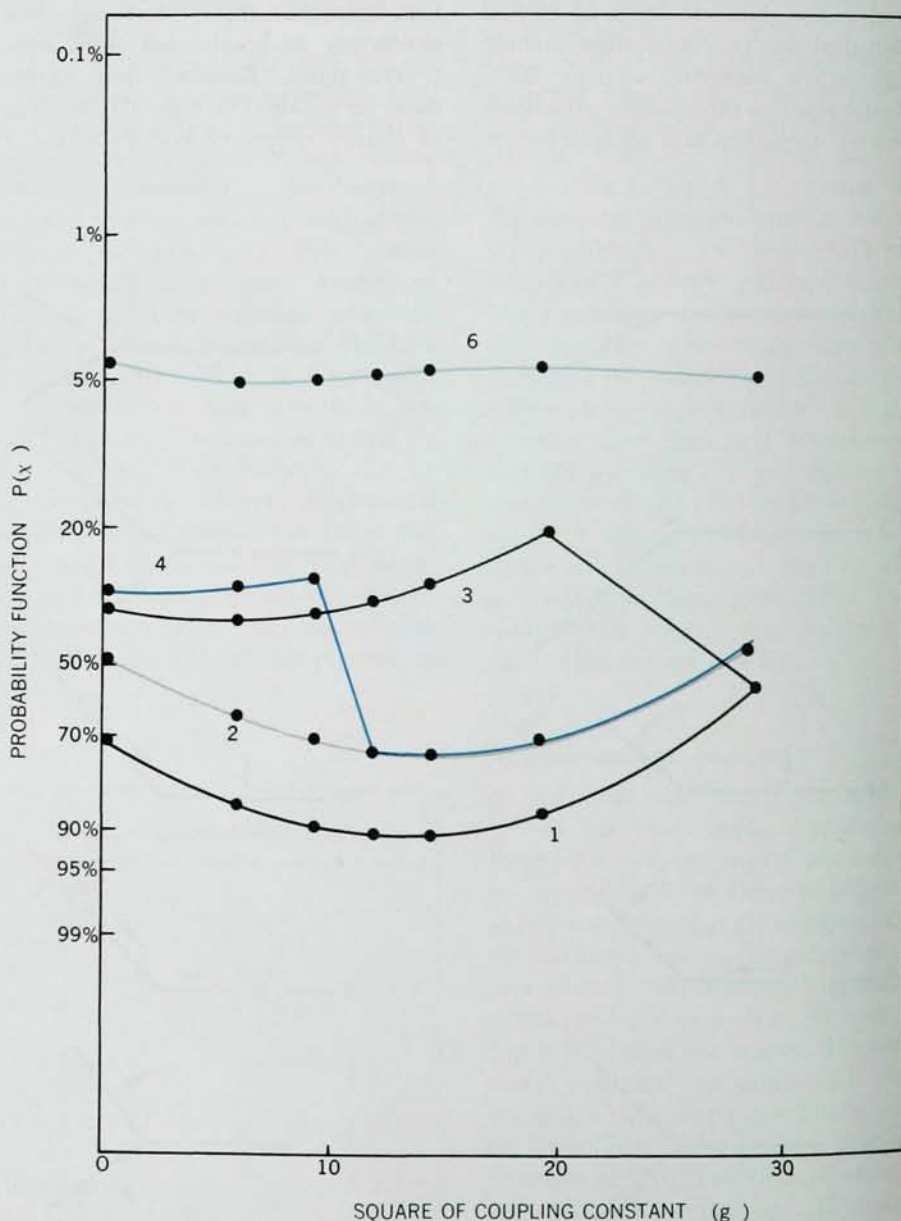
Early computer problems at Livermore used the grid-search method, in which one parameter at a time is varied. Because the parameters are highly correlated, this is a very inefficient method for a large problem. An improved method, used in early work at Yale,<sup>18</sup> is the gradient search in which all parameters are varied together but in an uncorrelated manner. The most efficient method for large problems is the matrix search,<sup>19</sup> in which all parameters are varied simultaneously along a correlated path in parameter space.

Although the matrix search has been used in other applications for a long time, its first application to the nucleon-nucleon problem was by Peter Signell.<sup>20</sup> The matrix search has an

additional advantage; the error matrix for the solution is automatically obtained. At Livermore, a method of matrix reduction devised by Richard Arndt<sup>19</sup> is used to split phase parameters and normalization constants into a two-step minimization process. This method lowers the dimensionality of the matrices by almost a factor of 2 and greatly reduces computer storage requirements.

### Early phase-shift results

The first use of a "computer" to attack the nucleon-nucleon problem was in the work done by E. Clementel and Claudio Villi<sup>21</sup> in 1955. Their computer was a set of mechanical arms that could be set to give an analog simulation of certain scattering-amplitude functions. They were able to show that, given only proton-proton



PROBABILITY FUNCTIONS for 310-MeV Stapp phase-shift solutions. The maximum probability obtained for Stapp solutions 1 and 2 at  $g^2 = 14$  agrees with the  $g^2 = 15$  value obtained from pion-nucleon scattering analyses.

—FIG. 4



differential cross-section data, there are four sets of P-phases for each value of the S-phase (up to some maximal value for S), and all give precisely the same fit to the data. This work was later adapted at Livermore<sup>22</sup> for UNIVAC I, the world's first true electronic computer.

Modern phase-shift analysis started at Berkeley. In 1956 a group using the 184-inch cyclotron completed measurements of  $\sigma$ , P, D, R and A at 315 MeV.<sup>23</sup> Armed with these data, Henry Stapp and his collaborators, who had access to Livermore and Los Alamos computers, did a proton-proton phase-shift analysis. They used 14 free phases (S-H waves), set the remainder equal to zero and found five acceptable phase-shift solutions.

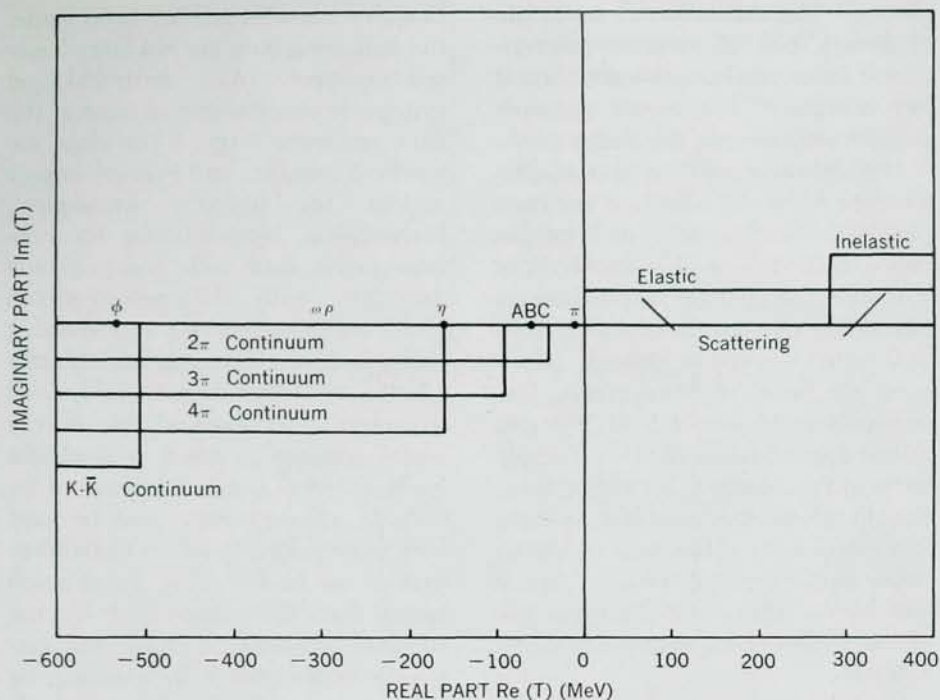
Following the lead of the Japanese school,<sup>24</sup> Michael Moravcsik<sup>25</sup> and A. F. Grashin<sup>26</sup> independently proposed that the Stapp analysis could be improved by calculating the higher phase shifts from one-pion exchange (OPE) instead of just setting them equal to zero. This reduced the number of acceptable solutions to two, called "Stapp solutions 1 and 2." In addition, the pion-nucleon coupling constant, which enters into the calculation of the OPE phases, was shown for the first time to have a nucleon-nucleon analysis value consistent with that obtained from pion-nucleon analyses. These results are illustrated in Figure 4. Subsequent analyses of Rochester proton-proton data at 210 MeV<sup>27</sup> showed that the Stapp-solution types 1 and 2 occurred there also. Later analyses have shown that Stapp solution number 1 is the correct one.

The first energy-dependent analysis of proton-proton scattering was carried out by the Yale group,<sup>28</sup> and was soon followed by a similar analysis at Livermore.<sup>29</sup> Subsequent Yale analyses<sup>14</sup> included both proton-proton and neutron-proton scattering.

#### Recent elastic-scattering studies

Analyses of energy-independent phase-shifts have been carried out by groups at Berkeley, CERN, Dubna, Harwell, Kyoto, Livermore, and Michigan State. All used essentially the same method of analysis; differences in solutions can be attributed to slightly different choices of data or of the number of phenomenological phases. The results of these analyses are in general agreement with each other.

The Yale group<sup>11</sup> has carried out energy-dependent phase-shift analyses



**SINGULARITY STRUCTURE** in the complex kinetic-energy plane for nucleon-nucleon scattering amplitudes. Poles on negative real axis become cuts when a partial wave projection is made. Left-hand singularities correspond to nuclear forces, and right-hand singularities are unitarity cuts. Here  $T = 4K^2/2M$ . —FIG. 5

of proton-proton and neutron-proton scattering from about 10 MeV to 350 MeV. We at Livermore have completed similar analyses from about 1 MeV to 450 MeV<sup>30</sup> and additional analyses extending to 750 MeV.<sup>31,32</sup> The Yale group, in choosing energy-dependent forms for the phase shifts, selected pure mathematical functions with the requisite flexibility to fit their data.

At Livermore functions that have a singularity structure<sup>19</sup> and threshold behavior<sup>33</sup> consistent with the dictates of the Mandelstam representation were used (see figure 5). In regions where data are complete and accurate enough to set limits on the solution, the Yale and Livermore phase-shift values are in reasonable agreement. This agreement indicates that neither analysis is appreciably form-limited and that energy dependences obtained for phase shifts are reliable. As further confirmation the Livermore work also includes single-energy analyses at 25, 50, 95, 142, 210, 330, and 425 MeV. The energy-dependent and energy-independent phase shifts are in agreement; this agreement would not occur if the energy-dependent forms were too rigid.

These phase-shift results fulfill the

longstanding goal of obtaining a set of nucleon-nucleon scattering amplitudes that cover continuously the entire elastic-scattering region. The final Livermore analysis includes 1076 proton-proton data from 1 to 450 MeV and 990 neutron-proton data from 0.5 to 425 MeV. 52 phenomenological parameters representing 27 elastic phases and one inelastic phase are sufficient to give a statistically accurate fit ( $\chi^2 = 1.1$  per data point) to the entire collection of 2066 data spanning this energy range. Also, because the parametrization is continuous and mathematically well defined, the parameter error matrix gives statistically determined uncertainties in the phases and in all functions of the phases over the energy range.

#### Remaining problems

There are still some difficulties with phase-shift analyses, particularly with the  $I = 0$  amplitudes. At low energies we expect from the sign of the deuteron quadrupole moment that the  $\epsilon_1$  coupling parameter should be positive.<sup>34</sup> Also, the  $^1P_1$  phase shift might be expected to approximate its OPE value at low energies. Phase shift analyses, however, often give anomalous values below 50 MeV for these



phases. The difficulty can be attributed to a lack of accurate neutron-proton differential cross-section data at low energies,<sup>30</sup> but recent measurements<sup>35</sup> may remedy this deficiency.

Unfortunately, existing neutron-proton data below 50 MeV are not completely self-consistent. At energies above 210 MeV, and particularly at 330 MeV, the neutron-proton data are incomplete enough that an accurate  $I = 0$  matrix can not be defined. However, the latest triple-scattering neutron-proton data at 425 MeV<sup>36</sup> give a well defined result for  $I = 0$  amplitudes at that energy. By adding these data to the energy-dependent analysis, it is possible to obtain reasonably reliable neutron-proton phase shifts at 330 MeV. This result illustrates one of the virtues of an energy-dependent analysis.

To achieve accurate fits to the data below 10 MeV, one must apply vacuum-polarization corrections to the proton-proton amplitudes and use separate  $^1S_0$  phases for the proton-proton and neutron-proton systems. The data are now so accurate that failure of charge independence for the  $^1S_0$  phase must be taken into account.<sup>11</sup> The other phases do not yet require this additional freedom.<sup>30</sup>

At energies above 280 MeV inelastic effects should be considered. Up to 450 MeV, inelastic scattering is less than 10% of elastic scattering. On theoretical grounds it is reasonable to attribute this small inelasticity entirely to the  $^1D_2$  phase shift. Inclusion of an inelastic component in the  $^1D_2$  phase does not appreciably lower  $\chi^2$ , but it gives slightly different and more realistic phase-shift values.

To summarize the proton-proton situation, 1076 carefully selected proton-proton data form a set that spans the 1-450 MeV region. This set yields good statistical accuracy, reasonable completeness at selected energies and self-consistency within the data set. These data determine a unique phase-shift solution; scattering amplitudes are accurate to within a few percent over the entire elastic energy range and up to about 450 MeV. Restrictions imposed by fitting all of these data simultaneously are stringent enough that inconsistencies between these data and any new measurements can be promptly identified.<sup>37</sup>

The neutron-proton situation is not so favorable: the 990 experimental points form a set that spans the energy region from 0.5 to 450 MeV, but al-

though some selection has been made, the remaining data are not completely self-consistent. Also, statistical and systematic uncertainties in some of the data are quite large. The data are nowhere complete and in many energy regions are woefully incomplete. Nevertheless, by combining the neutron-proton data with proton-proton data (or with the proton-proton  $I = 1$  scattering matrix) and invoking charge independence, we can obtain a solution type that is reasonably well delineated over most of this energy region. Errors in the  $I = 0$  phases given by error matrices appear to be realistic, although they must be used with some reservations; an incomplete data set can lead to actual errors much larger than those predicted by the standard statistical analysis, and systematic errors caused, for example, by improper corrections for binding effects in the deuteron, would not be reflected in the error-matrix calculations.

Errors in energy-dependent phases are given by the parameter error matrix. These should be regarded as the smallest possible errors and would be the true errors if the energy-dependent forms were correct. Errors given by energy-independent analyses should be regarded as the greatest possible errors and would be the true errors if experiments at one energy were completely uncorrelated with experiments at other energies. By carrying out both types of analysis, we can obtain bounds for the errors. Because phase shifts are highly correlated, so are the errors. To obtain accurate statistical results in fitting to a model, one must use the full error matrix; the diagonal components are not sufficient.

### *Recent inelastic-scattering work*

Inelastic corrections are small and can be appropriately handled at energies up to 450 MeV. Few data exist in the region between 450 and 600 MeV, but from 600 to 700 MeV quite a complete proton-proton data set exists. Most of the data are from Dubna,<sup>38</sup> but substantial contributions have been made at other laboratories, such as Berkeley and Saclay, France. The big difficulty at 650 MeV is that the inelasticity is now roughly 40% of the total scattering, and simple treatment of inelastic phases does not suffice.

Phase shift studies have been made at 650 MeV,<sup>39</sup> and solutions can be obtained that give excellent fits to the data. These solutions, however, in-

volve a somewhat arbitrary handling of the inelasticity; one must apportion the inelasticity among a number of phase shifts, and there is remarkably little theoretical guidance as to just how to do this. In studies at Livermore<sup>31</sup> we tried many different models for the inelasticity, and we obtained a corresponding number of elastic phase-shift solutions. Coupling between inelastic and elastic processes is strong.

Our conclusion at Livermore (to which some of our colleagues do not wholly subscribe<sup>40</sup>) is that a definitive set of proton-proton phases at 650 MeV can not be obtained from present data and the present state of inelastic-scattering theory. Nine complete proton-proton experiments would in principle define the proton-proton elastic-scattering matrix at 650 MeV, but these experiments do not yet all exist. The data on inelastic scattering and the theory to handle these data are both very sketchy. High-intensity cyclotrons planned for the Swiss Federal Institute of Technology, Zürich and for Los Alamos should supply important new measurements in this energy region.

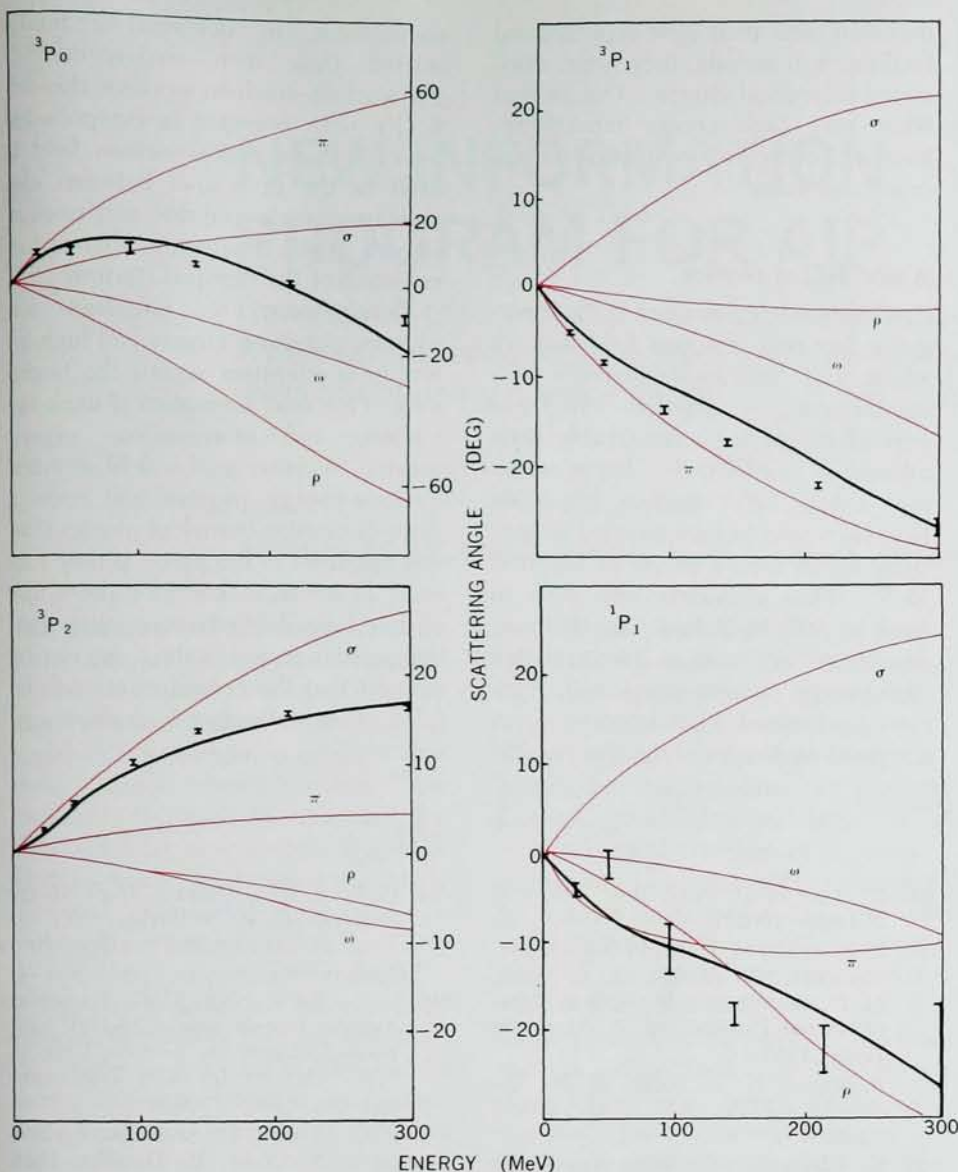
Any 650-MeV neutron-proton analysis, because it necessarily depends on  $I = 1$  amplitudes obtained from proton-proton scattering and on  $I = 0$  inelastic effects, is thus almost meaningless. Solutions can be obtained that give precision fits to the data, and the magnitudes of the large  $I = 0$  phases can be roughly determined. But small uncertainties in  $I = 1$  amplitudes become large uncertainties in addition to uncertainties for the  $I = 0$  amplitudes.

As far as definitive phase-shift analyses of the nucleon-nucleon system are concerned, I feel that present experimental and theoretical situations combine to impose a sharp cutoff at 450 MeV; this is perhaps just a way of saying that the opportunities exist at higher energies.

### *Implications for theory*

The outstanding theoretical success in the nucleon-nucleon field in the last decade has been the one-boson-exchange (OBE) model.<sup>41</sup> The only part of the nuclear force that can be calculated unambiguously from field theory is that due to exchange of a single (virtual) particle, the pion. If, however, we consider narrow resonances in multipion states as "particles," then we can calculate their contributions to the nuclear force. It is a remarkable fact that if the pion, the





P-WAVES as determined experimentally (error flags) and as calculated from one-boson exchange.  $\pi$ ,  $\rho$ ,  $\omega$  and  $\sigma$  Born terms all make important contributions, and the sum (heavy solid line) is in good qualitative agreement with experiment. —FIG. 6

$\rho$  and  $\omega$  resonances and a strong scalar-isoscalar interaction (taken for convenience to be the  $\sigma$  resonance) are treated in Born approximation, they give phase-shift values for P-waves and higher that are in good qualitative agreement with experiment for proton-proton and neutron-proton scattering over the elastic-scattering range (see figure 6). Furthermore, masses and coupling constants that must be used to obtain this good fit agree with values that can be deduced from direct measurements and other physical processes.<sup>42</sup>

There are, however, definite limitations to the one-boson-exchange model; unitarity corrections to the Born terms are small and unimportant for the high- $l$  amplitudes and are large and unbelievable for the lowest- $l$  amplitudes. Thus, although the lowest-

order OBE model is a good one, it is difficult to improve. The challenge imposed on theorists by the OBE model is to explain the existence of the  $\rho$  and  $\omega$  resonances. Just why these saturate their  $2\pi$  and  $3\pi$  quantum states, and why they contribute so decisively to the nuclear force, is in my opinion the main question to be answered by nucleon-nucleon theorists. From nucleon-nucleon analyses, we can not conclude much of anything about the width of the  $\sigma$  resonance. A strong enhancement in this state, however, is certainly required to fit the data.

The challenge at higher energies is to calculate, in a useful way, the pion-production amplitudes. It is clear from low-energy work that exchange of a single virtual pion is the dominant mechanism in all phases (even

including P-waves!) except S-waves. At energies above 280 MeV we are in the regime where a real pion is produced. One feels intuitively that ability to handle the appearance of a real pion from the virtual cloud surrounding a nucleon would contribute substantially to understanding the properties of that cloud.

Another challenge, one that may perhaps be studied at both lower and higher energies, is to see what limits nucleon-nucleon scattering data impose on interaction at very short distances.<sup>43</sup> The hard core has recently become in many models a softer core, and the nature of the core region is important in nuclear-structure calculations. The extent to which measured nucleon-nucleon amplitudes limit this region, and the relevance of these amplitudes to phenomena like the non-locality of the potential, remain fruitful areas for investigation.

#### At the crossroads

In the 1930's broad features of nucleon-nucleon interaction were determined. The ensuing three decades have seen this work extended experimentally until now a reasonably complete mapping has been obtained for proton-proton and neutron-proton scattering over the entire elastic energy region. Roughly speaking, this mapping has an accuracy of perhaps 5% for proton-proton scattering and 10% for neutron-proton scattering. This accuracy is good enough to impose reasonable bounds on potential models, and to make it appear unlikely that any major surprises will occur if these experiments are extended at the same level of sophistication.

The experimenter's choice is to retire or to aim for the 1% level. At Harvard and Rochester the choice was to retire. At Berkeley, Chicago, Dubna, Los Alamos and Orsay, experiments featuring polarized targets are superseding older triple-scattering experiments. At Saclay, a recent entrant into low-energy nucleon-nucleon work, a high intensity polarized proton ion source has been developed. Similar beams for low-energy measurements have been developed at Berkeley and Los Alamos.

At 1%-accuracy level, phase-shift analyses must include careful corrections for magnetic-moment effects, vacuum-polarization effects and manifestations of charge-independence breakdown. Theoretical models should be-



gin to show some sorting of  $2\pi$  and  $3\pi$  effects. Because experimental uncertainties are magnified in analytically continuing the scattering amplitudes off the energy shell, improved accuracy would permit a better determination of the usefulness of the bootstrap concept in this area.

In the inelastic region, the right turn at the crossroads would lead to a double-barreled experimental-theoretical attack on the nucleon-nucleon problem. Theorists must derive models for production processes, tell experimentalists just what kind of pion-production experiments they need to test the models, recheck their models with the experiments and repeat the process. An on-line collaboration is needed to get meaningful results in this difficult area. The planning groups at Zürich and Los Alamos see the need for this kind of close collaboration between theory and ex-

periment, and their new experimental facilities will include, they hope, associated theoretical groups. Dubna, and other very high energy laboratories have of course followed such as approach for years.

### A new field of physics

One outcome of this work is the emergence just now of a new field that we might call intermediate-energy elementary-particle physics. High-energy physicists have remarkably little interest in anything that happens below a few GeV; nuclear physicists have no reason to be interested in anything higher than a couple of hundred MeV. Thus physicists who wish to work at 500 MeV find that they are no longer welcome at the crowded high-energy conferences, and they can't understand what is going on at the nuclear-physics conferences. So

they have, in desperation, finally started their own conferences.<sup>39,44</sup> For nucleon-nucleon workers, this difficulty with energies is compounded because the nucleon-nucleon field is itself in the gray area between elementary-particle physics and nuclear physics. Does it belong in volume 4 or volume 5 of the Physical Review?

Development of polarized ion sources, polarized targets and high-intensity accelerators signals the beginning of the next generation of nucleon-nucleon and pion-nucleon experiments. Workers in this field of intermediate-energy physics will form a more distinctive branch of physics than was apparent in the past. If they succeed, however, in knocking down any of the formidable barriers that limit our present understanding, we can be assured that the consequences will be felt by their colleagues both above and below.

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# NEW INFORMATION PROGRAM FOR AIP

How do you cope with the ever-increasing flood of literature? A new computer-assisted system will offer new and better ways of obtaining physics information. We seek your opinions.

ARTHUR HERSCHMAN, FRANZ L. ALT and H. WILLIAM KOCH

THE AMERICAN INSTITUTE OF PHYSICS, with support from the National Science Foundation, is currently engaged in a major effort to develop and implement a computer-assisted "National Information System for Physics." Designed by physicists, for physicists, the new system is scheduled to begin pilot operations early next year. We at AIP believe the system to be urgently needed, but how do you, the physicists, feel about it?

This article presents a description of the main features of the new system so that you can form an opinion on its merits and its potential usefulness. After you have read the article, we hope to hear from you on this important question: Do you feel there is need for this program and is it aimed in the right direction?

## AIP responsibility

Because we believe that there is a need for a physics-information system and that AIP is the logical place for its development, we have assumed the responsibility and undertaken the formulation and development of a new system.

AIP was founded in 1931 as a federation of leading societies in physics to serve those needs of the physics community that could best be fulfilled by the societies jointly. It presently has seven member societies, whose 47 000 members are also institute members, plus 19 affiliated societies with an interest in physics, 150 corporate associates and a Society of Physics Students. The institute services for this sizable community run the gamut from publicizing physics and physicists, strengthening educational programs, documenting the history and development of physics and rep-

resenting physics nationally and internationally—to the largest single publishing effort for physics in the world. AIP publishes 16 archival journals, comprising 25% of the world's articles in physics and translates 13 Russian journals, for an additional 10%.

As a natural extension of its responsibility and in accord with its mandate to engage in activities "for the advancement and diffusion of the knowledge of the science of physics," AIP, with support from NSF, has been actively planning further information services since mid 1966.<sup>1</sup> These plans led to our program for the design and development of a national information system for physics. At the end of 1967, a new division was organized within AIP to handle the project.<sup>2</sup>

The division has the assistance of a

15-member advisory committee, which was appointed by AIP member societies, of about 100 physicist-respondents selected by the advisory committee and of liaison members from other interested groups, both from related scientific societies (chemistry, mathematics and engineering) and from interested government agencies. The results of this effort, a national physics-information system, will be ready for implementation during 1970. (A document describing the proposed system was recently presented to NSF in support of a request for funding the pilot operations.)<sup>3</sup>

## Why a new system?

A new system is needed to cope with the exponential growth of physics literature, which has been doubling about every seven and a half years. It



Arthur Herschman (left) has been director of the AIP information division since its inception in 1967. A theoretical physicist, who received his PhD from Yale University in 1954, Herschman was formerly coeditor of *The Physical Review*.

Before becoming AIP director in 1966, H. William Koch (center) was chief of the radiation-physics division at the National Bureau of Standards. Koch joined NBS in 1949, after receiving his PhD from the University of Illinois, and worked in the high-energy-radiation section until becoming division chief.

Franz L. Alt (right), who took his PhD in mathematics at the University of Vienna, became deputy director of the information division after 19 years with the National Bureau of Standards. At NBS he was assistant chief of the applied-mathematics division and, later, area manager for information systems, design and research.



is not that physicists are writing more, but that more physicists are writing—more physicists in every speciality. In 1968 alone, over 50 000 research papers were published in more than 500 journals. Finding information in the traditional way, by scanning journals and through formal and informal talks at meetings, is no longer practical. Although one may still keep up with new developments of immediate interest, it is almost impossible for any one physicist to keep abreast of bordering areas and related specialties with which he should be familiar.

The tendency of the present procedures to be designed for authors' convenience has also aggravated the problem. Authors, not readers, determine when, where and how the information is presented. As a result, papers on any given subject are dispersed over many journals, and a single journal may contain, side by side, papers on widely different subjects. The reader is left to cope with the flood as best he can, which all too often results in information coming to his attention too late for his need.

What is clearly required is a better way to organize and manage the information so it can be routed more accurately and efficiently from author to reader.

### Computerized file

The only feasible way to organize and manage a collection this large is by computer—to have a computer record of each new paper that allows a file to be organized and searched on a current basis according to physicists' interests.

As presently conceived, the file would initially contain records for about one half of the world's physics-journal articles, but would be expanded to cover almost all journal literature, as well as nonjournal material, in the not-too-distant future.

Every month, for AIP-published journals during the prepublication cycle and for other journals as they are received, the information-division staff will prepare, for each new paper, a "record" that contains basic information about the paper: author, journal, title, abstract, citations (that is, references to other literature), a list of "key words" and a special "AIP classification number."

These records are then transcribed onto magnetic tapes. Thus the cumulative file of all such records constitutes, in effect, a machine-searchable

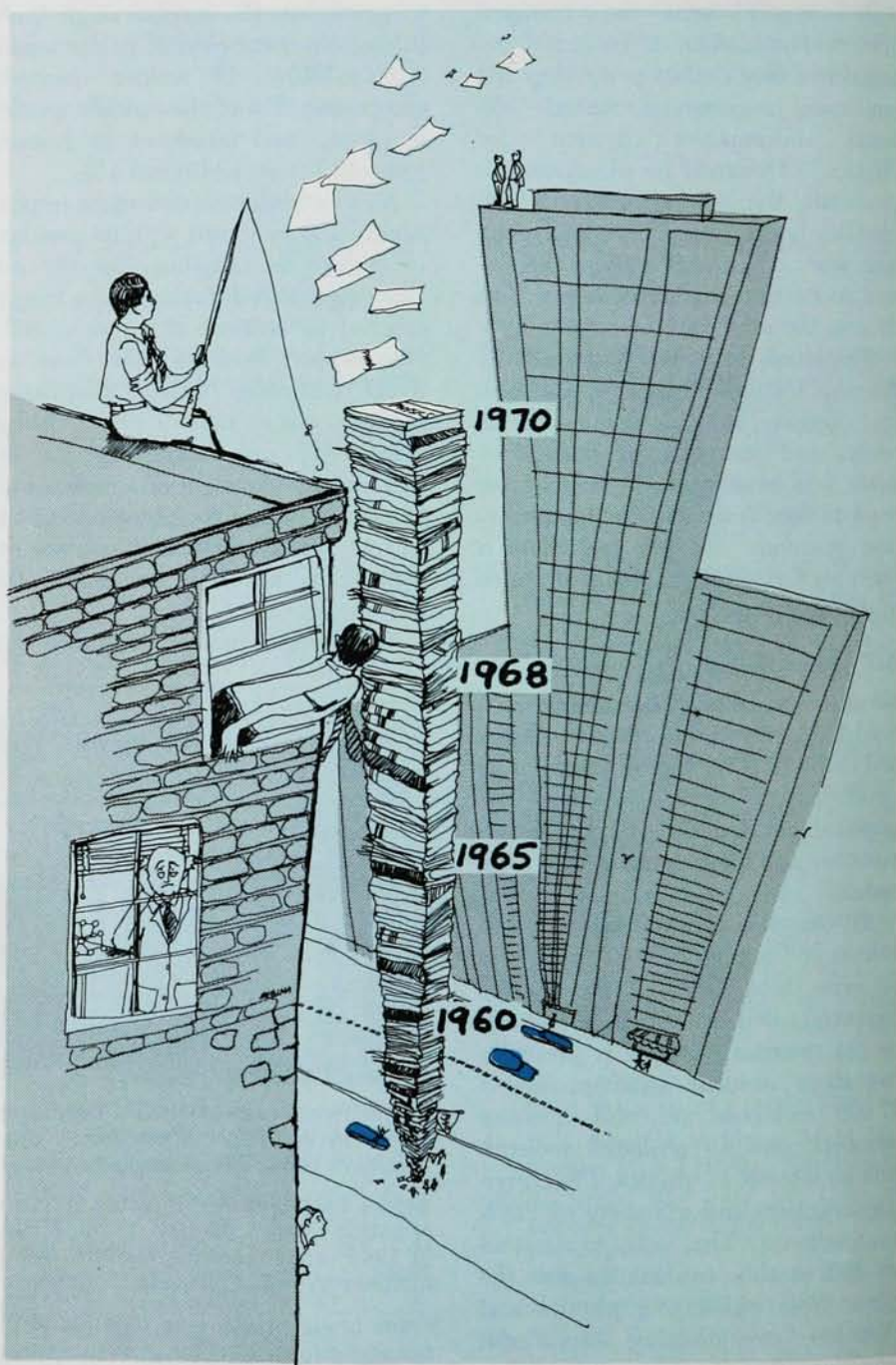
"physics almanac" that can be queried for a multitude of purposes and produces a variety of services. Specially formatted printed versions of all or part of the file can be widely distributed for ready reference. The file itself could answer specific questions both at AIP and at suitably equipped subscribing institutions.

Each item in the article record represents a "handle" that can retrieve the complete record. Thus one can ask for all articles published in a given journal or year or by a particular author or institution; papers that contain certain specific words in their title or abstract or that cite another paper or

have a number of citations in common with a given paper; and finally, papers about a particular kind of physics.

### Classification scheme

The classification procedure that the system as a whole will use was developed by AIP in coöperation with outside physicists specializing in various branches. It is a procedure for writing a formatted statement of what, objectively, a paper is about. The box on page 31 is an example and shows how the classification number is constructed using "verb expressions," represented as integers, followed by "nouns," represented as decimal num-



"THE READER IS LEFT TO COPE with the flood as best he can . . ."



bers. Typical verb expressions are: "the subject of primary interest is . . ." and "the method used is . . ." and "the host or environment is . . ."

The more digits in a numerical noun, the more specific its meaning. For example ".2" means *particle*, ".28" means *hadron*, ".282" means *baryon*, ".2821" means *nucleon*, ".28211" means *proton*.

This particular way of "spelling" nouns has the advantage of exhibiting the word roots. Suppose, for instance, you want papers on hadrons. All nouns beginning with the digits ".28" belong to the class hadron. Knowing this, a request can easily be formulated. The same request, in clear language, would require a specification of all words included in the class hadron (meson, pion, kaon, barvon, hyperon and nucleon). In the example (right) the title is less explicit, from the viewpoint of information retrieval, than is the classification number.

### What the file will do

One of the principles underlying the design of the system was that it should evolve gradually; not only will its coverage of physics literature be increased step by step, but also its services will become more sophisticated in stages. Thus we can improve the system as we go along.

The services that will be offered during 1970 and early 1971 are all straightforward products of the information file:

- **Current Physics Titles (CPT)**, a current-awareness journal, initially in four sections that probably will be: particle, field and nuclear physics; atomic, molecular, chemical, plasma and fluid physics; solid-state physics; and optics, acoustics, astrophysics and geophysics. We expect the sections will be published every other week, with each section representing a print-out of the accumulated records since the previous issue. The records will be arranged, under a new system of headings, in as many places as physicists would expect to find them. The journal will be produced through computer-controlled photocomposition, so that it will be of high typographic quality.

- A series of specialized bibliographies in several of the narrower fields of physics (updated periodically) as well as indexes for the primary journals published by AIP

- **Searchable Physics Information**

### CLASSIFICATION EXAMPLE

For the paper "Evidence of Quarks in Air-Shower Cores"  
0.1; 1.271; 2.9534; 4.24; 6.29

|        |                                              |
|--------|----------------------------------------------|
| 0      | The document type is . . .                   |
| 0.1    | experimental;                                |
| 1      | The subject of primary interest is . . .     |
| 1.2    | particle physics,                            |
| 1.27   | more precisely, a particle property,         |
| 1.271  | specifically, its existence;                 |
| 2      | The method used is . . .                     |
| 2.9    | a technique,                                 |
| 2.95   | more precisely, a particle technique,        |
| 2.953  | still more precisely, a detection technique, |
| 2.9534 | specifically, track visualization;           |
| 4      | The entity of primary interest is . . .      |
| 4.2    | a particle,                                  |
| 4.24   | more precisely, a hypothetical particle;     |
| 6      | The host or environment is . . .             |
| 6.2    | particles,                                   |
| 6.29   | more precisely, cosmic rays.                 |

*Notices (SPIN)*, a magnetic-tape service that will allow organizations with adequate computer facilities to establish their own current file of physics information. The tapes will be issued monthly and will contain the records accumulated since the previous issue. The subscribing institution could use its own search programs or specifically designed AIP programs.

At a later date, the system will offer:

- **File searches based on requests.** This service would be of particular value to scientists writing reviews or data compilations. Considering the importance of this activity in evaluating and distilling the literature into a more meaningful and digestible form, additional means for encouraging the production of such articles are also being planned.

- **Lists of articles tailored to the needs of groups of physicists working in specialties and who do not have local facilities for using the magnetic-tape (SPIN) service, as well as procedures for subdividing journals into packages that would better suit the needs of smaller interest groups.**

- **Microform copies of the primary articles, as a backup to CPT and SPIN.**

### Long-range prospects

We expect to improve the system on a continuing basis, rendering services as effectively, and as inexpensively, as possible. As a long-range prospect, we hope to offer a centralized service, with decentralized satellites, that

would cope with all the information needs of the physics community as well as those of the broader national scientific and technical community for physics information.

It would offer reference services and would obtain copies of hard-to-get material and refer questions it can not answer to sources that could. The system would also afford direct on-line access to the computer file from remote-access terminals in physics departments and other institutions. The centralized service also would have facilities for "scholars in residence," to supply clerical and reference aid for review writers.

Such a centralized facility would be linked into a network of "information centers" at various institutions and of similar facilities for other disciplines and for physics in other countries. This organization, with its information file and its broad spectrum of services, linked into a network of other information centers and services would constitute the "National Information System for Physics."

### Value and cost

Each potential user must determine for himself what a service like this is worth. How many hours per week do you spend looking for information? How many hours would you save if you only had to look through one booklet, a short list or a response on your computer terminal? How much time would you save if the article was in one small collection or a numbered



entry on a reel of microfilm that your librarian or secretary could copy?

These questions raise a number of imponderables. To put them into better perspective, consider that each published article represents about \$60 000 worth of research investment;

costs about \$500 to be published and will cost about \$15 to process and enter into the proposed physics-information file. The distribution price for listing that article in CPT will be only a fraction of a cent per copy. If, say, one out of a hundred articles is inter-

esting to you and one out of a thousand important, would it be worthwhile for you to have it pinpointed? Similar considerations apply to the other services. Is a system that could accomplish these things worth the cost?

The initial cost of development and pilot operation is being funded by the NSF, as part of its nationwide support of information services in scientific disciplines. Figures 1 and 2 show NSF expenditures for these purposes, both in absolute magnitude and in relation to total research support. Is it in the national interest to have the NSF support these programs? In 1968 NSF spent about \$14 million for information activities in various scientific disciplines—less than 10% of the total was for physics. The improvement of efficiency in physics research and development activities is clearly in the national interest. The saving of a fraction of an hour per week by each of the 30 000 physicists in the National Register of Scientific and Technical Personnel, not to mention all the other users of physics information, would more than make up for all of the costs borne by the NSF.

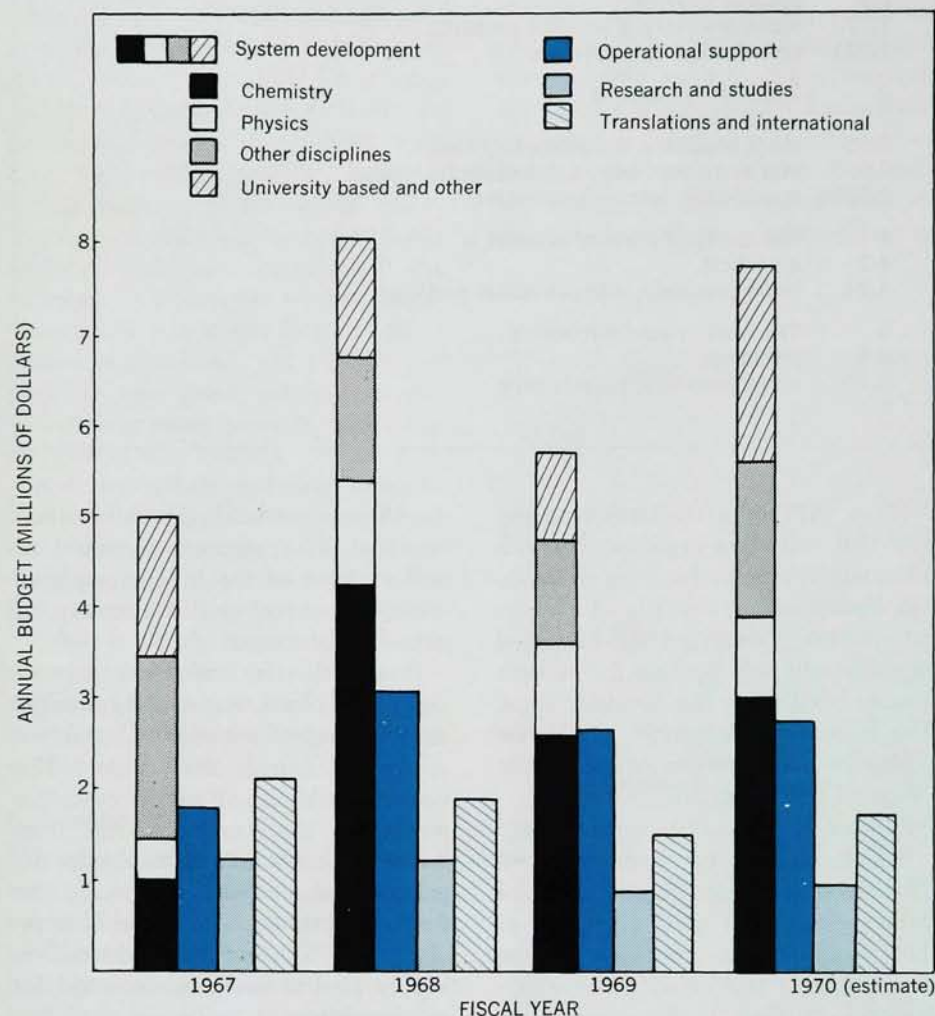
In the future many of the operating services are expected to be self-supporting after the requested funding period ends in 1973. Some of the newer services would still need subsidies, and funds would still be required for further development of longer-ranged projects. The rate of NSF support, however, would probably decrease, and the additional cost would be offset by the greater values of the ultimate system.

### A question

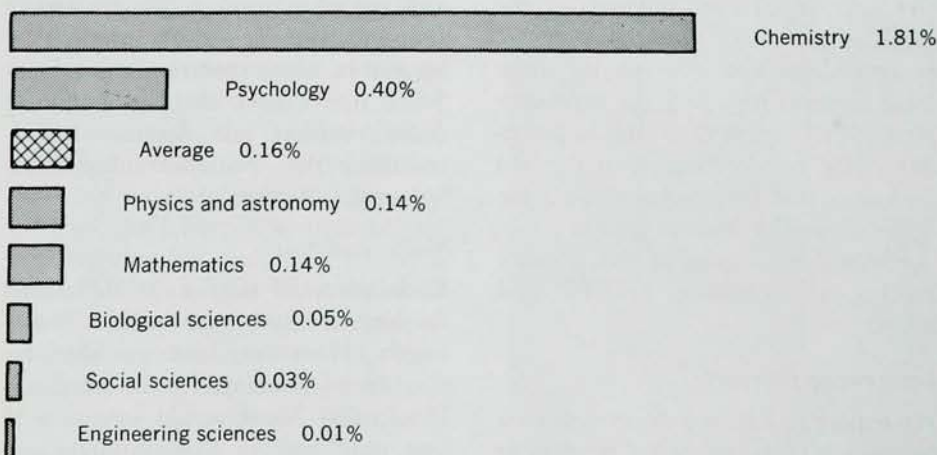
This program has been endorsed by the AIP governing board, which represents the member societies. But considering the magnitude of this undertaking, we would like the additional opinions of individual physicists: What do you think of our proposed system? Please write us and give us the benefits of your views and ideas on this matter.

### References

1. V. Z. Williams, E. Hutchisson, H. C. Wolfe, *PHYSICS TODAY*, 19, no. 1, 45 (1966).
2. H. W. Koch, *PHYSICS TODAY*, 21, no. 4, 41 (1968).
3. *A Program for a National Information System for Physics*, American Institute of Physics, publication no. 1D69R (August 1969). □



**COST OF INFORMATION SYSTEMS.** Chart compares NSF support of information programs by type and discipline during 1967-70. —FIG. 1



**INFORMATION VS RESEARCH COST.** Chart depicts ratio of NSF support for development of information systems to all federal support of research in the fiscal year 1968. The high percentage for chemistry is partly because most chemistry research is privately financed. —FIG. 2



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# PLASMAS

This "fourth state of matter" offers an immense variety of physical phenomena. Applications are tentative, but surprisingly widespread.

HAROLD GRAD

THE TENOR OF OUR TIMES is receptive to a very young science that claims dominion over 99% of the matter in the universe, proposes to fuel a cross-country auto trip with the deuterium from one gallon of sea water, offers to replace the magic of catalysis in polymer chemistry with precise knob turning, promises to alleviate the pollution problem by instant vaporization of waste and garbage, ventures to propel space ships and essays a role in cosmology. Even though these specific future applications of plasma physics are not proven, the potentialities of plasma, the "fourth state" of matter, are difficult to overstate.

Without regard to applications, the wealth of physical phenomena encountered in the plasma state exceeds the variety spanned by substances as diverse as air, water, peanut butter and superfluid helium. I will not presume to give a balanced picture of this explosively developing field. Instead, I present here some of the flavor of the subject through a few topics of personal interest and familiarity, binding it together by an overall evaluation of where we are and where we may be heading; at the conclusion are listed some complementary articles of general interest.

The frontiers of the subject are, in a word, everywhere. Despite a phenomenal growth in theoretical understanding and in experimental control of plasmas, there are almost daily revelations and discoveries of new and unexpected fundamental insights, frequently overturning our most cherished beliefs.

In common with nuclear physics we

hear echoes in plasmas; in common with superfluid helium we observe not only second sound but also third, fourth, and more; in common with gas dynamics, we find shock waves and turbulence, both in bewildering variety; in common with crystallography we find anisotropy, but in much more exaggerated form; in common with all other fields, plasmas display waves, but in an unprecedented assortment of types, packets and interactions.

One branch of physics, for example superfluidity, is catalyzed by the discovery of an unexpected natural phenomenon. Another, say fundamental-particle physics, explores unknown territory simply "because it is there" and will uncover unusual phenomena as a matter of course. Plasma physics lies closer, in spirit, to the latter. Unexpected and unfamiliar phenomena are abundant, and each discovery opens a new subfield. Yet no evident single focus unifies the subject other than our desire to discover what we can about ionized and conducting matter. Whether the conceptual unity hoped for in fundamental-particle physics will ever overtake plasma physics is doubtful. Certainly the basic qualitative principles that govern plasma behavior are not yet established. Even so, the initial dust cloud is beginning to settle, goals are taking shape, measurements are becoming reliable (see figure 1), and practical means of answering questions are beginning to emerge.

## *What is a plasma?*

A plasma is any electrically conducting medium whose electrical properties are sufficiently pronounced to react back

on an external field. There is no end of materials that fit this description. Plasmas are found in the ionosphere, in the solar wind, within the sun, and on reentry from space; within the laboratory, we have hot hydrogen plasmas and replicas of the sun, also relatively cool gas discharges and alkali plasmas; other plasmas occur in semiconductors, in polymer chemistry, and in metals, both liquid and solid. These diverse substances are related by many qualitative and even some quantitative features: plasma oscillations, Alfvén waves, the concept of magnetic flux carried with the flow, and so on. Nonetheless, even one of these distinct types of plasmas possesses a vast range of parameters and exhibits an awesome variety of qualitatively different properties.

For example, the major experimental programs that are currently considered





to be directly relevant to the controlled-thermonuclear goal deal with hydrogen plasmas at temperatures ranging over a factor of  $10^3$  and densities over  $10^6$  (see table on page 36). The comparison of air with water, which is only  $10^3$  times as dense, or water with a white dwarf, which is only  $10^5$  times as dense, or superfluid helium with atmospheric helium, which is only  $10^2$  as hot, leads us to expect similar large differences in the properties of plasma states separated by so many orders of magnitude.

One of the important plasma parameters is  $\beta$ , the ratio of plasma to magnetic-field energy density. A factor of  $10^5$  separates the values of  $\beta$  found in hot-plasma research. Thus we can expect all theoretical and experimental problems—orbital, equilibrium, diffusion, stability, injection, heating, impurity control—to be five orders of

magnitude apart. Different phenomena dominate plasma behavior in high- and low- $\beta$  plasmas; the technology, the diagnostic tools, the theoretical models, even the basic qualitative intuitions, are quite distinct.

#### Plasma parameters

A primitive but important clue to the qualitative types of phenomena that are likely to be encountered is given by the values of key dimensionless parameters. As a fluid, air behaves more like water, at similar Mach and Reynolds numbers, than slowly moving air behaves like hypersonic air. Similarly, macroscopic MHD (see box on page 37) theory may be adequate for very different solar and laboratory plasmas in comparable *scaled* parameter ranges. However, instead of two basic parameters as in classical dissipative fluid dynamics (Mach and

Reynolds), two in ordinary kinetic theory (Mach and Knudsen) and two in ideal MHD (Mach and Alfvén) we have *seven or more* in standard, fully ionized, plasma physics.

Crudely subdividing the range of each parameter into small, medium and large, we can expect  $3^2 = 9$  qualitatively different regions to cover fluid dynamics (potential, boundary-layer, hypersonic, turbulent flow, and so on), and  $3^7 = 2187$  regions to cover plasma physics in a comparably crude way. Entirely different physical phenomena will arise depending on the relative

**LASER INTERFEROGRAMS of Scylla IV, showing plasma compression and loss out of the ends, at 2.4, 3.6, 4.9 and 6.1 microsec. The number of fringes is proportional to density. —FIG. 1**





values of lengths such as the electron radius, collision cross section, mean distance between electrons, Debye length, Larmor radius, mean free path. Different phenomena will also arise that depend on the frequencies obtained by combining these lengths with thermal speed, or Alfvén speed, or speed of light, not to speak of interference and resonances with each other and with independent externally imposed geometrical lengths, excitation frequencies, and speeds. The high dimensionality of this parameter space is the key without which we cannot begin to understand the structure of plasma physics. Our goal is not to find *one* theory of plasma behavior but to find very many theories of the behavior of many different plasmas.

### Medium versus geometry

A considerable amount is known about shock waves. In ordinary kinetic theory of an unionized gas a single parameter (the shock strength or Mach number) completely determines the profile of a plane shock wave. The corresponding steady plane shock wave in a fully ionized plasma takes six dimensionless parameters to specify its profile. In the special case of a weak shock propagating perpendicular to the magnetic field, the profile depends only on  $\beta$  and the Hall factor  $\omega\tau$ , in addition to the mass ratio  $\alpha^2 = m_-/m_+$ . One might expect the mass ratio to be an unessential parameter, because it is always small, but the limit as  $\alpha^2$  approaches zero is quite singular. It is most easily



Harold Grad, since 1958 director of the Magneto-Fluid Dynamics division and professor of mathematics at the Courant Institute of Mathematical Sciences, New York University, lists his research interests as plasma physics and controlled thermonuclear fusion, kinetic theory, statistical mechanics and fluid dynamics. He is retiring chairman of the plasma-physics division of the American Physical Society and a former chairman (1963) of the division of fluid dynamics.

Typical Hot Plasma Parameters

| Device                                              | Density<br>(ions/cm <sup>3</sup> ) | Ion temperature<br>(KeV or 10 <sup>7</sup> K) | Confinement<br>time | $\beta$ |
|-----------------------------------------------------|------------------------------------|-----------------------------------------------|---------------------|---------|
| 2X (Lawrence Radiation Laboratory) Mirror with well | $5 \times 10^{13}$                 | 8.0                                           | 1 millisecc         | 0.1     |
| T3 (USSR) Tokomak                                   | $5 \times 10^{13}$                 | 0.5                                           | 20 millisecc        | 0.002   |
| Scylla IV (Los Alamos) Theta pinch                  | $4 \times 10^{16}$                 | 5.0                                           | 3 microsecc         | 0.8     |
| Focus (Los Alamos) Coaxial gun*                     | $2 \times 10^{19}$                 | 6.0                                           | 0.1 microsecc       | high    |
| DCX-2 (Oak Ridge) Mirror                            | $5 \times 10^9$                    | 500                                           | 0.5 sec             | 0.001   |
| Stellarator C (Princeton)                           | $3 \times 10^{13}$                 | 0.15                                          | 1 millisecc         | 0.0001  |
| Centaur (Culham) Cusp-ended theta pinch             | $10^{16}$                          | 0.3                                           | 5 microsecc         | 0.99+   |

\* Volume of plasma is about 0.01 cm<sup>3</sup>.

surveyed by taking  $\beta \approx \alpha^r$  and  $\omega\tau \approx \alpha^s$  for a variety of values (positive and negative) of  $r$  and  $s$ . Each  $r,s$  region shown in figure 2 represents qualitatively different behavior; the shock thickness is dominated by a different dissipative mechanism such as ion viscosity, electron Hall heat flow, and so on. The transition regions, combining two or more mechanisms, are more complicated. To obtain all this information must surely have taken scores of man-years of calculation! Fortunately this wealth of physical information, representing the asymptotic solution of a pair of Boltzmann equations (ions and electrons) and Maxwell's equations, is given by an explicit, though very complicated, algebraic formula. Unfortunately we cannot expect other problems to yield explicit solutions of such generality.

As we have mentioned, the number of parameters for more general (finite strength, oblique), but still classically collision-dominated shock profiles goes up from three to six. Only a very small part of this parameter space has been investigated. More seriously, entirely new dissipative mechanisms, involving a host of instabilities, turbulence and so on, enter with greater shock strength.

The shock problem is posed for an infinite medium with no boundaries or geometrical features; its complexity, in different parameter ranges, arises entirely from intrinsic plasma properties. To isolate plasma from geometrical complications we turn to the opposite extreme of the simplest plasma model, ideal static MHD. There is now only a single plasma parameter,  $\beta$  (instead of up to seven with more realistic models). But as soon as we try to

contain the plasma, geometrical complications enter.

For example, consider containment in a stellarator. In its more complex forms this concept may involve separate curved and straight sections, each with a different helical winding, or several superposed helical windings on a common circular axis. The simplest stellarator has a circular axis and a single, symmetric, periodic helical winding; to describe it requires four lengths and three field parameters for a total of at least six dimensionless parameters. But from the limited theory that is available, we find that the simplest MHD model is sufficiently fertile to reveal qualitatively different physical behavior in different corners of the six-dimensional parameter space. Most of this space is still terra incognita.

The shock and stellarator examples just given illustrate plasma and geometrical complexities respectively. Some idea of the possible interplay between physical and geometrical effects in plasmas can be obtained by a glance at classical fluid dynamics where much more is known. One description of a fluid is by a dispersion formula, say  $\omega^2 = k^2 a^2$  where  $a^2 = \partial p / \partial \rho$  is the speed of sound. Hidden in this trivial formula for an ordinary gas are the theory of the organ pipe, lift and drag, all of diffraction theory, and the transition from wave to ray optics. Spatial variation of  $a^2$ , through its dependence on density and temperature, introduces refraction, transonic flows, shocks, implosions and explosions, all sorts of waves (gravity, ship, and tidal), breakers and bores, wakes, cavitation, bubbles, and so on. Viscosity complicates the dispersion



## GLOSSARY

**MHD**—ideal, nondissipative, macroscopic magnetofluid dynamics

**guiding center**—small Larmor radius orbit (and collective plasma) approximation

$\beta = 8\pi p/H^2$ —ratio of plasma to magnetic pressure (or energy)

**z-pinch**—cylindrical plasma column with  $I_z$  and  $B_\theta$

**$\theta$ -pinch**—cylindrical plasma column with  $I_\theta$  and  $B_z$  ("Pinch" originally referred to a transient; now it refers also to static equilibria.)

**Q-machine**—alkali plasma (originally hoped to be "quiescent")

**Tokamak**—toroidal z-pinch; flux surfaces formed by plasma current

**stellarator**—toroidal; flux surfaces formed by external windings, usually helical

**multipole**—usually toroidal configuration with internal conductors, either supported or levitated

**banana**—drift surface (see figure 9b)

**loss cone**—part of phase space from which an orbit will eventually escape

**Bohm diffusion,  $D_B$** —an arbitrary unit,  $\frac{ckT}{16eB}$

visible in the dispersion formula and are found only in finite geometries with boundaries.

In an infinite homogeneous plasma the formula that describes propagation of small-amplitude plane waves, corresponding to  $\omega^2 = k^2 a^2$ , has been studied extensively but by no means exhaustively. It is a transcendental relation involving the ion and electron velocity-distribution functions, and it exhibits an infinite number of dispersive and anisotropic modes as well as many continua. In principle each plasma mode could ramify as widely as all of classical fluid dynamics in a real geometry. Taking fluid dynamics as our model, we expect that *most of the basic qualitative plasma phenomena will be discovered only as nonlinear and finite geometry effects*, not directly visible in the dispersion formula. Only in the simplest plasma models, such as MHD, guiding center, and magnetoionic theory, is there an appreciable corpus of nonlinear- and finite-geometry plasma effects. Some geometrical effects appear as rather direct generalizations of classical effects; an example is the Fresnel zones, which are essential to the description of excitation and detection of ion-acoustic waves. Other effects such as coupling of different linear modes through boundary conditions or through variable density of the medium are more peculiar to plasmas.

We should not leave the impression

that plasmas are always more complex than their neutral counterparts. As a possible counterexample we point to fluid turbulence, which is a strongly nonlinear and essentially three-dimensional phenomenon, only slightly related to fluid instability. We can compare it with the motion of a bouncing ball on a cobblestone street and the unrelated facts that the top of a stone is unstable and a pot hole stable. Plasma, with its greater variety of waves and interactions and spectral complexity, allows an entirely different type of *weak turbulence* in which nonlinearity can be handled as a quasi-linear perturbation. We also have strong plasma turbulence, which is likely to remain essentially empirical.

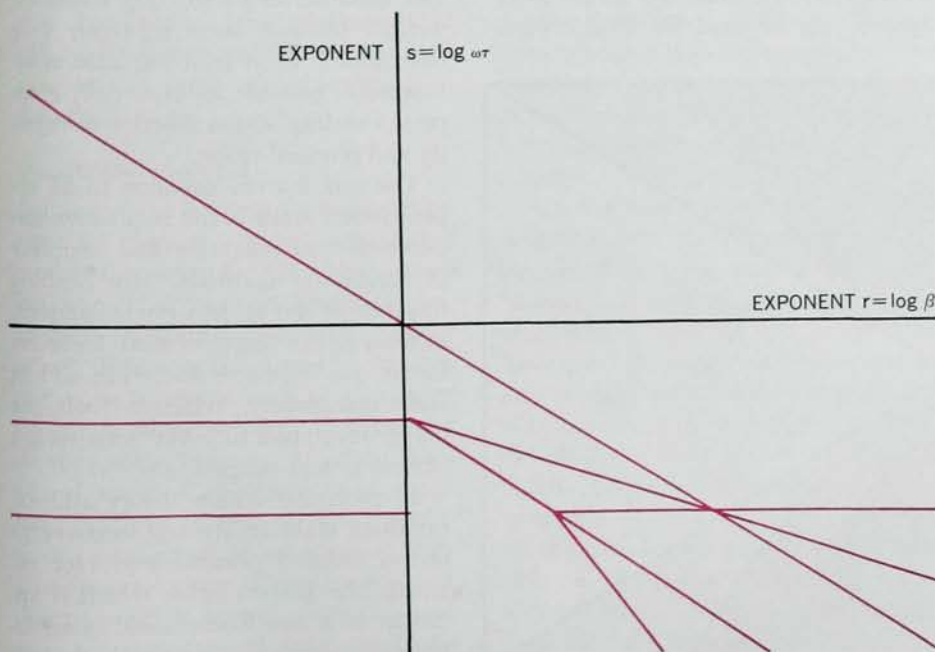
To return to the question "What is a plasma?", we can only say that we are just beginning to find out.

### To catch a hot plasma

Although nature is always exceedingly complex, physics gains its strength precisely by rejecting complexities as they occur in nature in order to study selected, isolated "basic" phenomena. "The unreasonable effectiveness of mathematics in the natural sciences"<sup>1</sup> results from natural selection of isolated phenomena—both experimental and theoretical—as the subject matter of science. The basic goal of experimental plasma physics is the construction of experiments, each of which isolates an individual phenomenon, in enough variety to cover qualitatively the entire field. This is a long-term project, but the multiplicity of effects is not the most serious road block. Before we can study a plasma, we have to catch one. For a hot plasma, this stipulation conflicts with the best scientific sequence. To create a hot plasma and keep it away from the walls long enough for study requires complex experimental procedures and complicated geometries that conflict with the desire to isolate individual phenomena. In a contained hot plasma the scientific problems are presented all at once rather than in sequence. Analysis of complex systems is commonplace in engineering, but *not* when the individual phenomena have not yet been scientifically explored.

The relatively high degree of understanding of mirror plasma, compared with toroidal plasmas, is probably a result of the dominance, in mirrors, of a single identifiable physical mechanism. The mirror configuration is characterized by extreme anisotropy

formula only slightly, but it introduces an assortment of boundary layers, sedimentation, and all of meteorology and oceanography. We see that *almost all* the interesting qualitative physical phenomena in a classical fluid are not



**DISSIPATION MECHANISMS** in a weak shock. Each region represents a different dissipation mechanism as a function of the values of  $\beta$  and  $\omega\tau$  relative to the mass ratio. From P. N. Hu, Phys. Fluids 9, 89 (1966). —FIG. 2



and a pronounced loss cone (see figure 3). The two principal containment problems are scattering into the loss cone and plasma instability. Both the basic scattering loss mechanism and the basic loss-cone instability can be studied analytically in an infinite homogeneous medium without boundaries, and therefore with a high degree of theoretical reliability. "Finite-geometry" complications can be adjoined afterwards as relatively minor perturbations of the basic phenomena that do not drastically change the qualitative picture.

Only recently have we discovered that toroidal systems, in addition to their own peculiarly toroidal difficulties, possess most of the problems of mirror machines. Loss cones emerge in many forms in a toroidal system, but less obtrusively than in a mirror. Many classes of mirroring (trapped) orbits appear with all their attendant problems, but they do not dominate. In all but the simplest toroidal geometries, anisotropy appears in an essential and complex way, but again in a weakened form. In even the geometrically simplest closed configuration (Tokamak), the field topology enters significantly. It appears more than likely that the reason we do not yet understand the limitations of toroidal confinement is that there are so many comparable competing effects, not that a single elusive effect remains to be discovered. Thus far every simple mechanism has been proved to be inadequate. Synergistic combinations are beginning to be explored. Empirically the simplest

configuration (Tokamak) is also the most successful. Perhaps recent recognition of the complexity of the toroidal-containment problem will turn out to be the single most important step towards its ultimate resolution.

The rapid growth of technology and empirical experience in containing hot plasmas (see table on page 36) has also made itself felt in accelerating the discovery of basic physical phenomena. For example, in a situation that is not atypical of containment experiments, the plasma found in the DCX-2 apparatus is quite different from what is injected, and it is contained by incidental fine structure in the applied mirror field. But the plasma is quite uniform and has served as an excellent medium for basic studies of finite Larmor radius and extremely anisotropic effects (including, ultimately, the discovery of the mechanism for the origin of the plasma).

There has also been a large recent development of relatively low budget, nonthermonuclear plasma experiments insensitive to wall isolation. From the point of view of basic physics, the two classes of plasmas are sufficiently distinct to require pursuit of both.

One example of a basic phenomenon not intimately tied to containment is the plasma echo (to be discussed later). Another example is the interaction of plasma oscillations and optical emissions. The fluctuating electron density modulates light emission at the plasma frequency. This effect, first observed in the ionosphere, can be used for atomic mea-

surements of excited-state lifetimes or for plasma diagnostics.

The relatively well developed field of alkali plasmas is, in some respects, a bridge between the physics of contained and noncontained plasmas.

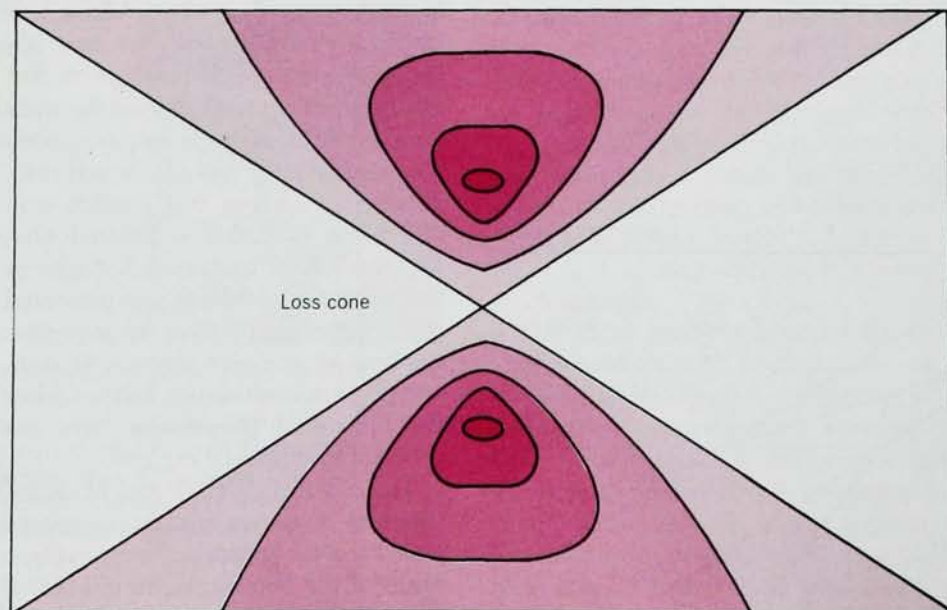
### Where is the frontier?

The goal in plasma physics, as in the study of any fluid such as air, water, or liquid helium, is to understand and to control it—to pump, to compress, to heat and extract energy, to propagate waves, to measure and, above all, to keep it from leaking.

In view of the ramification of the subject, it is not surprising to find that progress is not uniform over all of plasma physics. At the frontier, open questions range from determination of an equation of state in one type of plasma to highly specialized effects dominated by details of the geometry and distribution function in another. In highly condensed plasmas, such as very high-pressure alkali (classical) or solid-state (degenerate-electron) plasmas, elementary thermodynamic and transport properties are the immediate problem, both experimentally and theoretically. In moderately dense  $\theta$ -pinches, macroscopic equilibrium and stability questions on a microsecond time scale are the most urgent present concern. In some well documented mirror-contained hot plasmas the spectrum of identified phenomena is much broader, and we have consistent theoretical and experimental information about large classes of waves and instabilities in strongly non-Maxwellian plasmas on a relatively long time scale. In at least one case, measurements provide an essentially complete ion-distribution function in velocity and physical space.

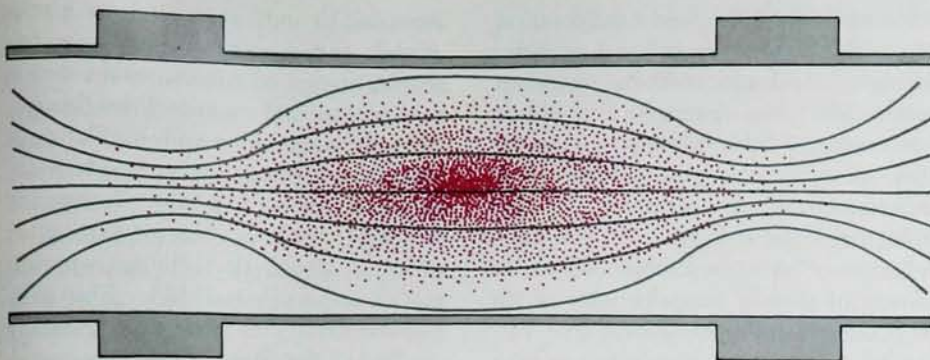
The one feature common to all experimental areas is the impressive improvement in reliability and flexibility of diagnostic methods. By pushing the state of the art in x-ray techniques, in charge-exchange neutral measurements, in Thomson scattering and in laser holography, selective tools are being developed to cover wide ranges of density and temperature.

In problems where theory and experiment make an attempt to converge on an isolated phenomenon (for example, the plasma echo, which is approximately one dimensional in an infinite homogeneous medium), there is very good agreement between the two. In containment geometries, where experimental flexibility is severely



DENSITY CONTOURS in velocity space for a typical mirror-machine loss-cone distribution. Darker color represents greater density. —FIG. 3





MIRROR-MACHINE configuration, typical of open-ended devices.

—FIG. 4

limited, the agreement is not nearly as good.

The factors that limit currently operating mirror machines and  $\theta$ -pinches are fairly well understood, but scaling to new parameters is not at all certain. In stellarators and Tokomaks even the present limiting factors have not been identified, and scaling is unknown. Multipoles lie somewhere in between.

The hallmark of a clean physics experiment, isolation of a specific phenomenon, is just as much a necessity for an effective theory. Even in pure theory it is not easy to isolate an effect by fiat, just by adding or dropping a term. As in experiment, the long-term goal of plasma theory is to find and develop an arsenal of models and diagnostic techniques that are capable of separating out different effects. The development of sophisticated theoretical diagnostics has lagged somewhat behind that of experiment. This lag may be because the tradition that valid experimental results require great care is not quite so widespread in theoretical work.

### Qualitative concepts

Because of the enormous complexity of plasma physics, rough qualitative models take on more than their usual importance. Where do the qualitative, intuitive concepts that bind a field arise? If history is any guide, they do not come from synthesis of masses of experimental or theoretical data; nor do they come from use of crude theoretical analyses of complicated problems (such as an analysis of a containment configuration). Rather they arise from solutions of simple problems that turn out to be more accurate and more representative of the general case than one could reasonably expect.

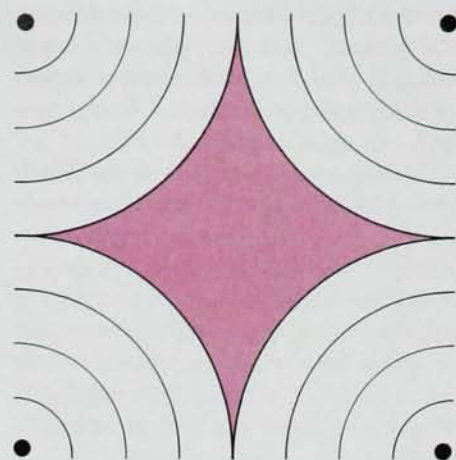
It is illuminating to consider fluid dynamics as a prototype of a well de-

veloped subject. The simplest fluid model, incompressible irrotational flow, is hardly realistic. But every aerodynamicist expends considerable effort to develop a strong intuition about this nonexistent fluid. He describes actual flows as deviations from this ideal (in boundary layers, shocks, and so on). Without a precise knowledge of these deviations, potential flow has very little value; with this knowledge, it is priceless. Without the aid of the ideal theory, the more exact viscous theory would also have very little value, simply because of its complexity. Despite great advances in theory and in computing capability, we still solve the full Navier-Stokes equations only in elementary geometries. Fluid dynamics thus exhibits a complex, symbiotic relation among its subtheories, with the whole greater than the sum of its parts.

The ideal plasma concept of "frozen" flux, carried with the plasma, has a similar significance. Although it is almost always a poor approximation, one can hardly carry on a sensible discussion of plasma containment or motion without this concept as the starting point.

On the other hand, qualitative descriptions do not always help. A simple illustration of the pitfalls of semantic analogy is given by the diamagnetic properties of a plasma. This concept clearly requires quantitative modification, because a plasma is a rather complex substance. But the extent of this modification is surprising.

The simplest evidence for the diamagnetic effect is the current carried by the circular orbit of a charged particle in a uniform magnetic field. Similarly, in a nonuniform but unidirectional field, the macroscopic equilibrium pressure balance,  $p + B^2/2 = \text{constant}$ , indicates that the plasma depresses the field strength. In a more



CUSP GEOMETRY with opposed Helmholtz coils. This is the prototype magnetic-well geometry.

—FIG. 5

complex geometry the orbits become complicated, and the static pressure balance is anisotropic. The guiding-center approximation to the orbits is closely tied to the diamagnetic image, because it assigns to each particle a constant (negative) magnetic moment. The plasma current perpendicular to  $B$  is the sum of a definitely diamagnetic contribution from the magnetic-moment density and a current arising from the drift of guiding centers across the field. The latter component, frequently called the "diamagnetic drift," can easily have a paramagnetic sign. When paramagnetic it can even dominate the contribution of the magnetic moment and create a locally paramagnetic region in the plasma.

One can be more precise with a special class of guiding-center equilibria that yields an exact mathematical analog of a classical nonlinear magnetic medium ( $B$  is a function of  $H$ ). For this anisotropic equilibrium, the pressure components,  $p_{\parallel}$  and  $p_{\perp}$ , are constant on  $|B|$  contours. Taking  $\mu \equiv B/H$  as the definition of permeability, we find the criterion for a paramagnetic region,  $\mu > 1$ , to be  $dp_{\parallel}/dB > 0$  (or  $p_{\parallel} > p_{\perp}$ ). The alternative definition,  $\mu \equiv dB/dH$ , yields  $\mu > 1$  wherever  $dp_{\perp}/dB < 0$ . These criteria are not intuitively evident. But with either definition, locally paramagnetic and diamagnetic regions can be found easily.

More striking than the existence of local paramagnetic regions is the possibility of a fully self-consistent plasma equilibrium that is globally paramagnetic (for example, in a simple mirror or cusp field, figures 4 and 5). In other words, the inductance of the



external coil is increased by introducing plasma. Because plasma current along  $\mathbf{B}$  complicates the interpretation of the diamagnetic effect, I have given only examples in which  $J_{\parallel} = 0$ .

The special case quoted, in which the plasma can be unambiguously identified as diamagnetic or paramagnetic, is also one in which stability is easily determined. In contrast to the classical result that a diamagnetic solid is stable in a well, each of the four combinations, diamagnetic or paramagnetic plasma in a well or on a hill, can be either stable or unstable.

One plain conclusion is that, in competition between the elegance and simplicity of a concept and the complexity possible in a plasma, complexity can usually be expected to triumph.

### Psychological roadblocks

Another example of an appealing but somewhat specious qualitative concept is that of a *magnetic well*. Basically, we expect a plasma (diamagnetic!) to be stable in a well. The original magnetic-well formulation (1955), for a plasma with no internal magnetic field, separated at a sharp surface from a vacuum field, gave the necessary and sufficient condition for MHD stability that the magnitude of  $B$  increase everywhere from the plasma surface. An immediate consequence was that no plasma with a smooth boundary, mirror or toroidal, can be stably contained; only the cusped geometries (for example, figure 5) are stable. This qualitative stability principle was dramatically demonstrated experimentally, in 1960, by applying cusped coils to the very unstable pinch.

A much more significant experiment from the point of view of thermonuclear confinement, was M. S. Ioffe's in 1962; he showed that cusped coils reduced fluctuations and improved containment in a mirror. But, although this was a landmark from the point of view of containment, the physics was (and is) not clear. The mirror-contained plasma is strongly anisotropic, and its boundary is not a flux surface. The mechanism for the initial fluctuations and high loss rate has not been definitely identified. Nor do we know the reason for the improvement after application of the well, because its imposition has implications for several classes of micro as well as macro stability and also for the equilibrium drift-surface topology.

Among the theoretical instabilities

affected by well-like field configuration are interchange, drift, trapped particle, resistive, local and modified negative mass. Each one depends on a different magnetic-field criterion. In addition, there are several related (but different) well-like properties of the field that have a bearing on the containment of individual orbits and phase mixing of plasma imperfections rather than on any collective property. And finally there are cases where application of a magnetic well is detrimental for containment.

To summarize, the magnetic well may be ten of the most important plasma-containment concepts, but it is not just one! It is one thing to synthesize and coordinate; it is another to obliterate essential differences.

Another example of a non-concept is the term "Bohm diffusion." As a diffusion coefficient, the Bohm value is the product of thermal speed and Larmor radius. The Bohm time can also be obtained as the length of time required for a drifting ion or electron to pass once around a minor circumference. As the term is used, Bohm diffusion does not refer to a phenomenon or to a mechanism but to a natural plasma time scale that can arise in many ways, both collective and non-collective. There are easily a dozen different physical mechanisms that can give rise to loss rates comparable to Bohm. Their semantic synthesis into a single concept is artificial and a degradation of information.

### Echoes, shocks, and phase mixing

Dissipation appears in a time-reversible theory in the guise of phase mixing; analytically, it is recognized as a continuous spectrum. The basic point is that any finite or infinite discrete sum,  $\sum a_n \exp(i\omega_n t)$ , oscillates indefinitely; an integral,  $\int a(\omega) \exp(i\omega t) d\omega$ , can, however, decay. The most important qualitative feature of a continuous spectrum is that it preserves much of the information fed into the system by initial and boundary data and gives rise to much more complex phenomena than a discrete normal mode, which is primarily a property of the medium. The fact that "Landau damping" is not universally given by Landau's formula and that the wave preserves initial and boundary data has long been recognized theoretically and has recently come to the fore with experimental observations of echoes and various "ballistic" or free-flow effects.

Phase mixing with collisionless

damping is not restricted to kinetic models but is also found in macroscopic theory of Alfvén waves and in cold plasma and magnetoionic theory.

In ordinary air, a wall oscillating at a fixed frequency  $\omega$  gives rise to a disturbance  $\exp[i\omega(t-x/v)]$ . Integrating over a Maxwellian velocity distribution gives a signal that damps approximately as  $\exp[-(\omega x)^{2/3}]$ . This collisionless decay has been experimentally confirmed for high frequency waves in argon. The damping is reduced at lower frequencies, and when the wavelength exceeds the mean free path and collective behavior dominates over ballistic, the wave eventually approaches an undamped ordinary sound wave.

Exactly the same phenomenon holds in a plasma, except that the collective effect of the charge-separation field enters much more strongly than that of collisions—at the Debye length instead of at the much larger mean free path. Landau damping describes the electrostatic modification of free-flow collisionless damping and is valid for wavelengths not smaller and not too much larger than the Debye length.

Only within recent years has more accurate theory delimited, and very careful experiment been able to confirm Landau's more than 20-year-old formula. At the same time, "non-Landau" damping effects, such as echoes and ballistic effects in ion-acoustic and other waves, are also being observed.

To obtain a spatial echo, two parallel grids are excited at different frequencies. Any nonlinear coupling of the two disturbances  $\exp[i\omega(t-x/v)]$  and  $\exp[i\omega'(t-x'/v)]$  will produce a signal  $\exp[i(\omega-\omega')t + i(\omega'x'-\omega x)/v]$ . The phase mixing disappears and the modulated signal is regenerated as an echo at a position such that  $\omega'x'-\omega x = 0$ .

Space-resolved electron-plasma echoes have been observed, as have time-resolved ion-acoustic echoes produced by asynchronism from plasma gradients. The magnitude of the echo is being used as a sensitive measure of the collisional dissipation and of the dissipation resulting from externally imposed noise between initiation and echo, as in the nuclear-magnetic-resonance effect.

Collisionless shocks are also a manifestation of phase mixing, but in a more complex nonlinear version. In most laboratory shocks that are identified as collisionless, the dissipation is presently attributed to an instability



or to turbulence. In some of the more elaborate theories, a structure involving two or more distinct instabilities in sequence is invoked. For example, a steep wavefront with large electron current density induces a two-stream instability. The instability produces thermalization only in the direction of the current; the resultant unstable anisotropic distribution induces further thermalization.

In principle, neither instability nor turbulence is needed to effect irreversibility in a collisionless model. The earliest collisionless shock models, antedating experiment, were laminar; they involved phase mixing of ion orbits and "collisions" of particles with the electric and magnetic fields.

Although the first reliable collisionless shock measurements were of the earth's bow shock, laboratory experiments have recently become quite reliable in several collisionless regimes. Present understanding is largely empirical, based on the introduction of ad hoc "anomalous" collision frequencies into a theoretical calculation to achieve an experimental fit. Definite identification of irreversibility mechanisms remains open, because the most reliable theory is for weak shocks whereas most experimental data is for strong shocks. The particular value of a shock wave in heating a plasma is that the plasma itself chooses which is the most efficient irreversible mechanism under the given circumstances.

#### Factors in containment

In the physics of hot plasmas the problem we must face first is containment. This problem can be split into a study of orbits, equilibrium, stability and diffusion. They are all interrelated;

in particular, knowledge of particle orbits is used everywhere. But only the most primitive approximations of the highly developed orbit theory can be used quantitatively in the more difficult self-consistent plasma application. Nevertheless, we shall see that even the most sophisticated orbit results give very important *qualitative* information about all these subjects.

The logical sequence for a study of containment is that given above: first individual orbits, then self-consistent equilibrium, then stability and diffusion. In particular, poor containment can follow as easily from orbit and equilibrium considerations as from instability. This prescription has been moderately well followed in mirrors, taking into account the dominant role of the loss cone. Although the emphasis in toroidal investigations has long been on microinstability (in particular, drift and resistive), it has, somewhat belatedly, become clear that these mechanisms should defer in priority to the more basic questions of orbits, self-consistent equilibrium, and macrostability, all of which are inadequately understood. Especially on the present time scale of high-beta experiments, it is quite unlikely that microphenomena are important.

The distinction between "macro" and "micro" instability is not a question of the theoretical model but a distinction between an instability that moves the plasma bodily to the wall and one that exhibits small-scale fluctuations. The containment effect of the latter is usually described as "enhanced" diffusion. Either type of instability can be tolerable or catastrophic, depending on the time scale. The basic time of growth of a small-

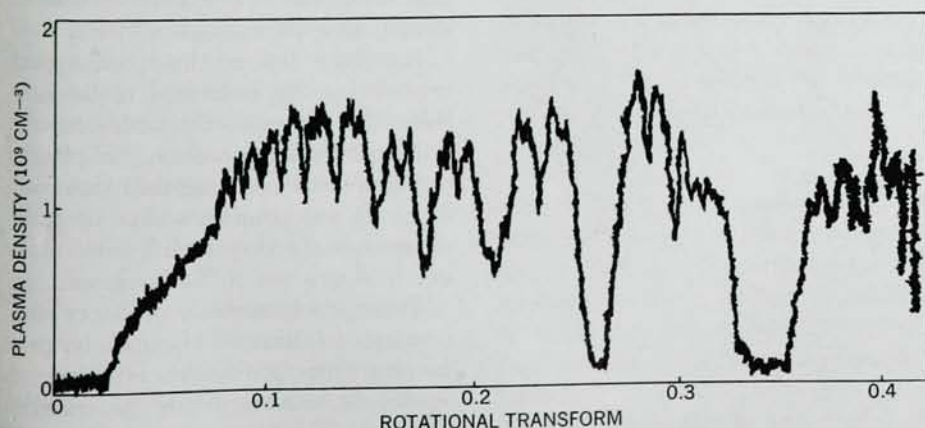
amplitude disturbance is rarely a measure of the importance of the instability. The two-stream instability, for example, exhibits extremely fast growth but is self-limiting, saturating at a low amplitude of fluctuation that preserves the velocity profile at a marginally unstable shape.

Exactly the same distinction should be made between a localized failure of microequilibrium and global macrodisequilibrium. For example, it was the impossibility of gross pressure balance in the simplest toroidal field that led to the invention of the original figure-eight stellarator. But there are also strictly local failures in maintaining a self-consistent plasma-field equilibrium. These failures can lead to anomalously high currents, irreducible fluctuations propagating as Alfvén waves, and enhanced losses.

Moreover, even in macroscopic MHD stability theory, it is only by making distinct separation between local and global instability that we can establish some points of contact between theory and experiment.

The one property that is unique to toroidal containment is *closure* (as distinguished from curvature, which can be mimicked in open systems). Magnetic lines carry all sorts of information: electrostatic potential, Alfvén waves, guiding-center orbits, and so on. In an open system, information is exchanged with the outside world in both directions along a magnetic line, whether intentionally or not. In a toroidal system, the information remains inside; there are specific plasma complaints that are neither sensed nor easily remedied. An example is resonance effects in magnetic surfaces and particle orbits, which have long been known from experience with accelerators. But recently *collective* plasma closure effects have been predicted and also observed. Figure 6 shows the observed dependence of equilibrium plasma density on rotational transform in a carefully designed stellarator. Distinct peaks are found for fields with resonances up to the 15th order (that is, a field line closes after 15 circuits the long way round the torus). Because the mean free path is less than one circuit of the torus, this observation can not be an orbit effect. A possible explanation of this effect is in terms of microequilibrium. Selective mathematical diagnostic methods that may allow comparison with experiment are gradually being developed.

Instability has had a much more in-



PLASMA DENSITY in the Wendelstein  $l=2$  stellarator as a function of the rotational transform. There are strong resonances where the field lines close after 3, 4, 5 . . . 15 . . . times the long way round the torus.

—FIG. 6



tensive development than equilibrium. The proliferation over the years of theoretical microinstabilities is itself characteristics of an explosive instability. There are recent signs of saturation. This can not be ascribed to the hypothesis that most instabilities are already known, because as we have already pointed out, only an infinitesimal part of the totality of gross qualitative plasma phenomena has yet been examined. What is a more likely explanation of the microinstability slowdown is discouragement, as only a small fraction of the list of theoretical instabilities has been identified experimentally.

### Equilibrium

The difficulty of attaining plasma equilibrium can be seen by a glance at figure 7, which shows a typical particle orbit in a stellarator magnetic field. As an indication of how an equilibrium configuration might appear, recall that a fixed value of the distribution function must be assigned to each orbit in phase space (this requirement is prior to any strictures of

self-consistency). It is clear that any equilibrium distribution function is very complicated, to say the least. More careful study shows that it is, in many cases, mathematically impossible. Even a Maxwell demon could not inject the plasma correctly. In a real plasma we must expect to find a certain irreducible level of fluctuations— independent of any question of stability. When we add self-consistency, we find that the number of special situations that allow time-independent solutions is even more restricted.

There is, of course, no reason other than mathematical convenience to look for stationary states. But without this convenience, the whole of conventional stability theory, based on perturbations about an assumed equilibrium, evaporates! When faced with the collapse of a theory one usually argues that something has been left out—finite Larmor radius, Debye radius, resistivity, and so on. But further study in this case shows that the only chance of resolving the crisis in containment theory lies in using cruder rather than more sophisticated models.

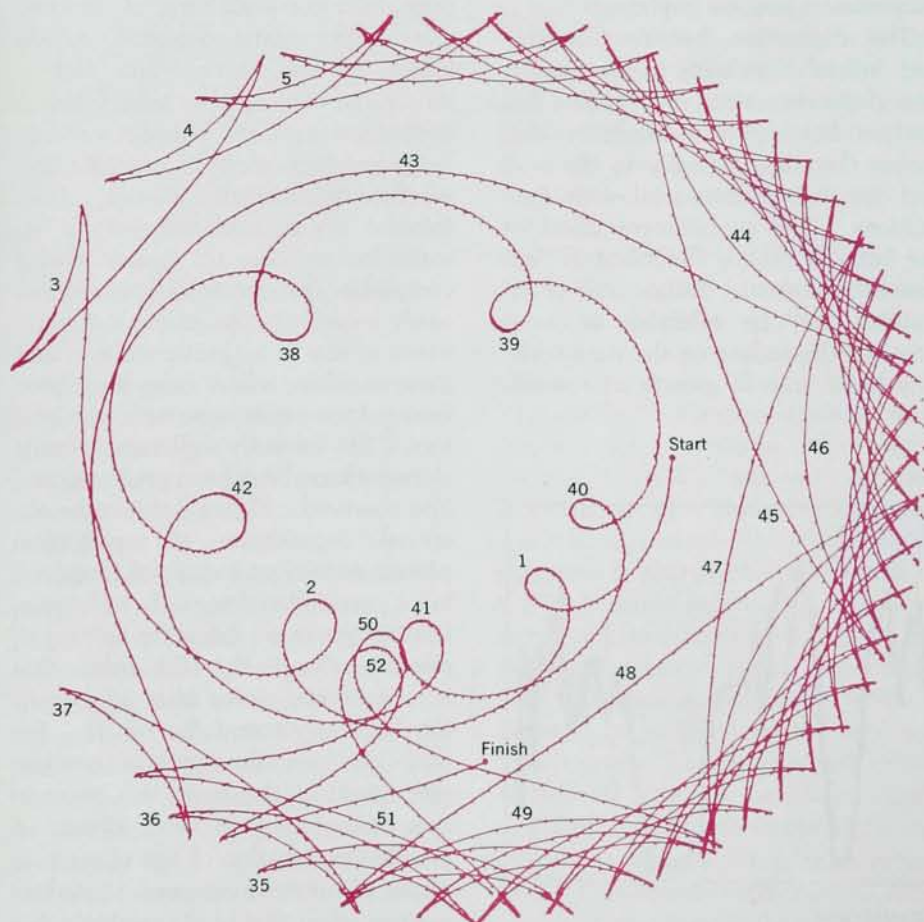
For some mathematical purposes it is appropriate to consider rational numbers as negligible, "of measure zero" compared to irrationals. But for many purposes the rationals must be considered on a par with the irrationals. For example, in a stellarator field with shear (variable rotational transform), the volume occupied by rational transform is finite. The variation of rotational transform would look qualitatively as shown in figure 8, where the flat stretches, of constant rational transform, occupy finite regions. Within these regions of constant transform, the magnetic field exhibits islands, ergodic regions, and all sorts of pathology. (The magnetic field is as smooth as you like—the pathology enters only in answer to the delicate question of what happens to a magnetic line if it is followed forever.)

With axial symmetry the magnetic field exhibits no such pathology. There are no gaps in the flux surfaces, and the rotational transform varies smoothly. But particle orbits (which can also be assigned a rotational transform) will, even in the case of axial symmetry, behave as in figure 8. In other words any time-independent equilibrium, in which a constant value of the distribution function must be assigned to each orbit, will be pathological.

Returning to an asymmetric geometry (such as a stellarator), although a field line can be very complex (for example, ergodic) in a flat region, it is contained forever between two legitimate flux surfaces. This is not true of orbits in the asymmetric geometry. There are everywhere dense (possibly thin) loss cones from which particles can escape, given enough time. A true equilibrium distribution would have the value zero on this complex distributed loss cone. The same is true of any asymmetric mirror machine.

In some cases the pathological regions can be estimated to be very thin. In this case, the additional requirement of self-consistency of plasma currents with magnetic field turns out to be an independent source of pathology, namely very high current density in a new set of "flat" regions.

There is a large body of theory concerning equilibrium, frequently presenting "proofs" of the existence of equilibria to all order in general geometries. Interpretation of such formal analytic results needs great care. They have the valuable property of being blind to certain complicated



PARTICLE ORBITS in a stellarator field, calculated by computer. The numbers are provided only to facilitate following the orbit sequence. From H. Gibson, J. B. Taylor, *Phys. Fluids* 10, 2653 (1967).

—FIG. 7



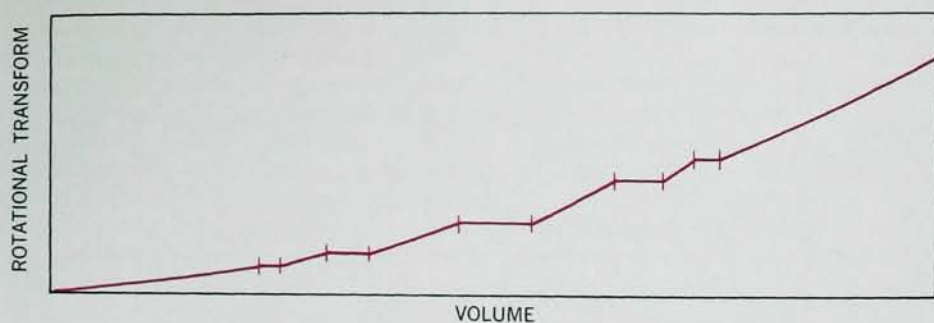
phenomena. But they remain blind whether the neglected phenomena are negligible or dominant! We need a careful mix of naive and sophisticated calculations to extract the most useful information—for example, how long does it take for an approximate equilibrium to break up? Some slight progress is being made here.

### More phase mixing

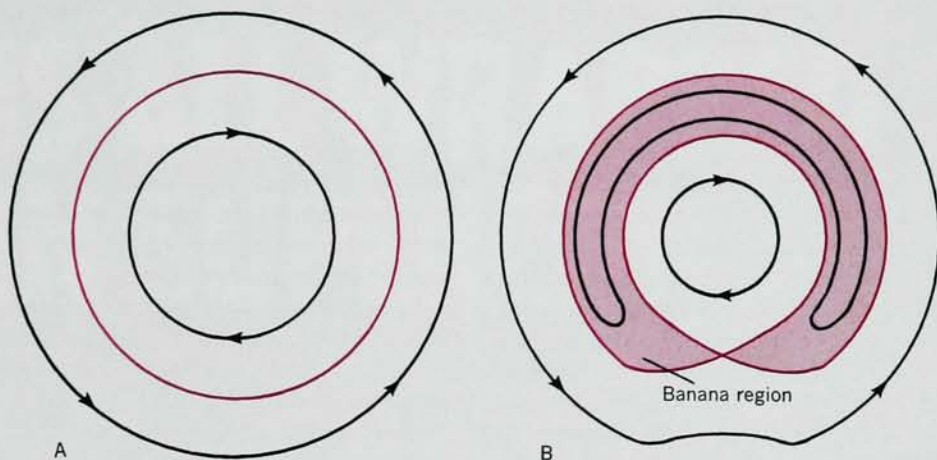
Most of the orbit and equilibrium pathology disappears when we use the guiding-center orbit approximation in a mirror machine. Moreover, phase mixing can sometimes be relied upon to correct imperfections of symmetry in the injected plasma. For example, consider an axially symmetric mirror machine. Because of field gradients, a guiding-center orbit will drift around a flux surface in a time comparable to the Bohm diffusion time. It is easy to verify that the sense of the drift reverses for particles that mirror close to the center and those that almost spill over. Phase-plane shear (variable drift speed, see figure 9a) provides phase mixing so that, if we ignore collective effects, any asymmetric plasma injection will be corrected after a number of drift periods. (More precisely, for low-energy non-drifting orbits, there is a stationary phase point, indicating poor mixing in this part of phase space.)

If axial symmetry is disturbed, the degenerate zero velocity curve in figure 9a will split, to form a drift pattern like that in figure 9b. The contours in figure 9a are level lines of a volcano, which, in figure 9b, lies on a slope. Although the shear is not zero, it is small throughout the banana region, and we can expect local errors in plasma symmetry to die away relatively slowly. During this process, local fluctuations would be observed. This poorly mixing region of phase space is the same one that is influential in creating drift instabilities, but the present phenomenon is quite distinct—in particular, it is noncollective. It is interesting to note that there are some magnetic-well configurations that do not exhibit drift reversal and banana regions.

In a mirror machine with multiple mirrors, and in most toroidal devices, there will be several trapped states, as in figure 10, each generating its own family of drift surfaces. These drift surfaces have no relation to the flux surfaces, even for vanishing Larmor radius. Since a local maximum of  $B$



ROTATIONAL TRANSFORM shown schematically as a function of plasma volume. In principle the flat portions occur at each rational value. —FIG. 8



DRIFT CURVES in a mirror machine. The direction of drift reverses as the turning point moves out. The zero-drift curve in a symmetric field (a) opens up into a "banana" region with asymmetry (b). —FIG. 9

will vary from one magnetic line to another, a drifting particle can spill and change its trapped state (see figure 11). This change produces a random walk among the drift "surfaces," which turn out not to be surfaces at all, but to cover a finite volume of phase space ergodically. Equilibration through phase mixing will probably be slow in such regions. An easy estimate shows that drift surfaces or drift volumes that touch a wall or a loss cone give a direct loss rate comparable to Bohm, without the intervention of scattering or fluctuations.

### Diffusion

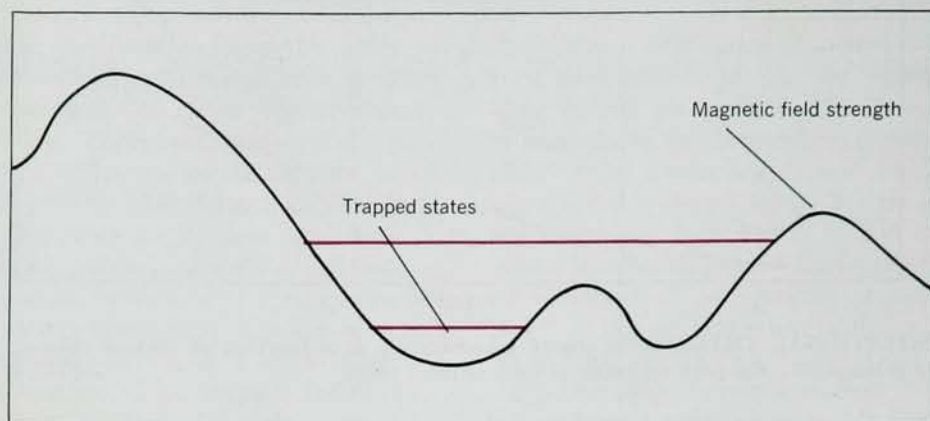
Diffusion is a very general term. It describes a variety of dissipative mechanisms that allow violation of the elementary, perfectly conducting concept of a plasma whose elements remain fixed to given magnetic lines or flux surfaces.

In the formulation as a random walk, diffusion of particles is not self-consistent. With an ambipolar calculation, it becomes slightly self-consistent.

But to make it fully self-consistent involves at least all the complexities of the self-consistent-equilibrium problem that we have briefly outlined.

The diffusion problem is both qualitatively and quantitatively different in different ranges of the collision frequency  $\nu$ . If  $\nu$  is large, we have a random walk of particles, localized in physical space. With  $\nu$  smaller than the Larmor frequency, there is a random walk of guiding centers. If  $\nu$  becomes smaller than the "bounce" frequency of reflection between mirrors, collisions induce a random walk from magnetic line to line. Still smaller  $\nu$ , comparable to the drift frequency around the machine, yields a random walk of drift surfaces (qualitatively similar to the noncollisional random walk induced by changes in the trapped state, figures 10, 11). The first two cases are considered to be classical because they can be treated macroscopically, with a plasma resistivity. Diffusion among magnetic lines or drift surfaces is frequently termed "anomalous," or nonclassical although





TRAPPED STATES. Particles in different energy states can be trapped in different mirrors in a complex mirror machine. —FIG. 10

it is a consequence of purely classical orbits and Coulomb scattering.

But even the completely macroscopic resistive model of diffusion (small mean free path) can exhibit anomalies. Macroscopic plasma diffusion across a field is a complex interaction between two more basic types of diffusion. In a mixture of neutral gases we have a diffusion coefficient,  $D_0$ . Diffusion of a magnetic field through a conducting solid is described by a coefficient  $D_M = 1/\mu_0\sigma$  ( $\sigma$  is the conductivity). The elementary kinetic-theory formula,  $\sigma \approx e^2 D_0/kT$ , suggests that  $D_0$  and  $D_M$  are essentially reciprocals! For a plasma in a simple magnetic field, the competition between these two effects at different rates gives (at low  $\beta$ ) the classical diffusion coefficient  $D_c \approx \beta D_M$ ; (the competition is fierce, and the combined diffusion equation that results is unconventional, with a nonlinear decay as  $1/t$  rather than exponential as in simple diffusion).

In more complex geometries we expect coupling between these two basic rates. This coupling is only partially accounted for in the standard theory by the Pfirsch-Schluter factor. Because this factor diverges under the same circumstances that lead to difficulties with microequilibrium, we can not consider the macroscopic "self-consistent" theory to be definitive.

Some rough estimates have been made of a slightly self-consistent model with realistic guiding-center orbits. A model that combines such drift surface (banana) diffusion with realistic self-consistency (as in the macroscopic theory) appears far in the future.

### Trends

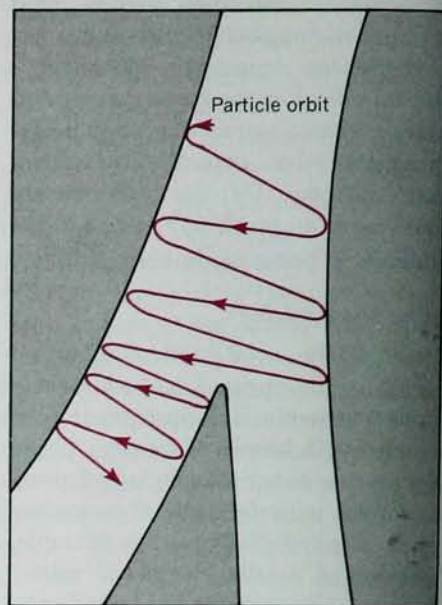
Originating in discharge physics and astrophysics, spurred mainly by the

controlled thermonuclear program in the past 15 years, plasma physics is now branching into many new directions, meanwhile developing into a recognized academic discipline.

We can grasp the significance of the field of plasma physics only in the context of its enormous phenomenological variety and—especially for hot plasmas—experimental difficulty. Growth, measured both in achievable experimental plasma parameters and in depth of understanding, is either fabulous, when compared to the state of the art a few years ago, or negligible, when compared to what visibly remains to be done. Observable trends toward simplicity in plasma experiments, toward simpler theoretical models and at the same time away from simplistic theoretical explanations are both in the right direction.

Both experimental and theoretical techniques are becoming more specific and more precise, more quantitative and more professional. A portent is the recent start, internationally, of serious engineering studies of hypothetical operating thermonuclear reactors. The significance is not that we can see a target date, but that we can imagine being caught short. The complexity of plasma phenomena implies a concomitant large variety of options; with some ingenuity, success is not in doubt. But the time scale is not easily estimated, because the scientific and technological problems that must be solved are not yet fully formulated.

Plasma physics is, in a sense, the union of three classical fields—fluid dynamics, kinetic theory, electromagnetic theory. Although classical, these are fields that have all seen profound advances in the past 20 years. In the specialities of nonlinear waves and in-



CHANGE OF TRAPPED STATE for a drifting particle. The choice of second state is essentially random. —FIG. 11

stabilities, there has been a gratifying infusion into plasma physics of ideas from electrical engineering. We should hope and expect that in a subject as vast as plasma physics, similar profit will ensue from interaction with these and other scientific disciplines with their divergent backgrounds, techniques and viewpoints.

\* \* \*

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Not so fast! say critics of the May article in which Bilaniuk and Sudarshan offered the arguments for faster-than-light particles. Their letters raise questions about causality and interactions. The original authors contribute a reply.

OLEXA-MYRON BILANIUK, STEPHEN L. BROWN, BRYCE DeWITT, WILLIAM A. NEWCOMB, MENDEL SACHS, E. C. GEORGE SUDARSHAN, SHOICHI YOSHIKAWA

"Anything that is not forbidden is compulsory," says Murray Gell-Mann's half-facetious totalitarian principle. What then about faster-than-light particles called "tachyons"? In their May article<sup>1</sup> Olexa-Myron Bilaniuk and E. C. George Sudarshan argued that valid solutions of Albert Einstein's relativity equations describe such particles. Thus if Einstein's equations are accurate descriptions of the physical universe and if solutions not forbidden are compulsory, tachyons must exist.

The May article stirred up a flurry of correspondence directed largely at two questions: Are the tachyon solutions valid? Do they have significance in our real world? From those letters we have chosen five that represent the principal viewpoints. With them we publish Bilaniuk's and Sudarshan's reply to their commentators.

## Real force, imaginary mass

The May article by Bilaniuk and Sudarshan presented a very interesting and provocative discussion of the possible existence of particles that can travel faster than light. After presenting their case, the authors pointed to several objections that have been raised against their proposal, and they showed how their own viewpoint answered these objections. Some further objections that could be raised, however, are not mentioned by the authors. I should like to discuss them in this letter.

The authors base their argument on the relationships among energy, mo-

mentum, mass and speed that follow from the mechanics of particles in special relativity theory. They point out that since both energy and momentum depend on the mass factor,  $M_0/(1 - v^2/c^2)^{1/2}$ , the conserved quantities could remain real numbers if simultaneously  $v^2/c^2 > 1$  and  $m_0$  is replaced with the purely imaginary proper mass  $im^*$ . The argument is that since energy and momentum—not inertial mass—are the observables, only these quantities must have a description in terms of real numbers.

A tacit assumption here is that the appearance of inertial mass originates in the expressions for energy, momentum, etc. But this is not actually true, according to the full meaning of relativity theory. For in Einstein's original approach, special relativity is only a special case of general relativity. (Indeed, the adjective "special" implies this fact). In general relativity theory energy and momentum are not defined quantities! The conservation laws are in fact only the asymptotic features of the general formalism in the limit of a local domain. However, inertial mass is defined here in global terms. It relates to the metrical field  $g^{\mu\nu}(x)$  through Einstein's field equations. Thus inertia is a more general property of matter than energy or momentum. The inertia of matter appears in terms of a (continuously distributed) field on the right

side of Einstein's equations. The metrical field solutions,  $g^{\mu\nu}(x)$  appear on the left side of these equations. Now if the inertial mass of any bit of matter (in the proper frame of reference) should be represented by a purely imaginary number, it would follow that the corresponding metrical field solution of Einstein's equations (in the same frame of reference) must also be represented by a set of imaginary numbers. But this would be inadmissible for several reasons. One important reason is that in the local limit, the metric tensor must approach the diagonal form  $(1, -1, -1, -1)$  that characterizes special relativity theory. The latter, of course, is a set of real numbers. If  $g^{\mu\nu}$  is represented by a set of purely imaginary numbers in its global description, it could not approach a set of real numbers in a continuous fashion under any circumstances! Physically, the continual approach of  $g^{\mu\nu}$  toward the Lorentz metric in the local domain corresponds to the diminishing effect that one bit of matter (in this case the tachyon) would have on other matter.

The gist of this argument is that the inertial mass term  $m_0$  derives from a more primitive relation than the expressions of energy and momentum in special relativity. Once the general relation that relates inertia to the global features of a physical system is found, one can take the asymptotic



limit and derive the value for the mass of a bit of matter in the local domain. Only at this stage (in principle) does one insert this parameter in the energy and momentum expressions. But the original general relation that identifies inertia with the metrical field necessarily requires that the proper mass be represented by a purely real number. In this case, the further requirement that the energy, momentum, etc., be represented by real numbers would *not* permit  $v/c$  to be greater than unity.

One further argument against the existence of tachyons has to do with the fact that one does not measure energy and momentum in any experiment; one rather measures the energy and momentum *transfer*, a *change* of energy-momentum. But a change in energy-momentum has to do with *force*—the force that causes an interaction between matter and matter and, in turn, relates to the corresponding change of state of motion of the interacting matter. Now if inertial mass relates to a measure of the resistance to the change in the state of motion of matter and if we define the force exerted by matter on matter

(the momentum transfer that is mutually exchanged) in terms of real numbers, then the mass itself must also be represented by a real number. Otherwise an imaginary-mass particle would not interact with a real-mass particle. In particular, if one part of this mutual interaction is a measuring apparatus—which we have already used to detect real-mass particles (for example, a cloud chamber)—then it should not be able to detect imaginary-mass particles.

At the root of this objection is the omission in the paper by Bilaniuk and Sudarshan of discussion of interaction between the tachyon and any other matter. But it is *essential* in this problem to introduce the description of interaction because of the necessary appearance of matter with real mass to interact with the faster-than-light particles. My argument above implies that as soon as this interaction is taken into account, the conclusion is reached that (within the framework of relativity theory) no matter described by real mass could respond in any way to the tachyon. From this point of view, then, the tachyon must remain in a theoretical domain

that is beyond the domain of physics.

My argument has been based on a look at the consistency of the tachyon description within the theory of relativity. Therefore I do not at all disagree with the attempt to find faster-than-light particles. But I do disagree with the authors' interpretation of the results of such experimentation. For if such particles should be found, I should have to conclude (in contrast with the authors' contention) that the theory of relativity would have been refuted.

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### Tachyonic Cerenkov radiation

I should like to raise one question in connection with the recent article by Bilaniuk and Sudarshan. The authors alleged that a charged tachyon, by the emission of Cerenkov radiation, would ultimately enter a "transcendent" state of infinite velocity or zero energy. However, this would not appear to be a relativistically invariant condition. An infinite-velocity trajectory is one that is orthogonal (in the space-time sense) to the time axis of one's reference frame, and it will not be orthogonal to the time axis of another frame. How can this be reconciled with the principle of relativity?

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Stephen L. Brown, who does operations research for the Stanford Research Institute, got a PhD at Purdue as a high-energy theorist.



Olexa-Myron Bilaniuk, professor at Swarthmore, was born in the Ukraine and educated in Belgium and at the University of Michigan.



E. C. George Sudarshan came from India and the University of Madras to the University of Rochester. He is now a professor at University of Texas, Austin.

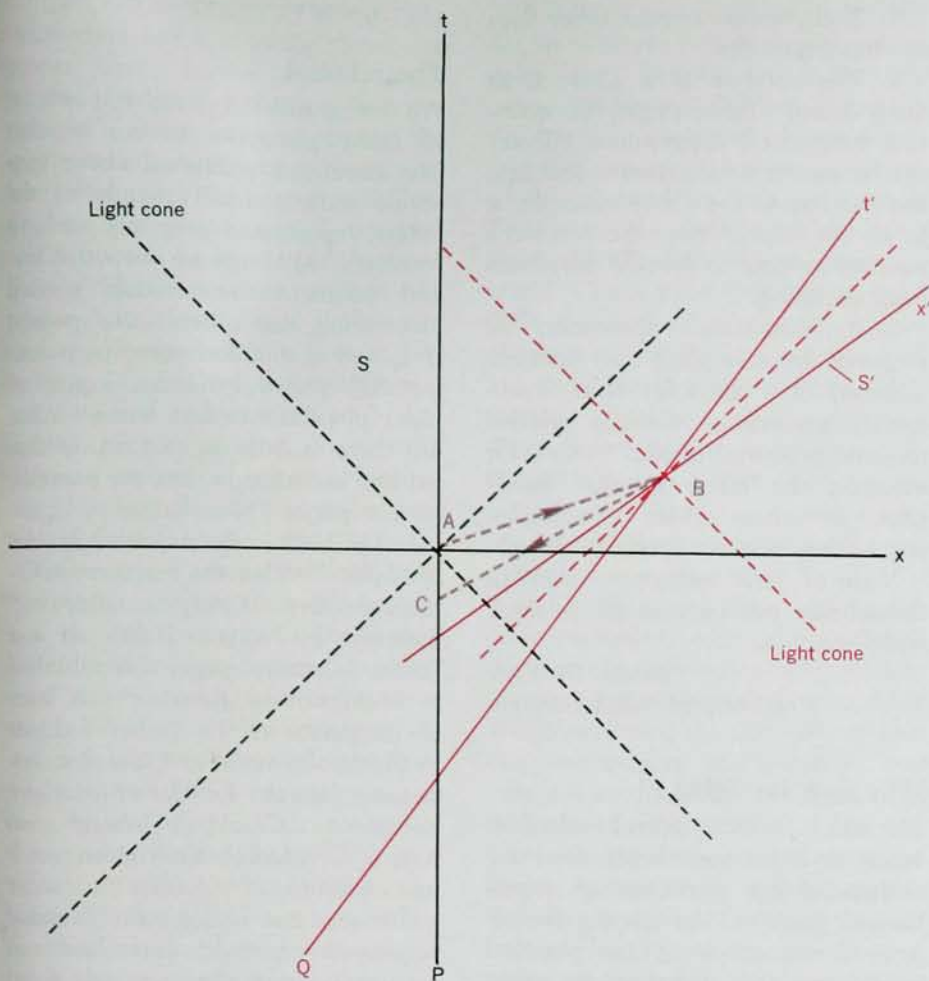


### Violation of causality

The article by Bilaniuk and Sudarshan is well written and the exposition of tachyon theory is almost perfect. This, however, permitted me to conceive the following objection: If tachyons are to be produced or absorbed by tardyons or luxons, the causality principle is not upheld. My objection does not exclude the possibility that tachyons may interact with other species in an *uncontrolled* manner. (I will clarify the uncontrolled manner in the last paragraph.)

The causality principle is to be put in the following form: If an event A causes the event C at the same location in a coordinate system S yet earlier in time (figure 1), the causality principle is violated. Whether the event C is the emission of a tachyon or absorption of a tachyon is immaterial. What I would like to point out is that by transmitting a tachyon





**CAUSALITY VIOLATION.** Effect in frame  $S$  appears to precede cause in  $S$  through signals to and from frame  $S'$  moving with respect to  $S$ . —FIG. 1

at  $t = 0$ , the observer  $P$  in the coordinate system  $S$  can induce the emission of another tachyon at  $t = -t_0$  ( $< 0$ ). This seems to me a very clear case of the violation of causality. It hardly requires any explanation. I shall sketch the argument. Observer  $P$  sends a tachyon at  $t = 0$  to another observer  $Q$  located at  $B$  on a moving coordinate  $S'$ . Observer  $Q$  then finds that a negative-energy tachyon is absorbed at  $B$ ; that is, a positive-energy tachyon is emitted in the negative  $x'$  direction. As soon as he notes the emission of this tachyon, he sends another tachyon with a faster velocity along the negative  $x'$  axis. This second particle is then absorbed by an absorber located at  $C$ . The observer  $P$  finds that a positive tachyon was emitted at  $C$  ( $t = -t_0$ ). Clearly the emission of a tachyon at  $C$  was caused by the decision of the observer at  $A$  ( $t = 0$ ). Hence, the causality principle was violated.

If we can control the interaction between a tachyon and other particles

in any way (such as blocking the motion of a tachyon), we can violate the causality principle. For example, if we let observer  $P$  pass only a tachyon with a specified velocity to reach observer  $Q$  and if we let  $Q$  allow the passage of only those tachyons faster than the first tachyon to reach  $P$ , eventually  $P$  finds a passing of a tachyon earlier in time because another tachyon with a specified velocity passes later in time.

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### Reinterpretation won't work

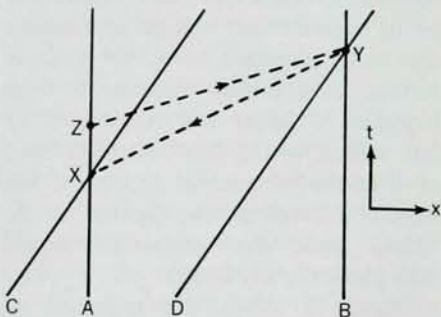
Your authors Bilaniuk and Sudarshan cannot get off the hook as easily as they pretend they can in their article. I refer to their claim that by reinterpreting negative-energy tachyons traveling backward in time as positive-energy tachyons traveling forward in time they can avoid the causality objections against the tachyon hypothesis. This is simply not true, and

it does not require sophisticated arguments or the invocation of thermodynamical irreversibility and quantum-mechanical uncertainties to prove it.

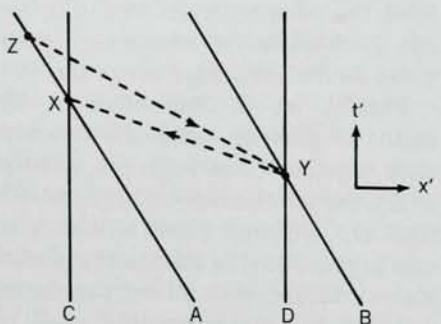
First of all, if tachyons exist, they must interact with normal matter. If they interact with normal matter, it must be possible, in principle, to produce them in a beam. Moreover, it must be possible to modulate this beam at the source, and hence to send a directed signal faster than light. For purposes of the present argument it is sufficient to represent such a signal as a spacelike line in spacetime. An actual signal would be a striped ribbon since time is required both to emit it and to receive it. But if emitter and receiver are far enough apart, the width of the ribbon can be neglected.

Let  $A$  and  $B$  be two observers, both at rest in an inertial frame  $(x, t)$ . (We suppress coordinates  $y$  and  $z$  for simplicity.) Let  $A$  emit a modulated burst of tachyons at the space-time event  $Z$ , as shown in figure 2. Let this signal be received by  $B$  at the event  $Y$ . Because  $Y$  is later than  $Z$ , in the common inertial frame of  $A$  and  $B$ , both observers agree that  $A$  is the emitter and  $B$  the receiver, and that positive energy has been transmitted from  $A$  to  $B$ .

Now suppose a third observer  $D$



**IN ONE FRAME**  $Y$  comes after  $X$  and  $Z$ . Dots show tachyon signals. —FIG. 2



**IN OTHER FRAME** event  $Y$  precedes events  $X$  and  $Z$ . —FIG. 3



is passing in the vicinity of B near Y, with a relative velocity  $v$  ( $<c$ ). In figure 2 the world lines of B and D are drawn as if they intersected at Y; the intersection could actually take place a little later. Suppose that during the time of intersection (that is, while they are fairly close to one another) B transmits to D the information he has received (by way of the tachyon signal) from A, and suppose this transmission takes place by means of ordinary photons. Because photons are quite conventional carriers of information, there will again be no ambiguity about who is doing the emitting and who the receiving. On the other hand, by the relativity principle, the laws of physics must be the same for D as they are for A, and hence he will be perfectly capable of immediately sending back to A, with an identical tachyon transmitter of his own, the information he has received from B.

Since the world lines of tachyons are spacelike, there exists a range of values for  $v$ , determined by the tachyon velocity, for which the second tachyon signal appears, from the point of view of observers A and B, to propagate into the past. Suppose  $v$  is in this range. Then arguments will arise, between A and B on the one hand, and D on the other, about who is doing the sending and who is doing the receiving. To avoid such arguments let us suppose that instead of sending the tachyon signal to A, D sends it instead to a fourth observer C who happens to be at rest relative to D but whose world line intersects that of A at the moment of receipt of the signal, denoted in the figures by X. (Here again the intersection *could* take place slightly later.)

Figure 3 shows the sequence of events as viewed in the common inertial frame of C and D, denoted by  $(x', t')$ . Because event X is later than Y in this frame, C and D agree that D is the emitter and C the receiver. Since the other observers, A and B, are not involved in the transaction, their views on the subject are irrelevant.

Finally, let C transmit to A by means of photons, while the two are close together (that is, in the vicinity of X), the information he has received from D. The net result is that A is now in possession of information about his own future, with all the paradoxes that such knowledge entails.

I can think of only three ways to avoid such paradoxes:

1. Tachyons never exist other than as virtual particles.

2. The universe as a whole is so finely tuned (for example, by quantum mechanical interference effects) that whenever information is sent into the past, as in the above example, it is always wiped from the receiver's memory in time to prevent paradoxes from occurring.

3. Emission and absorption of tachyons can take place only between members of a restricted class of observers possessing velocities relative to some *preferred* inertial frame (for example, the frame of the "fixed" stars, or some other cosmological frame) less than some critical value.

None of these restrictions holds in the scheme put forward by Bilaniuk and Sudarshan.

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### Why wait for light?

The article by Bilaniuk and Sudarshan seems to me a remarkably clear exposition of the possibility of superluminal particles. In reading the article, I was struck by the practical implications that such particles might have. (I have not kept sufficiently current with the research in the subject to know whether these implications have already been discussed.)

Briefly, the argument is as follows: Class II particles (luxons) can be produced, modulated and detected by tardyon observers. The tachyon properties discussed imply that similar control could be exercised over Class III particles (tachyons), especially through the intermediation of luxons, as in the Cerenkov-detection proposal. Tachyons could therefore be used for communication systems. Such communication systems would be useful only where ordinary electromagnetic radiation is too slow, as in interstellar communication. Finally, it would seem likely that any extra-terrestrial life of high technology would be aware of tachyons (if they exist) and would use them for communications instead of waiting centuries for replies at the speed of light. Perhaps, then, the Project OZMA concept of monitoring electromagnetic radiation for intelligible patterns will turn out to have much less potential for interstellar contact than a tachyon monitoring system.

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### The rebuttal

We are gratified by the response of so many physicists to our article.<sup>1</sup> The comments published above constitute only a small sample of the letters, reports and preprints we have received. Although we knew that several points in our article needed elaboration, that others were speculative, and that a few were pure conjectures, yet we did not expect so many physicists to take notice. After all, there is little in that article that we had not already said, for example, in our paper "Meta-Relativity" published in 1962 in the *American Journal of Physics*.<sup>2</sup> Then the reaction was entirely positive. A very favorable commentary by Angus Hurst on our "Meta-Relativity" paper was published in *Mathematical Reviews*.<sup>3</sup> A team of physicists at the Nobel Institute in Stockholm undertook the first systematic search for faster-than-light particles.<sup>4</sup> Gerald Feinberg<sup>5</sup> and Arthur C. Clarke<sup>6</sup> have given excellent exposition of our ideas to a wider audience. But because the causality arguments remained unresolved and because nothing at all was said about tachyon interactions, such a favorable



reaction seemed almost too good to be true. As Bryce DeWitt puts it, we did not expect to "get off the hook that easily."

After having studied the above letters and all the other correspondence quite carefully, we are now convinced more than ever that our extension of the special theory of relativity to include superluminal particles (metarelativity) is viable and that we can satisfactorily answer all objections raised so far.

**General relativity.** Let us first deal with the point questioned by Mendel Sachs. He argues that our theory is inconsistent with the general theory of relativity. We disagree. We had pointed out that for energy and momentum to be real, the proper mass of a tachyon must be imaginary. Sachs contends that an imaginary proper mass raises difficulties regarding gravitation because gravitation couples to inertia. Let us recall that the relativistic gravitational field is



coupled to the density of energy and momentum and *not* to the density of proper mass. In the limit of slowly moving tardyons (ordinary massive particles) one can approximate the relativistic interaction by a Newtonian interaction using the proper mass density but only in this special case and in this special limit. It just happens that under these circumstances the proper mass density and the energy density are equal (apart from the  $c^2$  factor). As long as the energy and momentum of tachyons are real (that is, the proper mass is imaginary) tachyons present no anomaly regarding gravitational interactions in general relativity theory.

**Transcendent tachyons.** William Newcomb's question is quite intriguing. Indeed, a charged tachyon that has reached its zero-energy "transcendent" state in one frame still has some energy left in some other frame moving with a velocity  $w$  relative to the first; hence in that frame the tachyon can keep on radiating. This contradiction can be resolved by recalling that according to an observer in the second frame the sign of the energy and the direction of travel in time will be reversed (in accordance with the switching principle) when the tachyon



reaches a velocity  $c^2/w$  relative to the first frame. The events that lead to a transcendent tachyon in one frame look like a head-on collision and annihilation of a tachyon and an anti-tachyon in another. Thus Newcomb is quite correct in pointing out that the transcendent state would not be a relativistically invariant condition. There is nothing disquieting about this because it is not the description of events that must remain invariant when we go from one frame to another, only the laws that govern these events.

**Causality.** As we pointed out in our PHYSICS TODAY article,<sup>1</sup> causality objections against superluminal particles are by far the most subtle, and much room for reflection remains in this regard. The questions raised by Shoichi Yoshikawa and DeWitt bear this out. Both are refined versions of earlier formulations of the causality paradox. Yoshikawa follows closely Richard Tolman's original arguments,<sup>7</sup>

and DeWitt essentially parallels chapter 28 of David Bohm's monograph on relativity.<sup>8</sup> Because the earlier presentations ignored the fact that a signal traveling backward in time carries negative energy, they were incomplete and could be dismissed as such. Yoshikawa and DeWitt, on the other hand, do allow for the sequence reversal. They point out that in principle the flow of information can be opposite to the direction of travel of a tachyon beam conveying the information. This is a novel conclusion. They show that if such counterdirected information flow were indeed possible, the closed causal loop would be reestablished notwithstanding our switching principle.

In devising gedanken experiments on superluminal communication it is necessary to take very careful account of cosmological boundary conditions. While assuming the existence of certain transmitters and receivers, we may not at the same time ignore the presence of other matter in the universe. In particular we have to make certain assumptions regarding the tachyon background. The simplest assumption is that the number of tachyons crisscrossing the universe is finite. Moreover, we know that as far as tardyons are concerned, this situation would still hold for an observer in a different inertial reference frame. Such would *not* be the case for tachyons.

**Preferred frame.** To see why the case is different with tachyons, consider two pieces of equipment, one a large emitter and the other a large receiver. Let both be located in what we shall call the "standard" frame where the flux of tachyons coming from distant regions of the universe is finite. Under such circumstances, however large the detector, the number of tachyons that it will detect per unit time is finite. On the other hand, the number of tachyons the large emitter can emit is at our disposal and can be made arbitrarily large. It should be noted that as long as the observations are made from the standard frame, the above situation holds irrespective of whether the emitter-receiver system is stationed in the standard frame or whether it is carried in a fast moving rocket. Furthermore the assumption that the number of tachyons streaming into the standard frame from distant random sources is finite implies that the number of tachyons within a certain mo-

mentum range is also finite. We know that corresponding to this momentum range there exists a reference frame in which the role of emitter and receiver for tachyons is interchanged. An observer in that frame would find that as far as he is concerned there is a limit to the number of particles that can be emitted within a velocity range greater than a certain critical value but that an arbitrarily large number of such particles can be detected by a suitable piece of apparatus.

**Refutation.** Let us assume now a standard frame  $S_0$  in which the tachyon background is zero. This will simplify our arguments without any essential loss of generality. While the tachyon background in  $S_0$  is zero, the observer  $P_0$  can still emit any number of tachyons of any velocity  $v > c$ . For another observer  $P_1$  moving with a velocity  $w < c$  relative to the standard frame this situation implies the impossibility of his emitting tachyons with a velocity greater than a certain threshold velocity  $u_1 = c^2/w$ . Instead he will see a flux of tachyons with velocities  $u > u_1$  streaming into his receiver every time he activates it. This is so because an arbitrary number of tachyons can be emitted by  $P_0$ . Every time  $P_1$  activates his receiver (which is an emitter for  $P_0$ ), it will register incoming tachyons. Conversely,  $P_1$  will not be able to use his emitter (receiver of  $P_0$ ) for sending tachyons with a velocity  $u > c^2/w$  towards distant regions of space because doing so would mean that observer  $P_0$  would register tachyons coming from infinity every time he opens his detector; such an action is contrary to our assumption that no tachyons from distant sources exist for the observer in the standard frame. In dealing with the causality paradoxes it is not necessary to assume that one of the observers is at rest in the standard frame. But by referring to this frame, we can determine which of the signals of the vicious causal cycle can not be sent. In other words, irrespective of the state of motion of the emitter, only those signals that carry information *and* energy in the same direction as seen in the standard frame are possible. Under such circumstances *no causal loops could arise* and no "anti-telephone," such as proposed by Gregory Benford, David Book and William Newcomb,<sup>9</sup> could be built.

The above suggested resolution of the refined causality arguments cor-



responds to the third way by which, according to DeWitt, causality paradoxes can be avoided. *It is in no way incompatible with our generalization of the special theory of relativity.* However, a question that may be in order is whether the assumption of existence of a preferred frame, such as  $S_0$  above, is consistent with the postulates of special relativity. After all, is not the exclusion of a preferred frame what relativity is all about? *No, it is not.* The postulates of special relativity require the laws of physics, including the speed of light, to be the same in all inertial frames. They do not preclude the existence of cosmological boundary conditions that permit us to single out a particular local frame as a preferred reference system. For example, the frame of reference in which the cosmic 3-K black-body radiation is isotropic could be considered a preferred frame that can be distinguished from all other frames.

**Other avenues.** The approach we suggest above is by no means the only way by which hypothetical superluminal particles can be reconciled with the logical requirements of the causality principle. For example, Raymond Fox, Charles G. Kuper and Stephen G. Lipson<sup>10</sup> attempt to accomplish this by extending the method of Arnold Sommerfeld and Léon Brillouin.<sup>11</sup> Another simple, if somewhat brute force, solution is offered by Ray Skinner<sup>12</sup> who simply postulates that negative-energy energy-momentum transfers must be unsuitable for signaling.

Although it is not our feeling that any radical changes in physical concepts are necessary to accommodate the tachyon hypothesis, there are some serious physicists who shrug off the causality objection by simply saying, "So what?" Roger G. Newton<sup>13</sup> and Paul L. Csonka<sup>14</sup> are doing precisely that. They feel that no precepts of logic would be violated if the temporal order of cause and effect were sometimes reversed. Whichever approach will ultimately prove the best, we are convinced that causality objections offer no compelling grounds for desisting from further theoretical and experimental work on metarelativity.

**Acausal experiments.** This assertion is particularly true of searchers for "single events" for which the causality arguments, such as raised above and elsewhere,<sup>9,15</sup> are irrelevant. An excel-

lent example of this type of experiment is the search for the reaction  $p + p \rightarrow p + p + T$  (tachyon) which Bogdan Maglic, James Norem, Howard Brody and their collaborators have told us they propose to carry out at the Princeton-Penn accelerator. In some other frame this reaction may appear as  $p + p + T \rightarrow p + p$ . Since data to be recorded by their missing-mass technique<sup>16</sup> pertain to tardyon channels only, this type of experiment would reveal the presence of tachyons without forcing them to disclose the direction of their path in time. [The experimenters are placing their proton detectors at 120 deg, whereas the maximum angle for protons from the  $p + p \rightarrow p + p + X$  (real-mass particle) reaction is 90 deg. Only tachyons having a proper mass between 0.5i and 3.5i GeV could lead to emission of protons in the 120 deg direction.] Providing the experiment is not thwarted by unexpected background problems, Maglic and his collaborators hope to be able to infer the existence of tachyons even if the cross section for their production is as small as  $10^6$  times smaller than that for the  $p + p \rightarrow p + p + \pi^0$  reaction. An earlier  $p + d \rightarrow \text{He}^3 + X$  missing-mass search for tachyons,<sup>16</sup> also initiated by Maglic, was inconclusive because the cross section for production of  $\text{He}^3$  turned out to be extremely low (about  $10^{-34}$  cm<sup>2</sup> at 3 GeV). Maglic holds out much more hope for the  $p + p \rightarrow p + p + X$  reaction.

Other experiments unaffected by causality objections include the bubble-chamber search by Charles Baltay and collaborators<sup>17</sup> for the reactions  $K^- + p \rightarrow \Lambda + T$  and  $p^- + p \rightarrow \pi^+ + \pi^- + T$  (we use  $p^-$  for antiproton), and the search for the reaction  $\pi^- + p \rightarrow n + T$  that Michael Kreisler tells us he is carrying out. In some other frame these reactions may look like  $K^- + p + T \rightarrow \Lambda$ ,  $p^- + p + T \rightarrow \pi^+ + \pi^-$ , and  $\pi^- + p + T \rightarrow n$ , respectively.

**Superluminal physics.** We are very much encouraged by imaginative suggestions such as that of Stephen Brown above and that of John W. Rhee,<sup>18</sup> but we prefer to withhold our comment on them until tachyons actually have been detected and their properties are better understood.

In conclusion we wish to say that we are pleased to see our sentiments echoed in a comment to us from Iwo Bialynicki-Birula to the effect that the concept of faster-than-light

particles is not really that unorthodox. He reminds us that all concepts of nonlocal interactions in field theory imply the existence of some agent carrying the interaction over space-like distances and thus nonlocal field theories have implicitly assumed the existence of some sort of superluminal entity. Notwithstanding questions of causality, we hope to have shown<sup>2</sup> that the special theory of relativity can be consistently generalized to accommodate faster-than-light particles.

By way of encouragement to all those working or contemplating work in the field of superluminal physics let us quote the adage coined by David Farragut at Mobile Bay: "Damn the torpedoes; full speed ahead!"

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### Transmitting the changing scene

This girl's picture was produced on a special Picturephone® system; it will never look like this in your home. The white areas mark the only picture points which changed in 1/30 second (the duration of one video frame). The remainder of the picture was blanked out.

This emphasizes how Picturephone use differs from ordinary television: the Picturephone camera usually points at a single scene throughout a call and most of the motion is confined to the subject's lips and eyes. Everything else—perhaps 90 percent of the picture—remains stationary.

Frank W. Mounts of Bell Laboratories used this fact to design an experimental video system that may

make it possible to transmit three Picturephone calls over a channel that otherwise could carry just one.

An ordinary Picturephone system sends thirty complete pictures each second. In Mounts' experimental system, only changes from one picture to the next are transmitted. A complete picture (information about dot positions and brightnesses) is stored at both the transmitting and receiving ends. As the camera's electron beam scans the original image, the brightness at each point is compared with the stored value. Whenever there is a significant difference, the system updates the stored frame and transmits the new brightness level and dot position.

At the receiving end, as the

picture tube's beam arrives at each point, the incoming information is checked to see whether a picture-point revision has arrived. If so, it is displayed and stored.

Because some areas of the picture do not change, while others change extensively, revised points may come in bursts. Transmitter buffers smooth the flow by reading the information out onto the line at a constant rate.

This new technique, one of several now being investigated at Bell Laboratories, promises to help keep transmission costs down when the Picturephone service becomes generally available.

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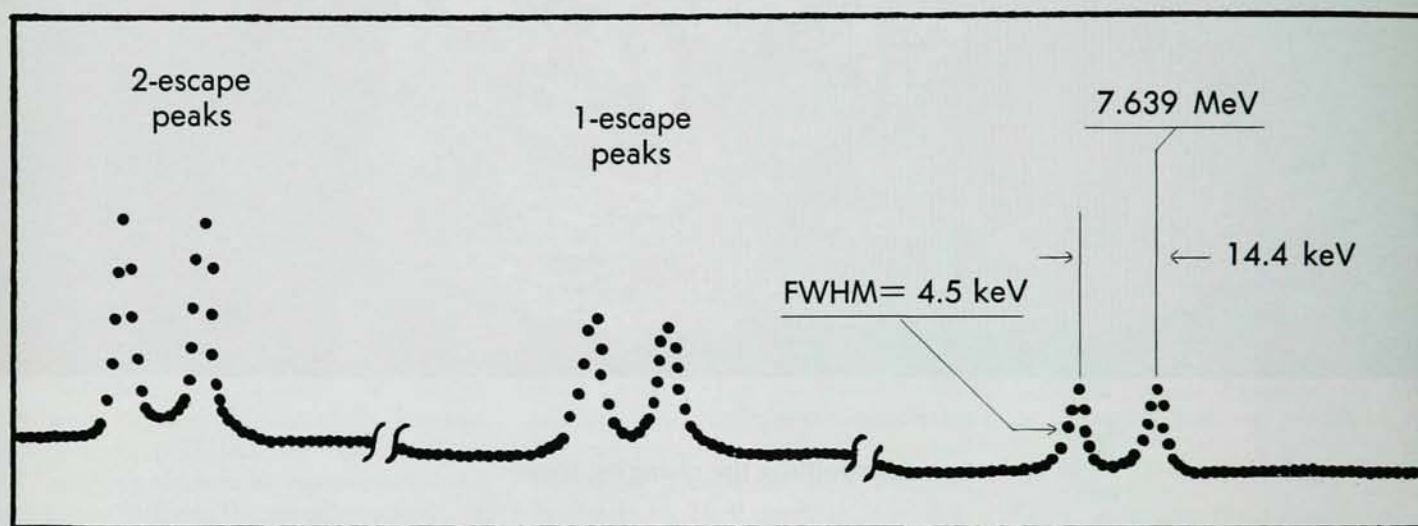


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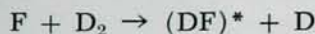
## Continuous-Wave Chemical Laser Requires No External Energy Source

Terrill A. Cool and Ronald R. Stephens<sup>1</sup> of Cornell University believe they have produced the first continuous wave all-chemical laser. In a paper he delivered 26 Nov. at the American Physical Society fluid dynamics division meeting in Norman, Oklahoma, Cool told how they mixed commercially available bottled gases to get  $1.06 \times 10^4$  nanometer emission from carbon dioxide without any external energy source to initiate or sustain lasing action. Maximal power output was 8 watts; lasing continues until the reactants are depleted (up to several hours). The laser operates at about 4% efficiency; Cool predicts 15% efficiency with proper design modifications. A typical electrically excited CO<sub>2</sub> laser has an overall efficiency of about 8%.

Cool's mechanism for chemical pumping of CO<sub>2</sub> involves a fluorine-helium mixture, deuterium and nitric oxide gases as well as CO<sub>2</sub>. To obtain fluorine atoms F<sub>2</sub> and NO are mixed:<sup>2</sup>



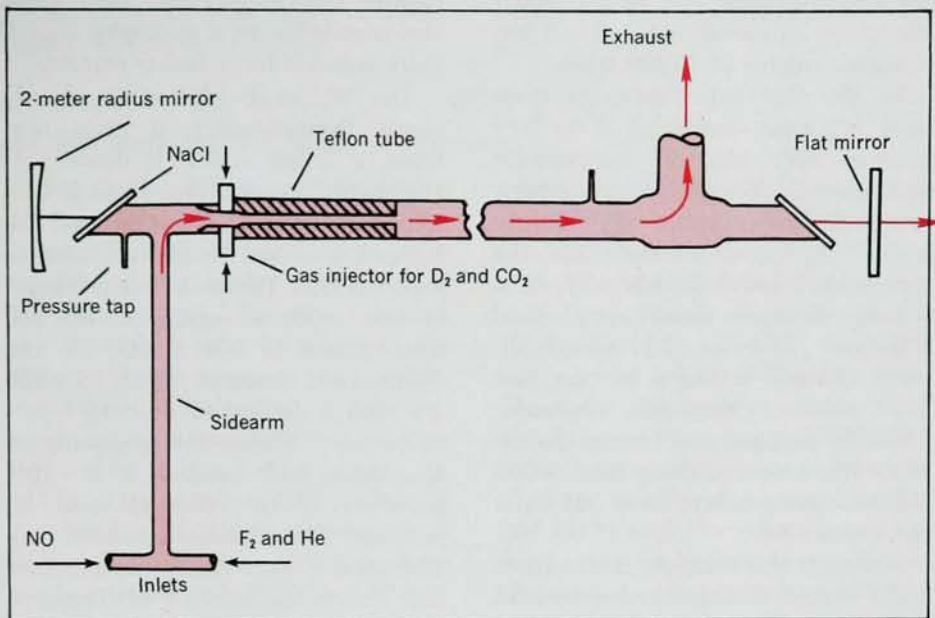
The flowing gas, which now contains both F and F<sub>2</sub>, is mixed with deuterium to produce vibrationally excited deuterium fluoride in a chain reaction<sup>3</sup>



The deuterium fluoride then transfers vibrational-rotational energy to CO<sub>2</sub>, pumping the CO<sub>2</sub> to the upper laser level<sup>3,4,5</sup> from which it emits  $1.06 \times 10^4$ -nm radiation.

The reaction vessel is similar to one used previously by Cool, Stephens and Theodore J. Falk. F<sub>2</sub> and NO are mixed in an 11-mm bore quartz sidearm; deuterium and carbon dioxide are injected at the upstream end of a 9-mm bore Teflon tube. The reaction time in this high-speed (600 m/sec) flow is extremely rapid (100–200 microsec); Cool believes that most of the laser output is from this portion of the flow (see figure).

The Cornell results have shown that a practical flow system is possible.



**CONTINUOUS-WAVE ALL-CHEMICAL LASER.** Arrows show paths of reacting gases. Lasing action occurs mainly in upstream portion of tube.

Because their system operates through a collision mechanism and, unlike some other chemical-laser systems, is not limited to a maximal size, the Cornell group believes it could be developed into a high-power laser. A continuous-wave chemical laser of this type might be used in space.

Chemical lasers were first developed by George C. Pimentel and Jerome V. Kasper<sup>6</sup> at the University of California, Berkeley. The Berkeley group, says Pimentel, has been using pulsed chemical lasers to investigate the role of vibrational and rotational energy states in chemical-reaction dynamics. Other groups<sup>1,7</sup> have reported continuous chemically pumped

lasers, but until now an external energy source has been required.

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## Cold Octopole and Hot Tokamak Show Long Confinement Times

Two results reported at the Dubna International Symposium on Closed Confinement Systems have excited fusion physicists. The high temperature and long confinement time that Lev Artsimovich observed with Tokamak (PHYSICS TODAY, June, page 54) have been confirmed by a visiting British team, and with the Gulf General Atomic multipole Tihro Ohkawa ob-

served very long confinement times. In further experiments (which Ohkawa reported at the November meeting of the APS plasma-physics division in Los Angeles) Ohkawa observed classical diffusion in a dilute cold plasma. His collaborators were Masaji Yoshikawa, Robert Kribel and A. A. Schupp.

Ohkawa used an octopole, which



consists of four internal rings carrying parallel currents in the toroidal direction. Just like all multipoles, the device has axial symmetry about the major axis of the torus. Ohkawa designed the device to reduce losses to the ring supports, one of the major limitations in earlier octopoles; it has a plasma volume of 10 000 liters.

In the first experiments Ohkawa used a plasma density of  $3 \times 10^{10}$  particles/cm<sup>3</sup>; electron temperature was about 5 eV. In the new experiments Ohkawa pushed the density higher ( $10^{11}$  particles/cm<sup>3</sup>) and the temperature lower (a few eV), to a regime where one should get classical diffusion. Ohkawa did indeed observe classical diffusion for the first 150 millise; then the behavior smoothly changed and became Bohm-like. His measured decay time of 200 millise corresponds to about 300 times the Bohm value.

Although the octopole confinement is the longest observed in any toroidal device, its plasma is cold and dilute and not likely to be scaled up into a

reactor because of the interior rings. (General Atomic plans to build a Doublet device, in which internal conductors are replaced by plasma current.) However, because the octopole plasma is well contained one might now try to understand what effects are responsible for the enhanced confinement and then apply the knowledge to a geometry that is more suitable for a fusion reactor.

The Tokamak plasma is already nearly thermonuclear; it gives neutrons, it is hot and it is dense. At Dubna N.J. Peacock and D. C. Robinson of Culham Laboratory and N. Sammikov of the Kurchatov Institute reported that Tokamak T-3 produced in one mode of operation electron temperatures of  $900 \pm 100$  eV and confinement times of about 25 millise with a density of  $2 \times 10^{13}$  particles/cm<sup>3</sup>. Earlier measurements by Kurchatov had yielded  $3 \times 10^{13}$  particles/cm<sup>3</sup> at 1000 eV and 20 millise. The Culham-Kurchatov collaboration determined temperature and density by analysis of Thomson scattering from a pulsed ruby-laser beam.

### Air Force Solar Telescope and OSO-6 Now Observing the Sun

Two new devices are now observing the sun—a solar vacuum-tower telescope built by Air Force Cambridge Research Laboratories and OSO (Orbiting Solar Observatory)-6.

The solar telescope is 111 meters

high and has a central core that contains the entire optical system, which is evacuated to 0.250 torr. Light enters through a 76-cm aperture, passes through a quartz window and is then reflected by two flat mirrors to the 64-inch (1.62-meter) focusing mirror (focal length 55 meters) at the bottom of the shaft. Theoretical resolving power is 0.2 sec of arc; so one can expect to resolve fine details on the solar disc.

Because the objective port is high above most air turbulence and heat currents that swirl up when the sun heats the ground, and because the optical system is evacuated, image stability is expected to be excellent. Richard B. Dunn designed the system.

Located in the Sacramento Mountains of New Mexico, the \$3.3-million instrument will be used to study solar centers of activity—sunspots, magnetic fields, flares and plage areas. One goal is identification of precursors to solar flares.

OSO-6 is returning data from seven experiments. From its vantage point above the atmosphere, it can study in detail the ultraviolet and x-ray spectra at any point in the solar disc. Its expected lifetime is six months.

### IN BRIEF

US and Soviet radio astronomers were to collaborate this fall on the longest baseline ever used for two-telescope interferometry. Telescopes at Green Bank, W. Va., and the Crimean Astrophysical Observatory near the Black Sea—9600 kilometers apart—should provide a resolution of 0.0003 to 0.0005 seconds of arc at a 3-cm wavelength.

Construction has begun on an observatory to house a 40-inch (101-cm) astrometric telescope at the Fan Mountain Observatory of the University of Virginia.

A two-year oceanographic study of the central Mediterranean is taking place. Geophysicists from the Woods Hole Oceanographic Institution, the University of Bologna and the University of Trieste are cooperating in the project and expect to obtain continuous reflection and refraction data from the earth's crust down to the Mohorovicic discontinuity.

### Dicke Panel Says US Lags in Radio-Astronomy Construction

The National Science Foundation Ad Hoc Advisory Panel for Large Radio-Astronomy Facilities, headed by Robert H. Dicke, has decried the lack of US radio-astronomy construction. The panel, originally convened in August 1967 (PHYSICS TODAY, September 1967, page 71), met again to review its original recommendations. In a recently issued report the panel points out that none of the suggestions made two years ago has yet been implemented. The US, it says, has stood still while Germany, India, the Netherlands and the UK have begun construction on large radio telescopes, several of which will soon be in operation.

Noting that discoveries since the panel first met (pulsars, existence of interstellar formaldehyde, ammonia and water) have made construction of new telescopes even more imperative now than two years ago, the panel recommends that:

- the 305-meter spherical-dish telescope at Arecibo, Puerto Rico (PHYSICS TODAY, April, page 65) be resurfaced so that it can be useful for centimeter-wave radio astronomy. Resurfacing was urged two years ago as a relatively inexpensive improvement.
- the Cal Tech proposal for con-



AIR FORCE SOLAR TELESCOPE is 111 meters high. The optical system is evacuated to 0.250 torr.



struction of an eight-dish array at the Owens Valley Observatory be accepted.

- construction of a fully steerable 134-meter radome-enclosed dish be begun immediately, probably in the dry southwestern portion of the US.

- construction of the Very Large Array of 27 antennas, as proposed by the National Radio Astronomy Obser-

vatory, be begun immediately. This array would produce up to three pictures daily with a resolution of 1 sec of arc, which is equal to that of optical photographs.

- studies of methods for construction of very large steerable dishes be continued. Emphasis should be on design of an antenna useful at wavelengths as small as 3–6 mm.

- support of university radio astronomy be continued and improved.

- grants and contracts for US support of radio-astronomy installations require not only that half the observing time be available to visitors, but also that the installations be managed to assure representation of national interests and maximal usefulness to visitors.

## Measuring It Better: A Visit to Bureau International des Poids et Mesures

In an old house in Paris  
All covered with vines  
Lived twelve little girls  
In two straight lines.

If you drive west from Paris toward Versailles, you can easily pass through the little town of Sèvres without knowing that in it is the International Bureau of Weights and Measures. Only when you turn through a narrow arched gateway and climb a few hundred yards through the woods to a small clearing in the Parc de St Cloud do you come to the little historic manor, Pavillon de Breteuil.

The approach and the exterior suggest an atmosphere like that of the lines that open Ludwig Bemelmans's "Madeleine in Paris." Once, in fact, it had such an atmosphere. "Forty years ago," Jean Terrien, the present director told me on a recent visit, "Bureau International des Poids et Mesures had the feeling of an old lady. There were few pieces of original research."

Step inside, though, and you find a different atmosphere. The neat laboratories are making some of the most careful measurements in the world. The aim is to determine standard values and best procedures to measure them. Major concerns are length, mass, time, acceleration of gravity, electrical units, temperature, photometry and ionizing radiation.

The main function of the bureau is coordination of efforts everywhere to define and measure quantities accurately. Its small staff ("about 50 persons including the gardener," said Terrien) can not do such amounts of work as go on at the US National Bureau of Standards and the UK National Physical Laboratory. But it does much to test and compare the methods suggested by these and similar national laboratories. Moreover seven international consultative committees based at BIPM make the most fundamental decisions required for coordination and cooperation. Their seven

subjects are electrical quantities, photometry, thermometry, ionizing radiation, definition of the meter, definition of the second, definition of units.

40 governments have signed the "Convention du Mètre," the 1875 treaty under which BIPM was born. They meet at least every six years and usually every four years in the Conférence Générale des Poids et Mesures. (Terrien shuddered at the thought that BIPM might have become part of the League of Nations or the United Nations. As an organization fulfilling a purpose, it is running more effectively than those trying to find purposes they can fulfill.) The 40 elect an 18-member committee, which operates BIPM and the seven consultative committees.

The bureau is in no sense French although it happens to have a French home and a French director. Former directors have been Swiss, Italian, Norwegian and British. It does not even function as a standards bureau



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**DIRECTOR JEAN TERRIEN** was formerly an optician on staff of the bureau.



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for France, which distributes standardization work among several ministries and only recently has moved to coordinate the various efforts more closely.

The BIPM staff has 12 physicists, eight of them "pure" and four experienced in the work of the bureau. Working with them are 12 very skillful senior technicians.

**Length.** "What measurements do you consider particularly your own?" I asked Terrien. "We make a specialty of length," he replied and described the work that went into the redefinition of the meter. When the change from a standard bar to an optical wavelength was proposed, the US suggested a  $\text{Hg}^{198}$  line, the Germans proposed  $\text{Kr}^{84}$  or  $\text{Kr}^{86}$  and the Russians preferred a standard based on  $\text{Cd}^{114}$ . Starting in 1955 Terrien, who was not then director but an optician on the staff, spent three years studying line shapes. He finally concluded that the  $\text{Kr}^{86}$  transition  $2p_{10} \rightarrow 5d_5$  (now the base of the definition) was best. It made a narrow spectral line, and Terrien could explain its shape completely in terms of Doppler shift, pressure broadening and lifetimes of states.

Unfortunately the line is not entirely symmetrical. To improve upon it as a length standard two courses are possible: One is to recognize the line shape in a Michelson-interferometer pattern and with it specify just which

part of the line is the standard wavelength. The other is to go to a laser method. Lasers, to be sure, have the difficulty that tuning can pull the oscillation away from the natural wavelength. To remedy it you can adjust the laser to a natural absorption frequency. Work is commencing on the scheme. For example the helium-neon laser has several coincidences with iodine and methane absorptions. Probably ten or 15 usable coincidences are known now and 100 might appear with two or three years of work.

In the laboratory, I visited the comparator BIPM uses to compare standard bars with the krypton line. Temperature of the room it stands in is controlled to a few hundredths of a degree, and temperature in its tank to a thousandth. The operator sits next door, directing a light beam along a selected interference path and recognizing scratches on the test bar by signals from optical scanning devices.

**Time and gravity.** Time is closely related to length, or, if you prefer, it has become the same quantity now that measurements of optical frequencies have become possible. The development puts BIPM into a new business. There are no time standards at Sèvres, but there exists the consultative committee on the second. "I am learning now what I must know," says Terrien as he discusses how BIPM may get involved. He feels that with recent improvements of technique the present second based on a cesium transition is the best unit, but the hy-

drogen maser might produce a better one. Laser standards are better in principle, but accuracy with them is not yet good enough to compete.

I stood at the spot where acceleration of gravity is known to eight significant figures. Changing elevation by 2 cm changes the last figure, pointed out Terrien. So would a significant amount of concrete construction in the basement. Then we walked next door where standard cells in temperature-controlled oil baths and standard resistors offer the basis for electrical measurements. Gravity and electrical measurements are closely related. As you know better the weight of a kilogram, you can measure more accurately the forces between coils; forces are related to the standard ampere, and so on.

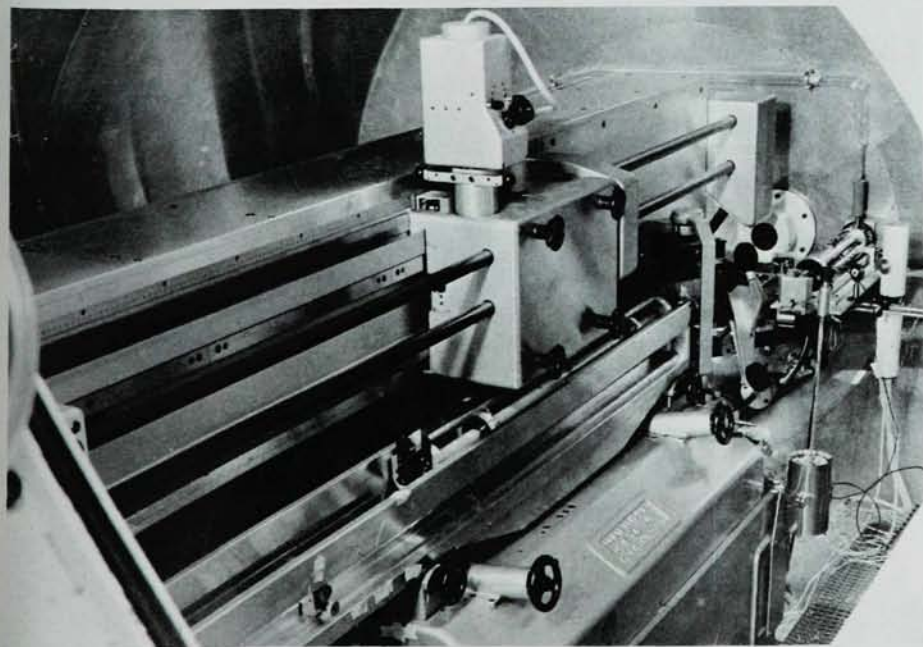
A working group of the committee on electrical units studies measurements of the proton gyromagnetic ratio. Well enough measured, it might some day be a basis for better electrical units.

Another new device that might serve the same purpose uses the Josephson effect: A constant potential appears across a narrow junction between superconducting metals when they are driven with a fixed frequency.

**Radiations.** The newest section of BIPM is devoted to ionizing radiations. I saw x-ray and neutron generators,  $\text{Co}^{60}$  irradiators, free-air and cavity ionization chambers, gamma spectrometers and counting devices for neutron and radioactivity sources. Among unique accomplishments of the section is an absolute alpha-particle spectrograph for maximal possible accuracy. It uses a homogeneous magnetic field that bends alphas emerging from a slit through semicircles and causes them to focus on a photographic plate. Results obtained so far add at least one decimal place to best former measurements. Hopes are for detection of line shapes produced by interaction between alphas and the electron clouds of the atoms from which they come.

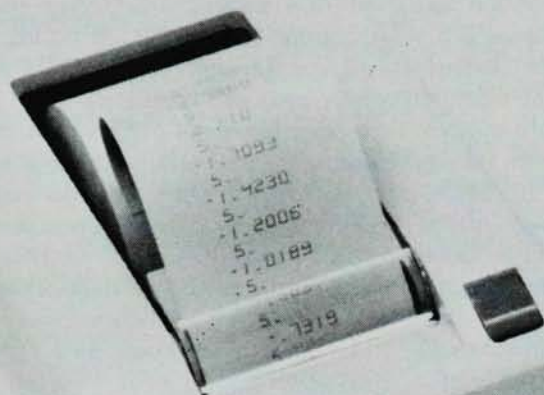
Terrien is a careful man whose manner suggests the precision with which French engineers design their cars and vacuum tubes. He says his job makes him travel too much in his efforts to learn what he must know. Like physicists of other times and places he and BIPM appear to enjoy the challenge of making discoveries by resolving the next decimal.

—RHE □



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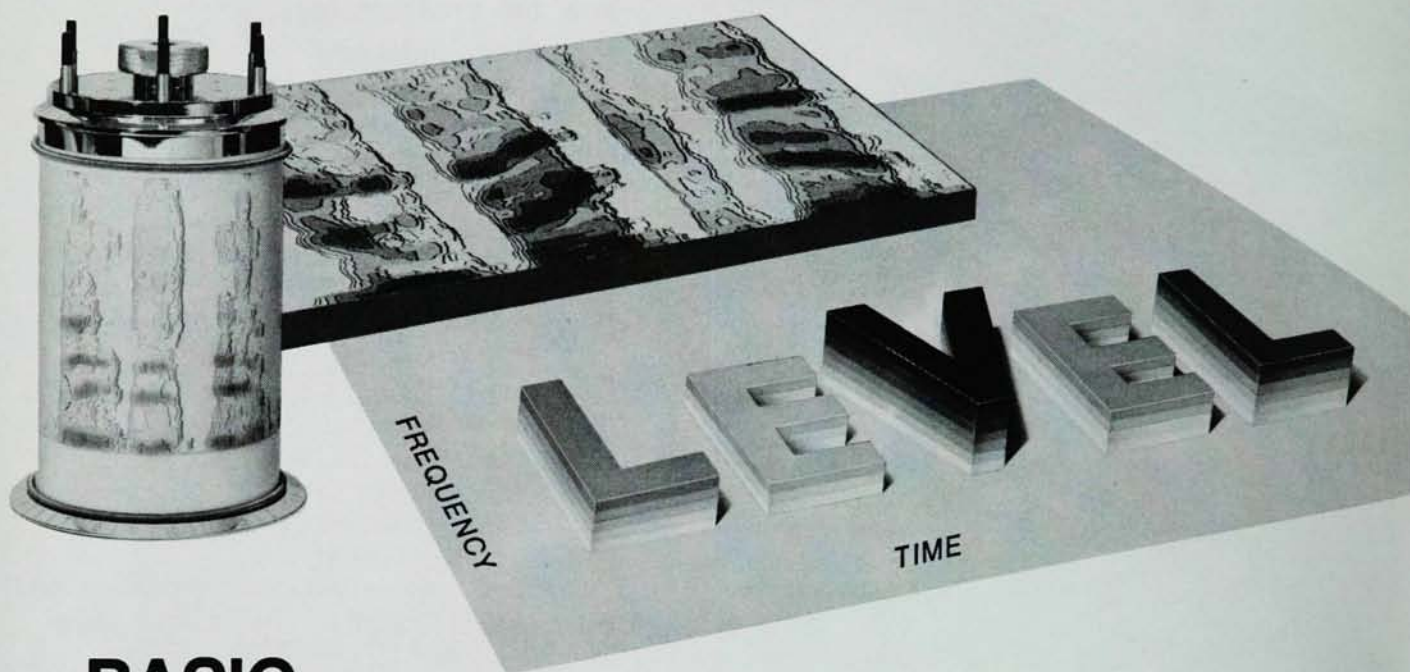
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## Metzner Named Assistant Director of AIP Publications

A. W. K. Metzner was named to the new position of assistant director of publications for the American Institute of Physics, where he shares responsibility for all operations with Hugh C. Wolfe, director of publications. Metzner will explore new composition techniques, particularly computer-aided photocomposition and



METZNER

typewriter composition. He and the editorial staff for *The Physical Review* are located at the institute's newly acquired 3000 square feet of space at 300 East 42nd Street, New York. Metzner was formerly an editor of *The Physical Review* and involved in the typewriter composition of Section I at Brookhaven National Laboratory.

David Howell was also recently appointed as manager of the AIP editorial section, within the publication division, and replaces David Biesel. Howell was formerly with the technical-information division at the American Institute for Aeronautics and Astronautics.

## Fund of Abdus Salam Has First Recipient

A Fund for Physics in Developing Countries has been set up by Abdus

Salam, director of the International Centre for Theoretical Physics (PHYSICS TODAY, Sept., page 77, 1968), with the \$30 000 he received as winner of the 1968 Atoms for Peace Award. The first fellowship recipient is A. Q. Sarker, an East Pakistani physicist who specializes in dispersion theory and high-energy physics.

In an effort to thwart the brain drain, the fund will help research physicists, particularly theoretical physicists, from the developing countries. First priority will be the awarding of fellowships to senior physicists, permitting them to participate in the Centre activities; preference will be given to theorists from Pakistan. The board of trustees consists of Paolo Budini, deputy director of the Centre; P. T. Matthews of Imperial College, London; I. H. Usmani, chairman of the Pakistan Atomic Energy Commission; and Salam.

Support funds, which are invested at 5.5% interest with the Italian bank, Cassa di Disparmio di Trieste, have come from the firms Messrs Piaggio and Co, Genoa; Pirelli S.p.A., Rome; SNAM Progetti S.p.A., Milan; and Tarbela Joint Venture, Milan—in addition to the bank itself, which made the first donation and agreed to pay interest on its donation at 11%. The appeal for donations was made by N.A.M. Raza, former Pakistani ambassador to Italy.

## Dart, Moravcsik to Evaluate Foreign Graduate Candidates

How can universities evaluate a prospective foreign graduate student without seeing him? Francis Dart and Michael Moravcsik, Univ. of Oregon, aim to do something about the problem. With support from the universities of Oregon, Michigan, Pittsburgh and California at Los Angeles they are writing evaluations based on interviews in Korea, Hong Kong, Thailand, Singapore, Malaysia, India and Pakistan. Their month-long trip was designed to include interviews with about 150 students who want to study advanced physics in the US. The visit dates were 11 Oct.–19 Nov.

Each report will be available to any school in which the student is inter-

ested and any interested in him. The four universities sponsoring the Physics Interviewing Project get the evaluations first; two months later it becomes generally available. Dart and Moravcsik only evaluate; they are not involved in recruitment, admission, scholarships and the like.

The two-man committee and their sponsors view the project as an experiment. Students admitted through it will be surveyed during 1970–71 to see whether personal interviews improve selection.

## JILA Has Fellowships and Associateships for 1970–71

The Joint Institute for Laboratory Astrophysics is soliciting applicants for 1970–71 visiting fellows and research associates. "Joint" means Bureau of Standards plus University of Colorado, and the institute is housed on the university campus at Boulder. Its subjects are theoretical astrophysics, low-energy atomic physics and related topics.

About ten stipends exist in each category (associates get \$11 000 plus expenses), and other visitors are invited to come with their own support. One more fellowship is shared between JILA and the university Laboratory for Atmospheric and Space Physics. Fellows are expected to come with extensive postdoctoral research and have no obligations. Associates are new PhDs and are appointed simultaneously to a university department and JILA.

## AIP Publishes Guide to Undergraduate Departments

A guide to the physics departments at 622 US colleges and universities offering undergraduate majors has been published by the American Institute of Physics. Another 207 schools that offer majors but did not respond to the AIP questionnaire are listed. The 176-page book includes information on faculty, students, equipment, and physics-major programs. Copies of *Student's Guide to Undergraduate Physics Departments* can be ordered from AIP, 335 E. 45th St., New York, N. Y. 10017. Price is \$2.00 per copy



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prepaid or \$2.50 if billed. Intended to be particularly interesting to advisers in high schools and colleges, the book is a companion volume to *Graduate Programs in Physics and Astronomy*.

### Health Physics Society Elects New Officers

The Health Physics Society has announced that officers for 1969-70 are: J. Newell Stannard (University of Rochester School of Medicine), president; Claire C. Palmiter (Federal Radiation Council), vice-president; Robertson J. Augustine (Bureau of Radiological Health), secretary; Robert L. Zimmerman (Nuclear Research Center, Georgia Institute of Technology), treasurer.

### Nixon Names 12-Man Task Force To Review US Science Policy

A second review of federal science policy in the US has been ordered by President Nixon. Ruben F. Mettler, executive vice-president of TRW, Inc, heads the 12-man group, which includes Charles H. Townes of the University of California at Berkeley, Alvin M. Weinberg of Oak Ridge National Laboratory and Philip Handler, president of the National Academy of Sciences.

### APS Arranges Group Flights To Europe and Japan in 1970

Group flights to Europe and Japan timed to coincide with major international meeting in 1970 are being arranged by the American Physical Society for its members and also their families. The schedule includes spring and summer flights to London, summer flights to Helsinki and Leiden, and a late summer flight to Tokyo. Prices are substantially below commercial rates. Details can be obtained from the business manager of the society at 335 E. 45th St., New York, N. Y. 10017.

### European Physical Society Announces Division Chairmen

Chairmen have been named for the first five divisions of the European Physical Society. The EPS Council has approved the divisions for two years, after which it will review the

situation and make any changes it feels appropriate. The divisions and their chairmen are:

Atomic spectroscopy, Alfred Kastler, Paris; Condensed matter, Samuel F. Edwards, Manchester; Low-temperature physics, Jan de Boer, Amsterdam; plasma physics, Bo Lehnert, Stockholm, and quantum electronics, Klaus P. Meyer, Berne.

The council can approve additional divisions on application of five or more members.

### AIP and Society Journals Available in Microfilm

Microfilmed volumes of all American Institute of Physics journals and some member-society journals are available as of 1 Jan. 1970. Supplied by University Microfilms Inc, they can be ordered from AIP at one cent per page only after publication of a complete journal volume. 1969 and later volumes are in 16-mm reels; some earlier volumes in 35 mm and others in 16 mm or microfiche; and Russian-translation volumes only in 16 mm. AIP will charge the original subscription price if the cost is higher than one cent per page.

### IN BRIEF

High isotopic-purity isotopes of  $U^{233}$ ,  $U^{234}$  and  $Pu^{242}$  are being sold by AEC, and  $Co^{60}$  at high specific activity (more than 200 curies per gram) is available on loan from AEC for heat sources.

State and local governments are getting help in developing and planning science policies under a program supported by the National Science Foundation. Charles E. Falk, NSF planning director, is in charge.

The University of Miami has established a solid-state laboratory with \$45 000 from the university research council and \$16 000 from the university budget.

The American Nuclear Society has added "organization members" (paying \$100 to \$500 per year) to its membership roster.

National Bureau of Standards has completed eight years of construction at Gaithersburg, Md., with a Fluid Mechanics Building, 20th primary structure on the grounds.

Spectronics, Inc, is a new developer

and manufacturer of optoelectronics and infrared systems in Dallas, Tex. G. W. Paxton is president.

An International Committee on Thin Films with nine members from nine countries has as chairman Klaus H. Behrndt, NASA Electronics Research Center, Cambridge, Mass.

Franklin A. Long, Cornell, is director of a new interdisciplinary program in science and technology. Starting with a \$140 000 National Science Foundation grant, it will study problems of national and worldwide concern: public policy, defense, food, ecology, population, urbanization.

Science for the Blind, a nonprofit organization providing scientific material on tape to about 500 persons, urgently needs volunteer readers with tape recorders for *Science Recorded*, which has been including selections from PHYSICS TODAY for more than five years. Volunteers should contact: Mrs Donald A. Duncan Jr, Science for the Blind, 221 Rock Hill Rd., Bala-Cynwyd, Pa.

The American Society for Mass Spectrometry has grown out of Committee E-14 of the American Society for Testing and Materials. J. L. Franklin, Rice University, is president.

A new national committee on material sciences (metallurgy, chemistry and solid-state physics) is headed by Frank J. Blatt, Michigan State.

The Hospital Physicists' Association (British) has transferred its secretariat to headquarters of the Institute of Physics and the Physical Society. IPPS will carry on day-to-day business, but the 25-year-old, 850-member association will still control its own activities.

Sigvard Eklund has been appointed to his third four-year term as director general of the International Atomic Energy Agency.

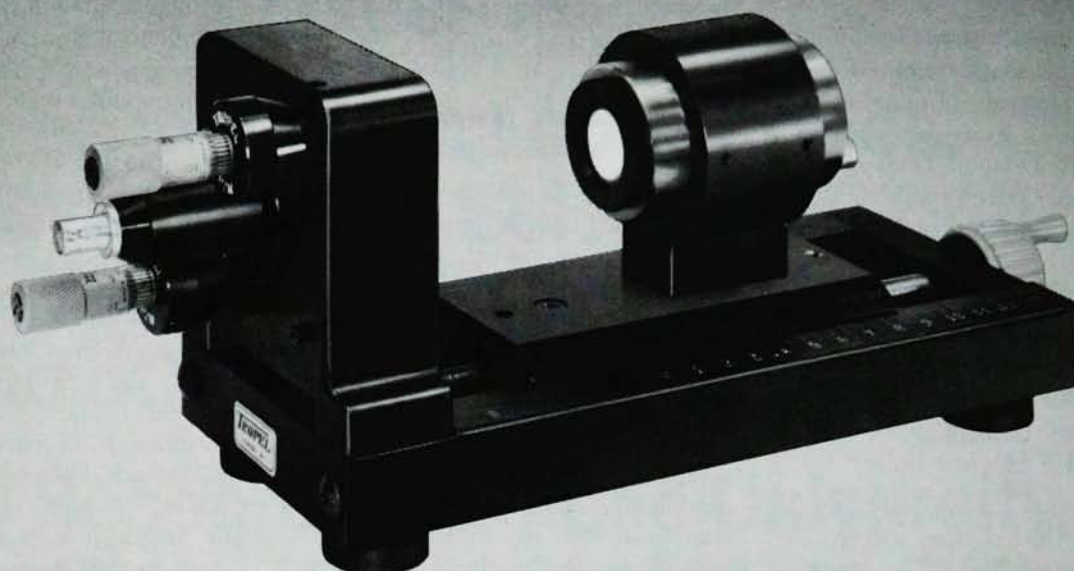
43 persons have been named to a Metric System Study Advisory Panel by Secretary of Commerce Maurice H. Stans. The panel will assist the secretary, the director of the National Bureau of Standards, and the Bureau's Metric System Study Group headed by Alvin McNish.

The Scientists' Institute for Public Information has received a \$210 000 grant from the Alfred P. Sloan Foundation to help expand its efforts to stimulate discussion of issues involving science and technology.

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has been sold for the second time in a year. Last year (PHYSICS TODAY, January, page 93) the Ealing Corporation acquired the optical concern. Now Sanders Associates of Nashua, N. H., has bought it from Ealing.

Ethel Snider was appointed to the new position of administrative secretary for both the American Crystallographic Association and the American Association of Physicists in Medicine, as of 1 Sept. Snider's office is at the American Institute of Physics, which has begun publishing the *Quarterly Bulletin of the AAPM*. ACA is a member society of the AIP, and AAPM is an affiliated society.

## NEW JOURNALS

Gordon and Breach is publishing four new journals: *Crystal Lattice Defects*, a quarterly, with R. R. Hasi-guti, University of Tokyo, as editor; *Geophysical Fluid Dynamics*, a quarterly with A. R. Robinson, Harvard, as editor; *Modern Geology*, a quarterly, with Luciano B. Ronca, Boeing Scientific Research Laboratories, as editor; *Earth and Extra-terrestrial Sciences: Conference Reports and Professional Activities*, to be published irregularly, with A. G. W. Cameron, Belfer Graduate School of Yeshiva University, as editor.

John G. Daunt is editor of the *Journal of Low Temperature Physics*, a new bimonthly from Plenum Publishing Corp.

*Atomic Data*, a quarterly journal "devoted to compilations of experimental and theoretical results in atomic physics," had its first issue in September. Katharine Way, Duke University, is editor, and Academic Press is publisher.

*IR&T Nuclear Journal*, published by International Research and Technology Corp in Washington, was started this year to provide analysis of developments in nuclear technology and its impact on society.

*Optics Communications*, a new quarterly devoted to "rapid publication of short contributions in the fields of optics and interaction of light with matter" is being published by North-Holland Publishing Co. The editor is Florin Abeles of the Laboratoire d'Optique, Paris. □

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## FORMULATIONS OF CLASSICAL AND QUANTUM DYNAMICAL THEORY

by **GERALD ROSEN**, *Department of Physics, Drexel Institute of Technology, Philadelphia, Pennsylvania*

This monograph reviews the mathematical structure within the logical relationships between classical mechanics and quantum mechanics for nondissipative, closed physical systems. Quantum mechanics is formulated according to Feynman, Schrodinger, and Dirac. Up-to-date detail is given for the conceptually paramount Feynman passage and "sum-over-histories" formulations for quantum mechanics. The mathematics necessary for the understanding of this text are introduced in elementary terms, making the work readily accessible to the reader. Recent quantum field theory applications of functional differential operator formulations are also included.

*December 1969, about 150 pp.*

## THE FUNDAMENTAL CONSTANTS AND QUANTUM ELECTRODYNAMICS

by **B. N. TAYLOR**, *RCA Laboratories, Princeton, New Jersey*. **W. H. PARKER**, *Department of Physics, University of California, Irvine, California*; **D. N. LANGENBERG**, *Department of Physics and Laboratory for Research on the Structure of Matter, University of Pennsylvania, Philadelphia, Pennsylvania*.

This volume offers the most critical, comprehensive, and up-to-date analysis of theoretical and experimental information bearing on the fundamental physical constants. This book makes available, in permanent monograph form, an original article appearing in the July 1969 issue of *Reviews of Modern Physics*; it has been prepared in cooperation with the American Physical Society in anticipation of the widespread interest this material is certain to evoke among physicists in general as well as workers in the precision measurements—fundamental constants fields.

*December 1969, about 350 pp., \$5.00*

## PHYSICAL ULTRASONICS

by **ROBERT T. BEYER**, *Department of Physics, Brown University, Providence, Rhode Island* and **STEPHEN V. LETCHER**, *Department of Physics, University of Rhode Island*

*Physical Ultrasonics* is intended for graduate students and scientists who plan to apply ultrasonic techniques to study the physical properties of solids, liquids and gases. A thorough presentation is given of the generation, propagation and detection of ultrasonic waves with emphasis placed on the physical processes such as irreversible thermodynamic treatment of relaxation theory, non linear effects, absorption in insulators, dislocation damping and spin wave interaction.

*November 1969, about 365 pp., \$18.50*

## RADIATION AND PROPAGATION OF ELECTROMAGNETIC WAVES

by **GEORGE TYRAS**, *Cullen College of Engineering, University of Houston, Houston, Texas*

This volume is intended for use in a two-semester graduate course in electrical engineering or electrophysics for students with only undergraduate preparation in electromagnetic theory. It will also provide practicing engineers with a highly valuable reference source. Topics covered include plane waves in anisotropic media and inhomogeneous media, spectral representation of elementary sources, field of a dipole in a stratified medium, radiation in anisotropic plasma, axial currents and cylindrical boundaries, diffraction by cylindrical structures, and apertures on cylindrical structures.

*1969, about 375 pp., \$17.50*

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## UFO's: fact or fiction?

SCIENTIFIC STUDY OF UNIDENTIFIED FLYING OBJECTS. E. U. Condon, scientific director; Daniel S. Gilmor, ed. E. P. Dutton, New York, 1969. Cloth \$12.95, paper \$1.95

ALIENS IN THE SKIES. By John G. Fuller. 217 pp. Putnam, New York, 1969. \$5.95

UFO's? YES!: WHERE THE CONDON COMMITTEE WENT WRONG. By David R. Saunders and R. Roger Harkins. 256 pp. The New American Library, New York, 1969. \$.95

by GERALD ROTHBERG

If I were asked for the most important guideline in studying unidentified flying objects (UFO's), I would undoubtedly say, "Be skeptical of everything!" I do mean *everything*, the con as well as the pro of the UFO controversy. Too many persons find it impossible to delve into the subject without eventually becoming overly zealous supporters of their own points of view. I like to believe this has not yet happened to me, but so the reader can judge I will first indicate my background in the subject.

In the summer of 1967 I worked for the University of Colorado Unidentified Flying Objects Project, directed by Edward U. Condon. The motivation was my belief that evidence of extraterrestrial intelligence (ETI), if UFO's could provide it, would be the most important discovery of all time. The first difficulty, however, is, "What constitutes evidence?" At one extreme is Condon's attitude:<sup>1</sup> "I won't believe in outerspace saucers until I see one, touch one, get inside one [and] haul it into a laboratory and get some competent people to go over it with me." At the opposite extreme are the religious fanatics who have gathered around some of the self-proclaimed contactees.

This already delicate question of evidence is further complicated by economics. With its limited resources, \$526 000, the Colorado UFO project produced a good, minimal effort. A thorough study would require orders of magnitude more money. For example, James E. McDonald, meteorology professor and senior physicist at the Institute of Atmo-

spheric Physics, University of Arizona, talks of an effort the size of the National Aeronautics and Space Administration. Faced with the economic implications, I would be very confident of my evidence before accepting ETI as a reasonable working hypothesis for recommending a large-scale investigation.

Almost all our information about UFO's is from reports of visual sightings. Some of these are truly startling and mysterious. I am very unhappy about these sightings, because it is so easy to be deceived, and after my work as a field investigator with the Colorado project I am even more skeptical. One night, for example, I was present when about a dozen people in Harrisburg, Pa., reported an object 1000 feet above the city, flashing red, white and blue. It turned out to be the star Capella, which was also later responsible for a report of a flashing object with projecting antennas and a dome.

My reason for working with the Colorado project was to attempt to improve the objectivity of the data by obtaining instrumental observations, or combined instrumental and visual,

with a team of scientists who were prepared in advance to go into the field with suitable instruments and who had good mobility. This appeared possible because there occasionally is an outbreak of UFO activity reported in a limited geographical area. There happened to be such an outbreak in the Harrisburg area that summer and, after a month of preparation at project headquarters in Boulder, I spent a month there making what I consider the best attempt so far to see and record a UFO at first hand. A brief description of this effort appears as case 27 in the UFO-project report. I personally investigated about 100 sightings and took 9000 pictures with an all-sky camera, set up in the center of activity, but never saw or recorded a UFO. Three or four of the sightings I investigated are as good as some of the classic cases in the UFO books, and it is this nagging residual that keeps me from dismissing the whole business as ridiculous. When you arrive at the scene of a sighting within 30 minutes and an otherwise normal, respectable family tells you a large luminous disk with a dome and a flashing red light hovered 30 feet



LENTICULAR CLOUDS over São Paulo, Brazil. (Photo appears in the UFO-project report and is reproduced courtesy of the Aerial Phenomena Research Organization.)



over their heads, it is hard to believe they did not see something real and strange.

The report of the Colorado project, *Scientific Study of Unidentified Flying Objects*, has to be read by everyone interested in the UFO question. It is almost a thousand pages long, but in the first reading many hundreds of pages containing peripheral technical information can be passed over.

*Aliens in the Skies* by John G. Fuller is essentially just the transcript of the "Symposium on Unidentified Flying Objects" held on 29 July 1968, before the House Committee on Science and Astronautics. At this meeting six reputable scientists, including McDonald and J. Allen Hynek, professor and head of the astronomy department at Northwestern University and chief scientific consultant of UFO's to the Air Force, presented a case for the continued and expanded study of UFO's in direct opposition to the eventual recommendation of the Colorado project.

Fuller has written two other books on the subject, *Incident at Exeter* and *The Interrupted Journey*, but this latest book is definitely not of their quality. Aside from a number of nasty remarks about Condon and editorial comments on the testimony, there is nothing in this book that can not be obtained from the printed record available from the government.<sup>2</sup> Some of the most important material at the symposium was prepared documents submitted for in-

clusion but not delivered orally at the meetings. Most of this material is not included in Fuller's book, which also lacks a table of contents and does not identify the congressmen who participated.

David R. Saunders and R. Roger Harkins's book, *UFO's? Yes!: Where the Condon Committee Went Wrong*, is meant to be read before reading the project report itself because, like Fuller's book, it attempts to question the credibility of the report by questioning Condon's objectivity and that of the project administrator, Robert J. Low. In my opinion the report does represent the thinking of a substantial number of the senior staff, perhaps even the majority, and therefore it can not be faulted on grounds of bias. I would have been less negative and recommended a small continuing study.

Although the report suggests that the usual funding agencies accept research proposals in this area, it appears very unlikely that the standard machinery for processing proposals will result in any grants. I would like to know if anyone has submitted a research proposal on UFO's. Whatever one thinks about the controversy between Saunders, on the one hand, and Condon and Low, on the other, which eventually reached the public in Fuller's article in *Look*<sup>3</sup> and resulted in the dismissal of Saunders from the project, this book and Fuller's are in fact important complements to the project report.

Harkins was a reporter for the *Boulder Daily Camera* during the project. Saunders is a psychology professor at the University of Colorado and was one of the principal investigators of the project and its driving force. He put together a catalog of sightings that numbered roughly 2000 by the time I left the project. The prospective reader then, if he is not put off by the lurid title and book covers, will find a very readable account of the inner workings of the project and the conflicts that finally resulted in its schism.

But the book has more in it than that. The title *UFO's? Yes!* means that Saunders now believes there are at least a small number of "real" UFO's, that is, reports of UFO's that lend themselves to thorough investigation and that have been investigated and found inexplicable in terms of known phenomena. This important point is also made repeatedly in the UFO symposium. Saunders also believes that ETI is the least implausible explanation of these real UFO's. This is in marked contrast to the project report, which plays down the few unexplained sightings by burying them in a mass of cases that were plausibly explained. Of the 59 field investigations carried out by the project, none, of course, conclusively support ETI, but a few interesting cases remain unexplained, and these should have been prominently displayed to ensure they would not be passed over. They possibly contain the only worthwhile information in the whole study. Actually the project case against UFO's is much stronger than these numbers indicate. Many sightings are discussed in other sections of the report and satisfactorily explained, and perhaps hundreds of other sightings, most of which have also been explained, do not even appear; for example, the 100 reported in Harrisburg, and the numerous ones investigated by telephone and discarded before field teams were sent.

Saunders discusses what he considers the strongest evidence for real UFO's and also describes his current research on statistical and psychological aspects of UFO's and possibilities for future studies. There are also descriptions of some other UFO sightings that sometimes seem overdrawn when compared with the descriptions of them in the project report.

These three books should appeal to a wide audience. The subject is

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-



inherently sensational and at times the documents read like first-rate detective stories, but the investigators' scientific training also comes through clearly. None of these books, though, should be read without the others.

One annoying feature of the project report is its deliberate obscurity in witnesses' names and exact sight locations in the case studies. It is not clear why this was done, because the report does not do it consistently, and in many other cases these identifications are made. Furthermore some of these cases are classics in UFO literature. This procedure makes it difficult to compare the results of the project's investigation of a UFO report with the descriptions given in the other books. Two of the more difficult examples are case 5 of the report, which appears on page 126 of "UFO's? Yes!" and case 42, which appears on page 197. Paul Julian's discussion of orthoteny, that is the straight-line relationship among different UFO sightings, appears in the report (section 6, chapter 10), but is not listed in the index and is relevant to Saunders's discussions.

The point of view of the project report is that all but a small percentage of UFO reports can be reasonably explained, including some that seem very strange. Therefore it is plausible that the residue of unexplained reports could also be explained if more information were available, and that the hypothesis of ETI is unnecessary and unproductive. Saunders, McDonald and others believe that among this residual are cases that are demonstrably not caused by known natural phenomena, and that ETI is the most plausible hypothesis. We now need some reputable journal to recognize this legitimate scientific controversy and to publish analyses of UFO reports with the ETI proponents also stating their results. Who knows? They may just be right.

References

1. W. Rogers, Look, 31, 6, 76 (1967).
2. "Symposium on Unidentified Flying Objects." Publication PB 179541. Clearinghouse for Federal Scientific and Technical Information. US Department of Commerce, Institute of Applied Technology, Springfield, Va. 22151.
3. J. C. Fuller, Look, 32, 10, 58 (1968).

\* \* \*

The reviewer is an associate professor of physics at Stevens Institute of Technology.

Precursors of Galileo and modern science

MECHANICS IN SIXTEENTH-CENTURY ITALY: SELECTIONS FROM TARTAGLIA, BENEDETTI, GUIDO UBALDO & GALILEO. Translated and annotated by Stillman Drake and I. E. Drabkin. 428 pp. The Univ. of Wisconsin Press, Madison, Wisconsin, 1968. \$12.50

by ROBERT S. SHANKLAND

This is a work of the very highest scholarship and in the tradition of Stillman Drake's other distinguished works on Galileo and related subjects in the history of science. The book was prepared in collaboration with the late I. E. Drabkin, and includes introductions written by him for his translations. This selection of writings covers a century that was the final transition period leading from medieval to modern science.

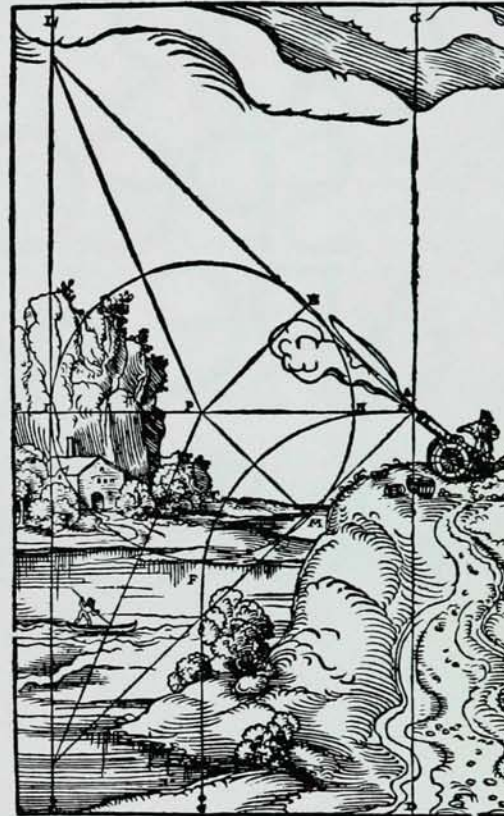
The emphasis at that time was almost exclusively on mechanics, hydraulics and the related mathematics, especially algebra, which was recently introduced into Europe. It is also the period when Aristotle's influence steadily declined and Archimedes of Syracuse's, whose works had recently become available in a useful translation, became more and more dominant. There is also evidence of Hero of Alexandria's influence and faint suggestions of ideas from Leonardo da Vinci.

The editors have prepared a splendid introduction that could hardly be improved upon as model writing in the treatment of the history of science as a rigorous intellectual discipline. The excellent translations present the works of Niccolò Tartaglia, Giovanni Benedetti, Guido Ubaldo and an early hitherto unpublished work of Galileo on motion, prepared during his teaching days at Pisa.

Many of the subjects that the physics student usually associates exclusively with the name of Galileo were considered in great detail by some scientists during the 16th century. Examples are the science of weights that led to important applications in the balance and the investigations of levers and pulleys that led to Fontana's success in erecting the Egyptian obelisks in Rome. Many military machines were studied and perfected and also the screw of Archimedes, which to this day plays an important role in the agriculture

of Egypt. Ballistics commanded great attention, and also closely studied before Galileo were falling bodies and projectile motion, including air resistance. During that time scientists investigated many simple and complex machines, both for their inherent scientific interest and for their great practicality in architecture, especially as applied by Alberti, and in shipbuilding and maritime equipment, as shown above all in the great arsenal at Venice.

This is a fascinating book that clarifies the earlier scientific developments that made Galileo's great advances possible: It is also history in



TRAJECTORY DIAGRAM by Niccolò Tartaglia, superimposed on a landscape, as shown by Walther Ryff in *Der geometrischen Buxenmeisterei*, in *Der Architectur . . .* (Nürnberg, 1558). Photo courtesy of Burndy Library.

a broader and deeper sense than simply a record of scientific progress. There is a fine presentation throughout of the groping and progress needed to develop the scientific concepts so essential to Galileo's synthesis of mechanics. The literary style is excellent, and the scholarship is detailed and authoritative. The book is certainly a contribution to our



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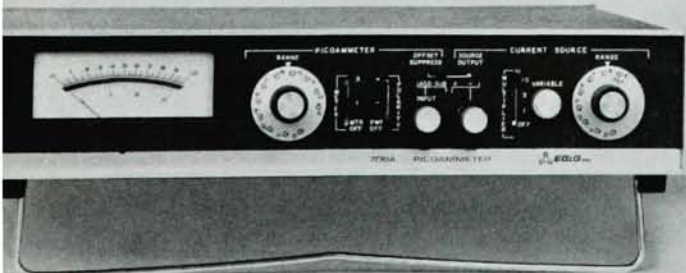
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The reviewer is with Case Western Reserve University, where he is Ambrose Swasey Professor of Physics.

## Emphasis on hard facts

PHYSICS OF PLANETS. (NASA-TT-F-515). By V. I. Moroz. 412 pp. NASA, Washington, DC, 1968. \$3.00

by ROMAN SMOLUCHOWSKI

There are few, if any, sciences that stir the imagination more than astrophysics. Even the length of articles in the *New York Times*, which actually is acquiring an enviable reputation as a "science journal," shows that the only peers of astrophysics in this respect are genetics and other biosciences. For the last ten years or so we have been bombarded with spectacular discoveries concerning either remote parts of the universe, which are populated by such mysterious objects as quasars, pulsars and John Wheeler's "black holes," or concerning our own familiar and much more easily identifiable solar system and its planets.

Unfortunately there are no recent books in English written on a reasonably advanced level dealing with physics of all planets. Some do exist on the popular side, such as the otherwise excellent series published by the National Aeronautics and Space Administration and edited by C. M. Michaux. Others encompass several volumes each written by many authors, which precludes continuity and uniformity of level, and there are also books that deal only with a few planets, like the recent (1968) and very good *Introduction to Planetary Physics* by W. M. Kaula.

The author of *Physics of Planets*, V. I. Moroz from the P. K. Sternberg Astronomical Institute in Moscow, has contributed widely to spectroscopic observations of nearly all planets. His present book is an excellent and compact introduction to the whole field of planetary physics. It starts with a good summary of basic concepts, tools and pertinent measurements, followed by chapters dealing with Mars, Venus, Mercury and the giant planets. There are a large number of illustrations, diagrams and over 600 references.

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JUPITER with red spot and shadow of the satellite Ganymede above.

The tone of the book would appeal to a skeptical observer; that is, the primary effort is placed on facts and on their evaluation, and only the most acceptable theories are expounded in some detail. This is a very welcome feature in a field where the ratio of hard facts to theories and hypotheses is probably even lower than in biosciences.

The main drawback is that the references do not go beyond 1965, and thus the book does not cover such exciting observations as F. J. Low's measurements of the thermal emission of Jupiter, newer data on the nature of the polar caps of Mars and of its surface composition and the recent controversy concerning the surface tem-

## A partisan view

QUANTUM THEORY OF MATTER. (2nd edition) By John C. Slater. 763 pp. McGraw-Hill, New York, 1968. \$15.00

by PHILIP L. TAYLOR

It is probably true to say that the quantum theory of matter is a subject that has broadened rather than deepened in the 18 years since the first edition of this text was published. Our current view of a crystal as a bestiary of elementary excitations has led to an understanding of many previously puzzling phenomena. On the other hand, our present knowledge of atoms and molecules, as well as of energy bands in solids, owes more to large digital computers that helped us develop concepts formulated in the early days of quantum mechanics.

In this new edition of his book, John

Slater has chosen not to follow the path of diversification, but has instead concentrated on enlarging his treatment of the topics covered in the first edition. Thus the first half of the book represents an introduction to quantum mechanics in the wave-mechanical-cum-historical tradition, and the second half discusses the application of the one- and two-electron Schrödinger equation to a large variety of molecules and solids. The discussion of molecular orbitals is particularly clear and extensive and includes descriptions of the ammonia, ethylene and benzene molecules. There are ample instructive problems at the end of each chapter.

Some readers may fault this text for its failure to mention any aspect of collective behavior or of those most ex-

\* \* \*

R. Smoluchowski is professor of solid state sciences at Princeton University and has been active in the part of astrophysics that deals with properties of condensed matter, especially the surfaces and the interior of the moon, Mars and Jupiter.

Slater has chosen not to follow the path of diversification, but has instead concentrated on enlarging his treatment of the topics covered in the first edition. Thus the first half of the book represents an introduction to quantum mechanics in the wave-mechanical-cum-historical tradition, and the second half discusses the application of the one- and two-electron Schrödinger equation to a large variety of molecules and solids. The discussion of molecular orbitals is particularly clear and extensive and includes descriptions of the ammonia, ethylene and benzene molecules. There are ample instructive problems at the end of each chapter.

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citing states of matter, the superfluid and the superconductor. I would be more inclined to accept this work for what it is—a partisan view of the theory of matter—and forgive its author for retaining the book's overly ambitious title. This new edition will be warmly welcomed by anyone who has enjoyed the earlier version, and will bear further witness to Slater's qualities as one of our most notable teachers.

\* \* \*

*Philip Taylor is associate professor of physics at Case Western Reserve University, and is the author of a forthcoming text on the quantum theory of solids.*

## Waves and lines

**LIGHT AND SOUND FOR ENGINEERS.** By R. C. Stanley. 344 pp. Hart Publishing Co., 1968. \$12.00

by ROBERT LINDSAY

This book, by a British author who is lecturer in applied physics at Brighton College of Technology, is an effort to provide a broader and deeper exposition of sound and optics than the typical British engineering student might be expected to obtain from his elementary physics course. The chapters devoted to geometrical optics give considerable attention to such often bypassed topics as thick lenses, aberrations and photometry as well as analyzing in more than usual detail some of the commonly encountered optical instruments.

The chapters on physical optics employ standard approaches to interference, diffraction and resolving power with a theoretical development based almost completely on the principles of superposition and the Huygens tradition. No mention is made of recent work in lasers and holography. The chapters on sound include the description of techniques for measuring the velocity of sound in solids, liquids and gases, a thorough but elementary treatment of the vibrating string and several resonance situations and a survey of architectural acoustics and ultrasonics.

Most US engineering curricula require three or four semesters of elementary physics. Existing texts already treat these subjects at a reasonable depth and it appears unlikely that a book at this relatively low level would be suitable as a regular text. It

does have some attractive features that would make it worthwhile for reference purposes, including a commendable neatness of organization, a clarity of exposition that takes nothing for granted and many meticulously drawn diagrams. To the teacher of elementary physics it would provide a good source of supplementary material, but for the practically minded engineer

and technician, who encounters problems involving light and sound, it has enough useful information that can be obtained quickly to make it a good place to look first.

\* \* \*

*Robert Lindsay is a professor of physics at Trinity College and has been teaching physics to science and engineering majors for 15 years.*

## Two aids for galactic research

**STELLAR KINEMATICS.** By W. M. Smart. 320 pp. Wiley, New York, 1968. \$12.50

**GALACTIC ASTRONOMY.** By Dimitri Mihalas, with collaboration of Paul McRae Routly. 257 pp. W. H. Freeman, San Francisco, Calif., 1968. \$10.00

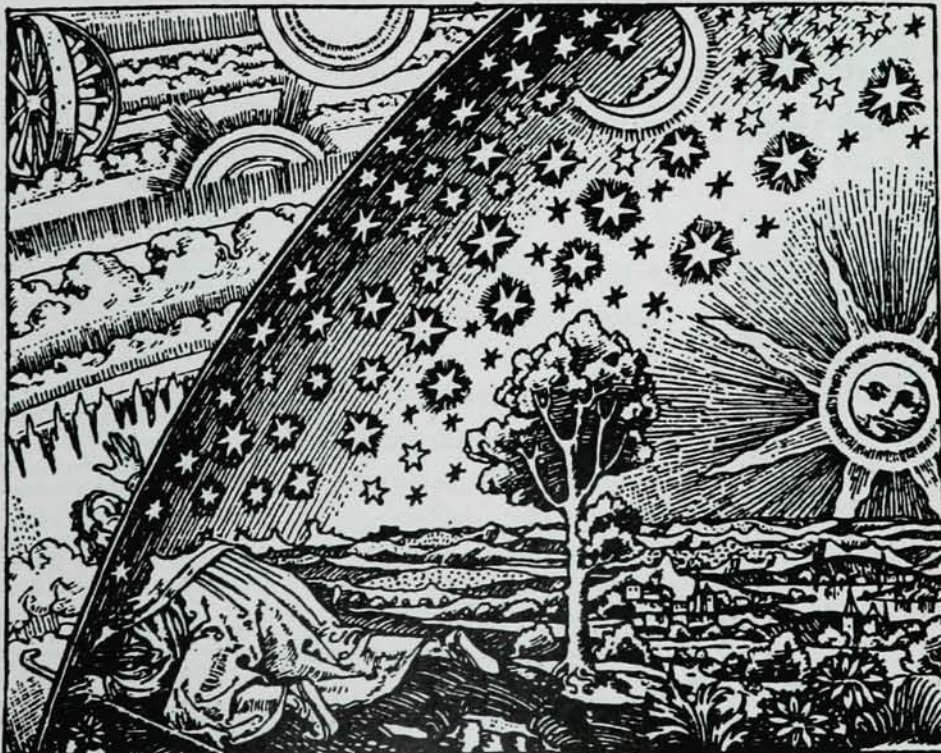
by KENNETH YOSS

These two books are useful additions to the sparse list in galactic research, which is receiving more attention with the recent availability of a new generation of modern observational equipment. Proper appreciation and analysis of the resulting data is essential, and these two books should aid in this increased activity.

Despite the first-glance similarity (six of eight chapters in one are on

the same topics as six of 14 in the other), the purposes are totally different, as are the levels of usefulness. One is a textbook for a first course in galactic structure, the other a detailed mathematical explanation of well known classical problems in stellar kinematics.

W. M. Smart is well known for his precise mathematical developments concerning problems in galactic structure. His *Spherical Astronomy* and *Stellar Dynamics* are classics, familiar to and often used by researchers in galactic structure. *Stellar Kinematics* is limited to basic problems concerning stellar motions, and many sections are modifications from *Stellar Dynamics*, which does not lessen its usefulness. Smart's attention to detail is



**MEDIEVAL COSMOLOGY.** Woodcut depicts traveler putting his head through the vault of the sky to discover the complexities that move the stars. (Photo taken from *Knowledge and Wonder* by Victor F. Weisskopf, Doubleday, 1966.)



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MALLORCA INTERNATIONAL SCHOOL OF PHYSICS, AUGUST 1969

Director, **L. M. Garrido**, *Professor of Theoretical Physics, University of Barcelona*

Edited by **A. Cruz**, *University of Zaragoza*  
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In an attempt to encourage new research and to consolidate progress made, eminent physicists discuss numerous aspects of the many-body problem. Invaluable as a state-of-the-art report on this vital topic, the book features papers by L. J. Boya, E. R. C. Caianiello, C. B. Dover, C. P. Enz, I. Fujiwara, L. van Hove, N. J. Horing, P. C. Martin, W. Thirring, and E. J. Verboven.

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## ELEMENTARY EXCITATIONS IN SOLIDS

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Edited by **A. A. Maradudin**, *Department of Physics, University of California at Irvine*

and **G. F. Nardelli**, *Gruppo Nazionale Struttura della Materia, C.N.R. and Physics Institute, University of Milan, Italy*

Reporting the latest advances in the field, this volume will be of great value to solid state physicists and crystallographers.

**CONTRIBUTORS:** A. A. Maradudin, G. F. Nardelli, W. Ludwig, M. Balkanski, M. F. Collins, A. J. Sievers, R. O. Pohl, R. J. Elliott, J. Callaway, P. Resibois, E. Burstein, J. J. Hopfield, G. Baldini, I. P. Ipatova, A. A. Klichikhin, R. F. Wallis, G. Chiarotti.

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again evident, and this book should prove invaluable to a worker concerned with proper procedure, the effect of observational errors and incomplete sampling on the results.

This concern for detail is in vivid contrast to his virtual omission of modern interpretation of the observations. For example, solar motion is treated in a classical manner; the cause of variation in solar apex for different stellar groups and the distinction between standard and basic solar motion are not mentioned. Chapters 3 and 4 are concerned with star streaming, which has historical and mathematical interest but little immediate practical use. The next chapter introduces the concept of ellipsoidal distribution of stellar-velocity vectors, but his discussion of its fundamental cause, found in the final chapter on galactic rotation, is very brief. He avoids such interwoven topics as stellar-density distribution and galactic dynamics so limiting the book to kinematic problems, as the title indicates.

Dimitri Mihalas is known for his fine work in stellar atmospheres, and he is to be admired for his motivation in writing *Galactic Astronomy*. A first text in galactic structure is needed, and this book goes far in filling the vacancy. It is regrettable that the first modern text has not been written by an experienced researcher in the field, however, because so much of its value depends on the proper evaluation of available observational data. Unlike *Stellar Kinematics*, discussion and interpretation of observations are included but at times should be more extensive.

The first three chapters in Mihalas's book are devoted to brief descriptive topics found in elementary texts, but these 45 pages should either have been omitted entirely or significantly expanded. The weakness of this section is exemplified in the discussion of errors in trigonometric parallaxes. His explanation for negative parallaxes is actually incorrect. It is regrettable that he did not reference more authoritative sources, such as Peter van de Kamp's *Principles of Astrometry* (W. H. Freeman), which deals in detail with problems of this type. From chapter 4 on, where he is concerned with specific details of stellar motions, galactic rotation and galactic dynamics, there is little to criticize. Mihalas writes well, and the book contains sufficient detail to introduce

the student to the concepts. Unlike Smart, he effectively discusses the currently important problems of galactic structure, such as interstellar absorption and the relation between stellar populations (ages) and motions (velocity ellipsoids). In many cases diagrams would have better conveyed the concepts than the extensive tables, most of which are unnecessary in a book of this type.

The difference in detail in the two books, which illustrates the basic difference in their purposes, is vividly depicted in the respective chapters on statistical parallax; Mihalas devotes six pages to it (a good length for a textbook), and Smart takes 36 pages.

Both books will remain useful for some time: *Stellar Kinematics* because it represents a rigorous mathematical approach to standard problems in the subject, quite independent of the constant, but slow, improvement and increase in observational data; *Galactic Astronomy* because it is presented in a readable form and includes most major topics of interest in the subject at a useful level for a first text in the field.

\* \* \*

Kenneth Yoss is an astronomy professor at the University of Illinois Observatory, Urbana, Ill.

## Beauty in the eye of the beholder

LAGRANGIAN DYNAMICS: AN INTRODUCTION FOR STUDENTS. By C. W. Kilmister. 136 pp. Plenum, New York, 1968. \$7.50

by GARRISON SPOSITO

In 1834, while in the process of delivering his own name onto the list of the immortals in physics, Sir William Rowan Hamilton wrote in celebration of the men who had created analytical mechanics. He singled out with obvious gratitude Comte Joseph Louis Lagrange as one who had "perhaps done more than any other analyst to give extent and harmony to such deductive researches, by showing that the most varied consequences respecting the motions of systems of bodies may be derived from one radical formula; the beauty of the method so suiting the dignity of the results, as to make his great work a kind of scientific poem."

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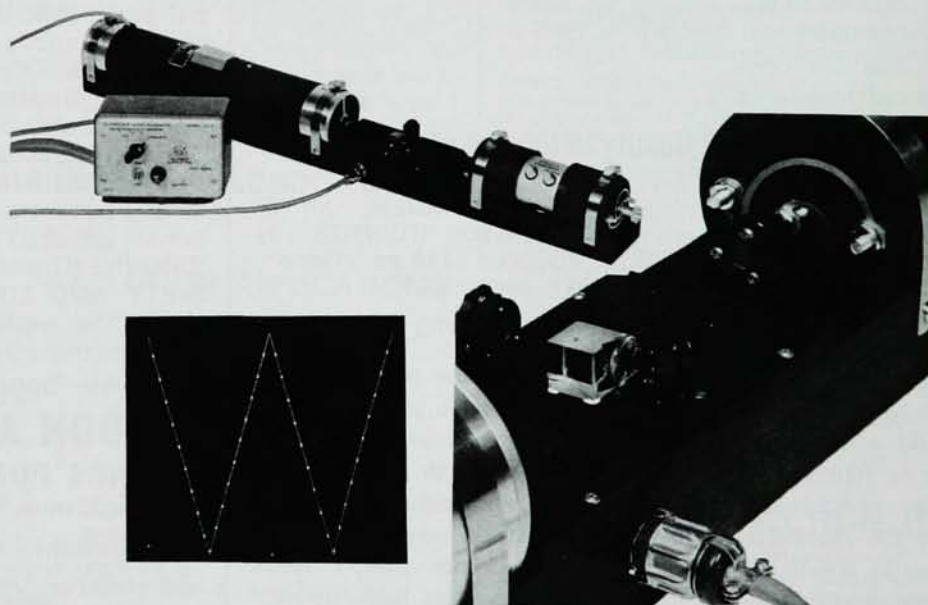
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sage of time. In remarkable analogy with its creator's undiminishing prestige as succeeding revolutions racked his adopted country, the Lagrangian method has stood impervious to the two great revolutions that have transformed dynamics in this century. The words written by Hamilton could in all respects have been written as well by Richard Feynman or Julian Schwinger.

It is no wonder then that one might wish to include at least a peek at Lagrangian dynamics in an advanced undergraduate course on classical mechanics. The problem is that such a peek has to be elementary but not superficial, and that this condition is difficult to meet in most textbooks without their becoming impossibly bulky. The solution to the dilemma, according to C. W. Kilmister, mathematics professor at King's College in London, is to add to the reading list a little volume such as his *Lagrangian Dynamics: An Introduction for Students*.

Kilmister's book contains six chapters, of which the third through fifth are involved directly with illustrations of the Lagrangian method. To be honest, one must say that these chapters will be largely incomprehensible to the reader who does not know fairly well the calculus of variations and vector analysis. Moreover the reader must have a feeling for, or at least a great tolerance of, the dynamics of rigid bodies, because the discussions deal solely with macroscopic systems subject to constraints.

In chapter 3, for example, we meet the symmetric top, a hoop (inside of which dangles a simple pendulum) a bell and clapper slightly idealized and a centrifugal governor. In the fourth chapter, on small vibrations, we face the double pendulum; in the fifth, on impulsive forces, we observe a rhombus of uniform rods collide with a wall. The character of these applications will likely preclude the use of the book by anyone who believes heartily that the notion of constraint is artificial in the present milieu of dynamics.

It is probably not without some value to remark that this book might have a special appeal to professors or students who prefer to see classical mechanics as applied mathematics rather than theoretical physics. The tone of the book is decidedly mathematical, and it achieves its finest form with the statement, in chapter 2, that "the reason why the anholonomic case can arise is now simply that not all

vector fields are families of normals to hypersurfaces." In the same sense one might add, with a twinkle in one's eye, that the reason why aperiodic oscillations in three-space can arise is that not all numbers are rational. Evidently beauty is indeed in the eye of the beholder.

\* \* \*

*An associate professor at Sonoma State College, California, the reviewer has taught courses on analytical dynamics for the past few years.*

## Highly coherent

**FUNDAMENTALS OF QUANTUM OPTICS.** By John R. Klauder and E. C. G. Sudarshan. 279 pp. W. A. Benjamin, New York, 1968. \$13.50

by MARVIN M. MILLER

Since the publication in 1963 of a series of papers by R. J. Glauber, the quantum theory of optical coherence has become an active area of research. However, with the notable exception of Glauber's 1964 Les Houches lecture notes, an authoritative account of the many interesting developments in this field has not been available in book form. The appearance of a monograph by two of the leading contributors in the field, J. R. Klauder and E. C. G. Sudarshan, is especially timely because of the importance of this research, and the fruitful application of the notion of coherent states to the study of problems outside the domain of quantum optics.

The first three chapters are devoted to a concise review of selected topics in classical-coherence theory and semiclassical-counting statistics. Chapter 4 considers the physical origin and treatment of coupled, nonlinear, partial differential equations with stochastic-driving terms, or stochastic-initial conditions or both. Although such equations arise in many physical contexts, this discussion has particular relevance in quantum optics, in view of the success of model-laser theories that describe the dynamics of the nonlinear interaction between the laser systems and reservoirs by means of fluctuation equations with Markoffian noise-source excitation.

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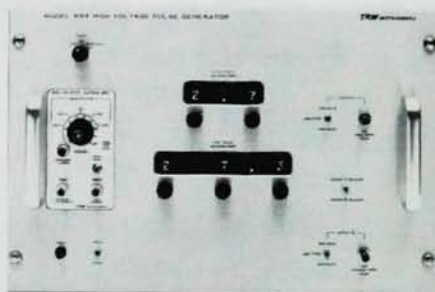
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magnetic field. Chapter 7 is a detailed account of the properties of the coherent states. Among the topics discussed are the completeness (apparently first noted by John von Neumann) and over completeness of these states, their relationship to Segal-Bargmann Hilbert spaces of entire functions and the differential-operator representation of the creation and annihilation operators.

Especially noteworthy is the discussion in chapter 8 of a particular coherent-state representation of the density operator that specifies the statistical state of the radiation field and is known in the literature as the diagonal or  $P$  representation. As Glauber has remarked, the question of the generality of this representation "lies in mathematically rather deep waters," and has provoked a fair amount of controversy and confusion in the past. The authors' rigorous formulation of the optical-equivalence theorem, and their discussion, particularly on page 192, of its physical implications should

prove valuable in illuminating the relationship between the quantum and semiclassical theories of optical coherence.

The last two chapters deal with special states of the radiation field and intensity interferometry in quantum optics. There is a discussion of various laser models and J. P. Gordon's interesting approach to the model developed by M. Lax is considered in some detail.

The level is suitable for advanced students and research workers in quantum optics. It is written in a clear style with a careful attention to mathematical and physical subtleties not often considered in the literature, and it is highly recommended to those who wish an authoritative account of recent work in this area.

\* \* \*

*The reviewer is assistant professor of electrical engineering at Purdue University specializing in quantum optics and electronics.*

## A review of reviews

ANNUAL REVIEW OF PHYSICAL CHEMISTRY, VOL. 19, 1968. H. Eyring, C. J. Christensen, H. S. Johnston, eds. 645 pp. Annual Reviews, Palo Alto, California, 1968. \$6.50

by E. E. MUSCHLITZ, JR

Volume 19 of *Annual Review of Physical Chemistry* is the 1968 edition of a long and successful series. The current volume contains 20 articles and presents the reviewer with a difficult task in doing justice to the efforts of all the authors involved. Physical chemistry includes a wide variety of topics, and the breadth of the subject is amply demonstrated by individual review titles in the volume. The reviews are well written and documented, most having 100 or more references and many having over 200.

Periodic short reviews of progress in active areas of research are of value not only to the experienced investigator but also to the graduate student. Especially for the latter, a good review article should be instructive as well as informative. Most of the reviews in this volume have achieved this objective.

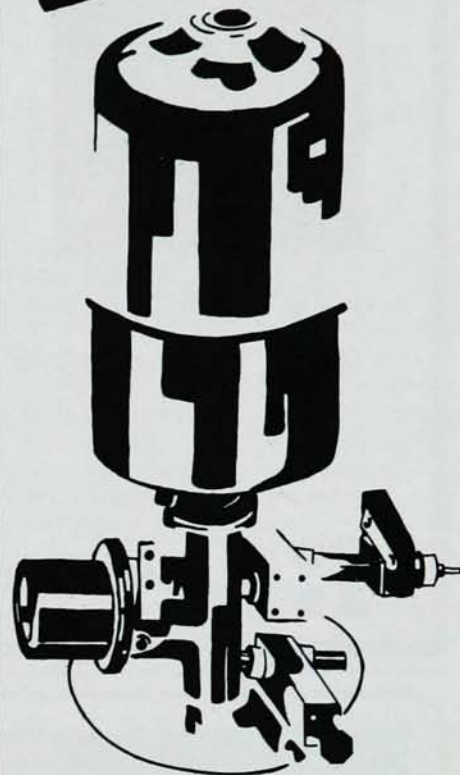
A. N. Frumkin and N. M. Emanuel of the USSR Academy of Sciences are the authors of an interesting survey,

"Fifty Years of Soviet Physical Chemistry," which heads the list of articles. This is followed by reviews on "Electric Paramagnetic Resonance" by Alan Carrington and Geoffrey Luckhurst; "Fused Salts" by S. J. Yosin and H. Reiss; "Electrochemistry" by Fred Anson (perhaps too broad a field for a short review article) and "Experimental Inorganic Thermochemistry" by W. N. Hubbard, P. A. G. O'Hare and H. M. Feder. Recent developments, particularly new experimental techniques, in studies of "Fast Reactions in Solution" are described by Edward Eyring and Bruce Bennion.

R. Henry and Michael Kasha have written a penetrating review on "Radiationless Molecular Electronic Transitions" in which they give a critical historical summary of the theory of these processes and develop the stationary-state approach to excited-state interactions of Rhodes, Henry and Kasha and of Jortner that eliminates radiationless transitions. "Ligand Substitution Dynamics" by Cooper Langford and Thomas Stengle is the next review, and it is followed by a thorough analysis of recent theory and experiment on "Vibrational and Rotational Relaxation" by Roy Gordon, William Klemperer and Jeffrey Stein-

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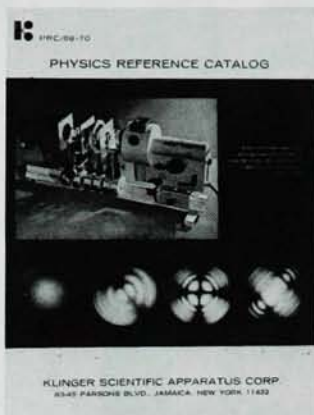


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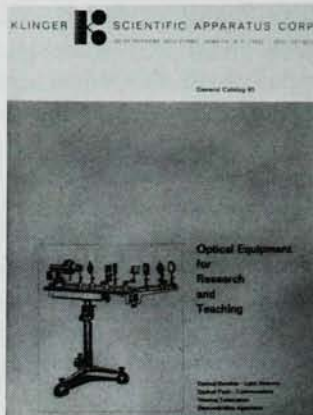


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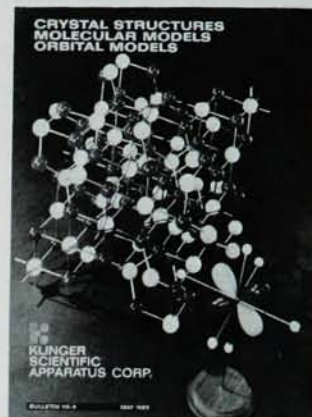
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feld. These authors have included several valuable tables collating the various systems that have been studied with the experimental methods.

Only in recent years with the advent of ultrahigh-vacuum techniques has it been possible to carry out surface studies on clean single-crystal surfaces. Gabor Somorjai reviews the experiments on surface structure, surface dynamics and gas-surface interactions in his article on "Surface Chemistry." Lewis Friedman's review on "Ion-Molecule Reactions" emphasizes the new experimental techniques with tandem mass spectrometers (measurements of the product-ion energies and angular distributions have recently been made for several reactions), ion cyclotron resonance for studies of reaction mechanisms and photoionization as a means of producing reactant ions in known internal-energy states.

The review "Mass Spectrometry" by Kenneth Rinehart Jr and Thomas Kinstle attempts to cover too large a topic for a short review article. The emphasis is on high-resolution mass spectrometry and structure of organic positive ions. Alan Haught writes a very instructive review on "Lasers and their Applications to Physical Chem-

istry." This is followed by reviews on "Gas Reactions Yielding Electronically Excited Species" by B. A. Thrush, "Statistical Mechanics—A Review of Selected Rigorous Results" by Joel Lebowitz, "Vibrational Spectroscopy" by Herbert Strauss and "Nuclear Magnetic Resonance" by J. Jonas and H. S. Gutowski.

D. W. Urry's review "Optical Rotation" is centered on applications to peptides and polypeptides. This is followed by an article on "Quantitative Conformational Analysis; Calculation Methods" by James Williams, Peter Stang and Paul Schleyer and one on " $\text{He}^3\text{--He}^4$  Solutions" by Norman Phillips.

The editors are to be congratulated on their selection of authors for this volume, for each is an acknowledged expert in his field. These authors have treated their subjects not only in a comprehensive but also a critical fashion. In the current era of a burgeoning literature, good reviews such as these are filling a role that is becoming more and more essential.

\* \* \*

*E. E. Muschlitz Jr is a chemistry professor and head of physical chemistry at the University of Florida.*

## Machine calculations

COMPUTING METHODS FOR SCIENTISTS AND ENGINEERS. By L. Fox and D. F. Mayers. 255 pp. Oxford Univ. Press, New York, 1968. \$6.25

by NORMAN A. BAILY

The authors, members of the Oxford University Computing Laboratory, state that the primary purpose of this book is to enable its users to improve their use of the computer and to obtain more accurate and meaningful solutions. If one restricts its application to that of a handbook, it should have no difficulty in achieving the authors' aims. However, the mathematics are complex enough that even physical scientists who are primarily experimentalists would have to spend considerable time studying the suggested methods to determine the proper one for a particular problem.

The field of automatic computation is of prime importance in all branches of science, and the book emphasizes the proper selection of methods for the numerical solution of many different mathematical forms. The book,

therefore, makes a very valuable contribution because a vast majority of computer users have not ordinarily delved deeply into the problems discussed. Sections of the book are quite sophisticated and possibly would be difficult for the occasional machine user to apply properly. It is specifically designed for persons thoroughly familiar with computing but who perhaps do not have either the training or experience to obtain the best results. In general, the book is an excellent review of the methods for handling common difficulties.

Some of the more important topics covered are: error analysis, floating-point arithmetic, recurrence relations, finite differences and the usual common operations such as polynomials, matrices and numerical integration. The authors have stressed the important difference between inherent and induced stability and have treated fundamental theory where they felt that it was not well known by most computer users. The book does not em-

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phasize derivations but rather provides detailed procedures for testing and arriving at a specified computational error that might be caused by rounding errors and degree of approximation. Even though its treatment is limited, the book provides a much needed compendium of computational methods applicable for the solution of many common problems.

\* \* \*

*The reviewer is with the University of California, San Diego, and is a machine user both for the numerical evaluation of theoretical expressions and for the practical applications of radiation dosimetry.*

## NEW BOOKS

### ELEMENTARY PARTICLES

**Phenomenological Theories of High Energy Scattering: An Experimental Evaluation.** By Vernon D. Barger and David B. Cline. 201 pp. W. A. Benjamin, New York, 1969. Cloth \$15.00, paper \$6.95

**Theory and Phenomenology in Particle Physics, Part A and B.** A. Zichichi, ed. 315 pp. Academic, New York, 1969. \$14.00

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**Francis Bitter, Selected Papers and Commentaries.** T. Erber and C. M. Fowler, eds. 551 pp. MIT Press, Cambridge, Mass., 1969. \$20.00

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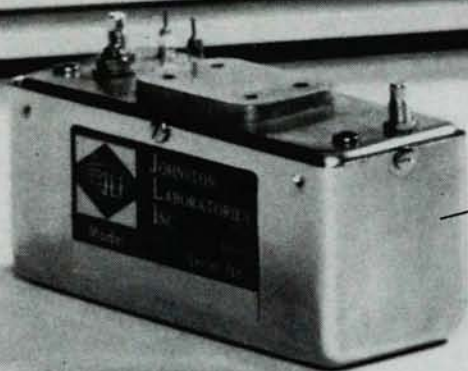
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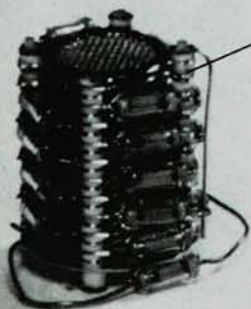
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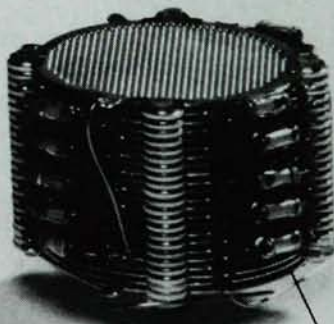
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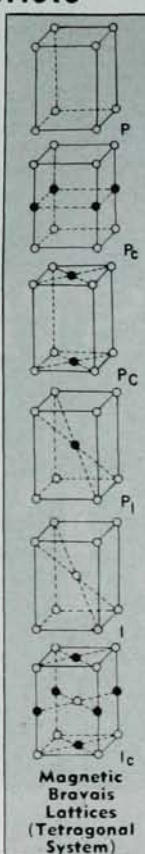
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**Physics Literature: A Reference Manual.** Robert H. Whitford, ed. 272 pp. Scarecrow Press, Metuchen, N. J., 1968. \$8.50



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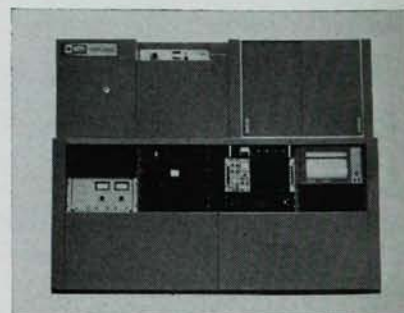
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## Normal-State Electron Tunneling Only Qualitatively Understood

From an experimentalist's point of view, the field of electron tunneling owes its present lively state to the discovery of the p-n tunnel diode by Leo Esaki in 1957 and of tunneling through oxide layers by John C. Fisher and Ivar Giaever in 1960. The results in the oxide system became even more remarkable when the electrodes against the oxide were made superconducting by Giaever in 1960. In the five years that followed, a happy combination of theory and simple experiments led to confirmation of the Bardeen-Cooper-Schrieffer gap and square-root singularity in the electronic density of states, the fascination of the Josephson effect and the measurement of the details of the electron-phonon interaction.

However, in the past two years, interest has again cycled to p-n diodes, metal-semiconductor conductor contacts and metal-insulator-metal (M-I-M) junctions in the normal state. As a result a conference on nonsuperconducting electron tunneling was held at Prouts Neck, Maine, during 3-5 Sept. The meeting was arranged in the style of a Gordon conference with morning and evening sessions. In keeping with the Gordon conference tradition, no further publication of proceedings is contemplated. Previous conferences, which considered tunneling into both superconducting and normal electrodes, were held at Philadelphia (1961) and Risø (1967).

The result of this conference can be summarized briefly: Tunneling in normal systems, for experimentalists and theorists alike, is in some trouble unless one is satisfied with a purely qualitative understanding of the field. Remembering the successful application of tunneling to superconductivity, we may find this conclusion surprising. The origin of the difficulties of the normal state was summarized by Doug Scalapino (University of California, Santa Barbara) in his impressions at the end of the conference. The superconducting experiments probe properties of the electrodes over distances comparable to the coherence length, generally large enough to sample bulk effects (maybe not in the

case of transition metals and type-II materials), whereas the normal-state experiments are affected by the nature of the tunnel barrier, sometimes only a few atom layers thick, and by the metal electrodes within a screening length of the oxide-metal interface. Thus tunneling has become a problem of surface physics.

The first topic dealt with at the conference was: How well can the overall conductance-versus-voltage dependences be explained by single-particle tunneling theory? Next, interactions of the tunneling electron with the oxide, or impurities or particles in the oxide, led to discussion of "zero-bias anomalies." Finally, observations of interactions within the electrodes, the many-body or self-energy effects, were reported and the theory of these effects received considerable discussion.

**Single-particle tunneling.** The calculation of single-particle tunneling currents through a potential barrier requires an exact knowledge of the barrier potential as a function of distance. As Gerald Mahan (University of Oregon) pointed out in the opening talk, this is poorly known in p-n diodes and only guessed at in M-I-M junctions; therefore, the metal-semiconductor contact (Schottky barrier on degenerate material) has received the most attention recently. He showed that a calculation of the tunneling current could be made, based on uniform charge density in the depletion region, which results in a parabolic potential barrier. Experiments in which the barrier height and thickness are determined by independent measurements give an absolute conductance in "better than an order of magnitude" agreement with the calculation. The experiments also show the correct voltage dependence of the conductance. This agreement holds only when the surface-barrier contacts are made by cleaving the semiconductor in vacuum. Mahan's gloom with respect to M-I-M junctions was questioned by Carver Mead (Cal Tech) who presented a detailed investigation of aluminum-aluminum-nitride metal junctions. Combining capacitance

and current-voltage measurements on a series of junctions with different nitride thicknesses, he and collaborators have determined the  $E$  versus  $k$  relationship for the electron over the whole of the forbidden gap of the insulator. This result raised the inevitable question: What "band structure" can we associate with such thin layers, and is the insulator crystalline or amorphous? The extension of such careful analysis to other systems will be of interest.

For M-I-M systems, the tunneling conductance at low voltages (less than 200 mV) is not constant but has a roughly parabolic dependence on voltage. As reported by J. M. Rowell (Bell Labs), calculations based on simple trapezoidal barriers also show that the minimal conductance only occurs at  $V = 0$  for symmetrical barriers. However, no comparison of calculated and measured conductance has been made for junctions with barrier parameters determined independently.

Returning to an older barrier problem, phonon emission in p-n diodes, Charlie Duke (University of Illinois, Urbana) concluded that the theory of Kleinman offers a good description of the effect. New measurements of such phonon-assisted tunneling in a very wide gap material with complex lattice dynamics, silicon carbide, were reported by Phil Stiles (IBM).

**Impurities.** Although most oxide junctions contain unknown impurities, the addition of intentional impurities to the barrier is a relatively recent development. Two talks on the very interesting effects of adding metallic particles were given by Hansrudi Zeller (GE) and John Lambe (Ford). Although different in concept and results, the two experiments both raise a puzzling question. In the work described by Zeller (performed in collaboration with Giaever) the well known agglomeration of very thin metal films is used to introduce an array of particles (about 10 nm or less in diameter) into the oxide of a tunnel junction. The current flows as electrons tunnel to the particles, localize, and then tunnel to the other electrode. However, if a particle is about 5 nm in diameter,



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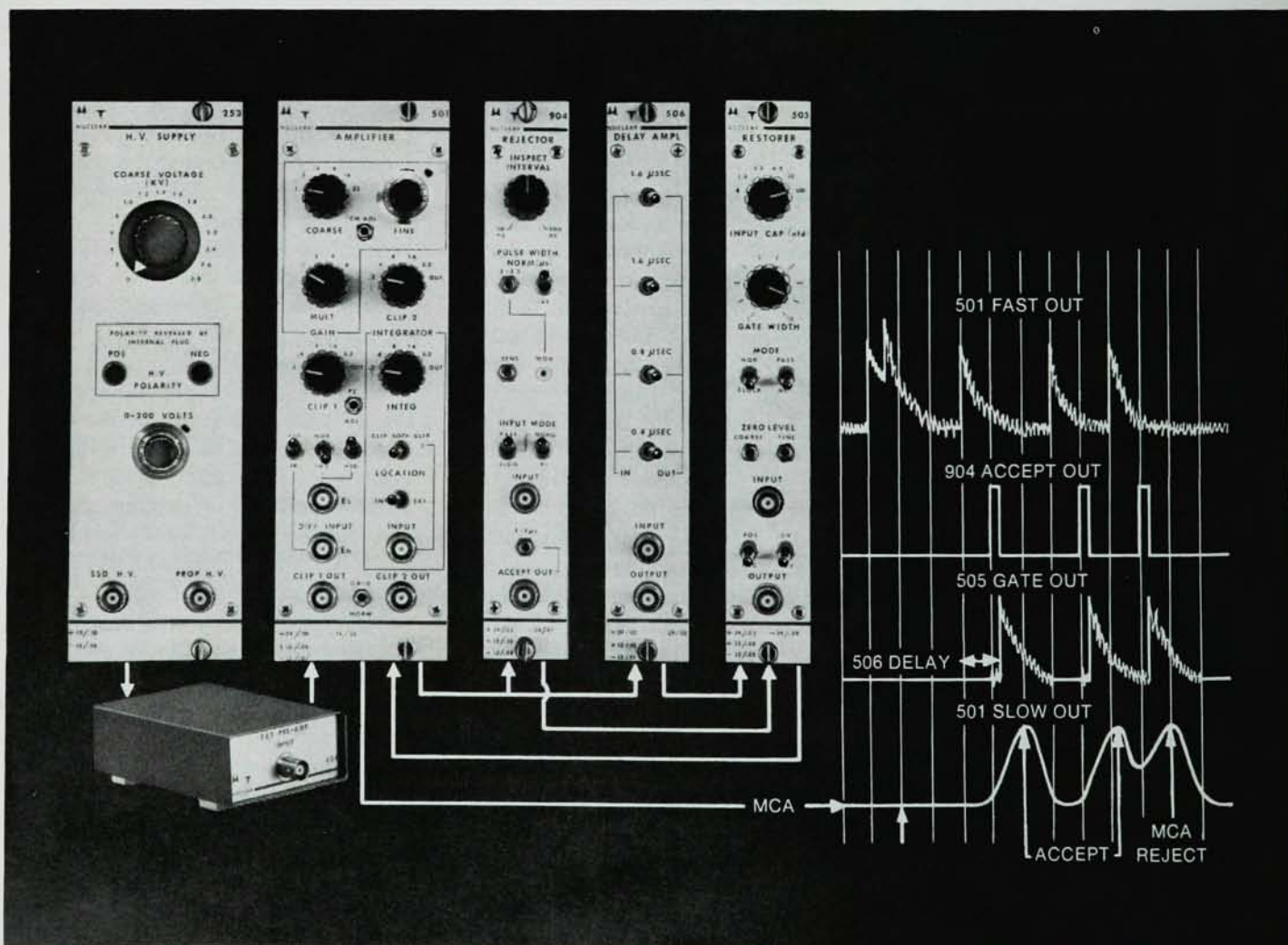
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the addition of one electronic charge requires a charging energy ( $\Delta_c$ ) of approximately 10 meV. For a single particle the junction conductance would show a step at  $\Delta_c$ . For a distribution of particles the conductance rises rapidly as a function of voltage; that is, the resistance shows a strong peak at  $V = 0$ . Zeller frequently pointed out that this picture can be generalized to explain all "zero-bias resistance peaks," by claiming that "states" exist in the barrier with a density given by  $dG/dV$ . This explanation, of course, is possible, but it appears dangerous to assume that it is always correct, and hence to lose interest in the problem. For example, an alternative explanation of the conductance dip near  $V = 0$  in metal-semiconductor contacts involves the excitation of phonons in the semiconductor depletion layer.

The theory of Duke and others was compared to experiment by Tom Caruthers (University of Chicago) and, although the energy range of the observed structure is correct, a disagreement in line-shape was apparent. Further discussion of the various excitation processes observable in metal-semiconductor contacts was given by Matthew Mikkor (Ford) and William Thompson (IBM). The possibility of observing organic impurity vibrations was a point of disagreement in these two talks.

Let us return now to the physics of particles. A small globule brought close to a metal electrode will, by tunneling, lose or gain electrons until its highest filled electron level is within the charging energy ( $\Delta_c$ ) of the Fermi level in the electrode. In order to explain the data Zeller assumes that, over all the particles of a given size, the highest filled level is uniformly distributed within  $-\Delta_c$  to  $+\Delta_c$  of the Fermi level. In other words, there is no preferred alignment of the particle level with the electrode, because partial electronic charge cannot be exchanged. However, it is just such an alignment that is essential to the new work described by Lambe. He and Bob Jaklevic (Ford) studied the metal-oxide-particle-oxide-metal system where one oxide is too thick to permit tunneling. The properties of the device are probed using capacitance measurements, that is by making electrons hop on and off the particle through the thinner oxide. The resulting capacitance-voltage depen-

dence, which shows symmetrical structure about  $V = 0$ , is explained on the basis of some degree of alignment of particle "Fermi level" with that of the electrode. Even more dramatically, if a voltage (or series of voltages) is applied to the device at room temperature and maintained during cooling, then at low temperatures the capacitance-versus-voltage structure is removed from  $V = 0$  and shifted to the "forming" voltage (or voltages). This result implies that realignment of particle and electrode "Fermi levels" is induced by the applied voltage. The necessary transfer of partial charge to the particle is achieved by "polarization" of the oxide. Although details of this polarization were not understood, results described by John Adler (University of Alberta) may be relevant. In a study of the excitations of molecular impurities in aluminum-oxide tunnel junctions he found that the relative strengths of the various vibrational modes could be changed by applying a voltage to the junction at room temperature. If this change implies a motion, or rotation, of polarized molecules then it is equivalent to rearrangement of charge in the oxide.

So far, all tunneling layers between metal films have been thermally grown oxides. However, Giaever described his fabrication of junctions using evaporated semiconductors such as germanium, zinc sulfide and cadmium sulfide. By oxidation, any pinholes in the semiconducting layer were filled with oxide of the base metal. That tunneling was taking place through the semiconductor was confirmed by observing conductance structure at the correct energy for excitation of LO phonons in the semiconductor. In the case of cadmium sulfide Giaever showed that the tunneling characteristic could be changed by shining light on the junction; a "tunable tunneling matrix element."

**Zero-bias anomalies.** As mentioned above, the question of zero-bias anomalies was discussed frequently at the conference. One of the best understood of these is the conductance-peak anomaly. This anomaly is categorized by a conductance obeying the law

$$G \propto \log \frac{(eV)^2 + (k_B T)^2}{E_0^2}$$

where  $eV$  is the voltage,  $k_B T$  the temperature, and  $E_0$  a cut-off parameter. The conductance is also strongly dependent on magnetic field. An explanation for this effect had been ad-



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vanced by Appelbaum and Anderson, based on the electron spin-flip scattering of magnetic impurities in the barrier region. David Losee and Edward Wolf (Eastman Kodak) presented data on a number of different vacuum-cleaved degenerate semiconductor Schottky-barrier junctions, in which they ascribe the origin of the magnetic impurities in their systems to the neutral donors at the edge of the depletion layer. They found good agreement between their data and the Appelbaum theory if they suitably extended the theory to include the lifetime broadening of the magnetic level as well as a  $g$ -shift. Work on these anomalies in metal-doped insulator-metal junctions was reported by Adrian Wyatt of Nottingham University and also Paul Nielsen of Chicago.

**Many-body effects.** The influence of many-body effects on nonsuperconducting electron tunneling generated considerable discussion. In the past, tunneling has been a powerful probe of the many-body interactions in superconductors. This is because of the strong momentum dependence of the electron self-energy in a superconductor, which makes the structure seen in the conductance large, and the superconductor's large coherence length, which makes the superconducting wave functions near the metal-oxide interface only weakly dependent on the details of this surface. Both these effects no longer operate in nonsuperconducting tunneling. There the self-energy is predominantly frequency dependent, leading to small (1%) structure in the conductance. The effective "coherence length" is the order of the Fermi wavelength, so that the exact form of the metal-oxide interface (or semiconductor-Schottky barrier depletion layer) has an important influence of the structure one sees on the conductance.

Craig Davis (Ford) presented work, done in collaboration with Duke, on the influence of the electron self-energy, (resulting from the electron-optical phonon interaction in semiconductors) on the conductance of Schottky barriers. The self-energy in this case is purely frequency dependent. He emphasized that no structure in the conductance would be predicted unless the momentum dependence of the tunneling matrix element is taken into account. This momentum dependence is uniformly ignored

in superconducting tunneling; so we see again the important difference between the two types of tunneling.

The standard approach to tunneling calculations, the tunneling Hamiltonian, came under attack in work presented by Joel Appelbaum and Bill Brinkmann (Bell Labs). They argued that the tunneling Hamiltonian predicts the incorrect form for the transition matrix elements because it first calculates the coupling between the electrodes and then considers the influence of the many-body effects. To rectify this problem they proposed a theory that considers the transition rate between exact many-body states of the electrodes. If the transition rate is calculated by the WKB approximation, they find they can recover the conventional formula for the current, but with the transition matrix element replaced by one that is predominantly frequency dependent. In general, they find that the current depends on the electron Green's function in the vicinity of the barrier. They showed, for the particularly simple example of the electron interacting with magnetic impurities (zero-bias conductance peak), that the size as well as the sign of the zero-bias anomaly depends on the relative position of the impurity and the junction interface.

The theoreticians therefore concluded that the surface can have a profound influence on the self-energy effects observed in the conductance of metal-insulator-metal junctions. It was also obvious that experimentally great variations in junction properties (presence of zero-bias conductance peak, for example) are obtained by altering oxidation procedures. This result indicates that, in future, tunneling experiments must be increasingly tied to surface studies of the metal electrodes, with such tools as low-energy electron diffraction, field emission, Auger spectroscopy and optical studies.

\* \* \*

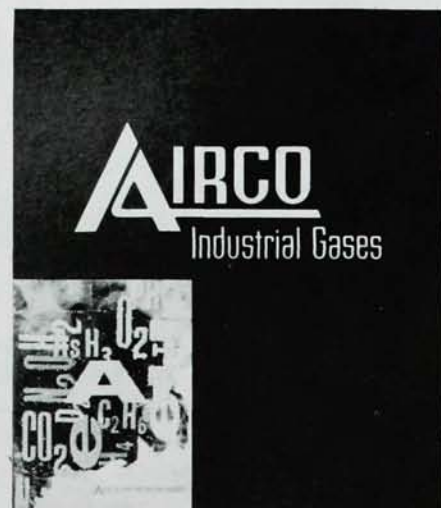
*The conference was sponsored by the Ford Scientific Laboratory, by the National Science Foundation, and by the Air Force Office of Scientific Research. As stated above, no proceedings are to be published, but those interested in further reading on the subject will find an excellent up-to-date review in: C. B. Duke, Tunneling in Solids, Academic Press, New York (1969) (Solid State Physics, Supplement 10).*

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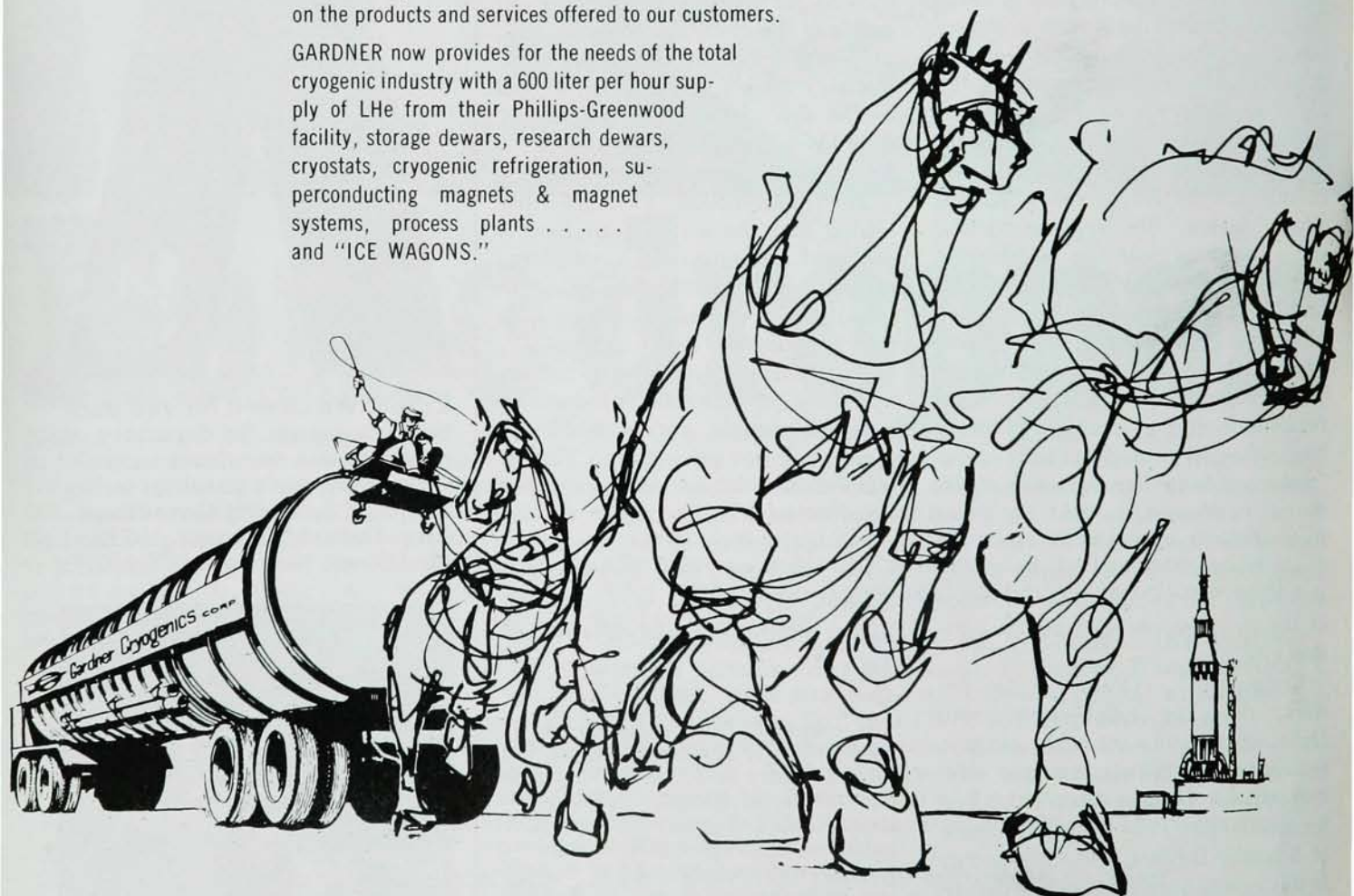
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VAN VLECK

**John H. Van Vleck** has retired from Harvard University and is now Hollis Professor of Mathematics, Emeritus. Particularly well known for his work in magnetism and quantum theory of atomic structure, Van Vleck was instrumental in creating the division of engineering and applied physics at Harvard. In 1952 Van Vleck was president of the American Physical Society, and he has served as vice-president of both the American Academy of Arts and Sciences and the International Union of Pure and Applied Physics. Among the awards he has received are the National Medal of Science, the Michelson Award of the Case Institute of Technology and the Langmuir Prize of the American Physical Society.

**Kenneth Fox** has returned to the University of Tennessee as assistant professor after a two-year leave as a National Academy of Sciences senior postdoctoral research associate. Fox spent the two years at the Cal Tech Jet Propulsion Laboratory.

Two Colgate University physicists, **James N. Lloyd** and **Charles H. Holbrow**, are on leave for the current academic year. Lloyd, an assistant professor, is at the University of Maryland, and Holbrow, an associate professor, is at Stanford.

**Miles E. Anderson**, professor of physics at North Texas State University, has been appointed associate vice president for academic affairs. **James R. Sybert** is now chairman of the physics department, **H. James Mackey** has become professor, and **James A. Roberts** and **Thomas J. Gray** are associate professors. **R. Muthukrishnan** of Michigan State University has joined the department as assistant professor.

**George C. Weiffenbach** has been named director of geoastronomy programs at the Smithsonian Astrophysical Observatory. In this new post,

Weiffenbach will be responsible for optical-laser tracking, long-base interferometry and maser-clock space experiments. Weiffenbach was formerly supervisor of the space research and analysis branch of the applied physics laboratory at Johns Hopkins. **Harri-son E. Radford** has also joined the observatory staff. Radford was formerly a molecular physicist with NBS, and will establish a laboratory at the observatory to study molecules known to exist in interstellar space.

**John A. Davies** has been promoted to associate professor in the physics department at Clark University. **William R. Fehlner**, formerly at the University of Illinois, is now assistant professor at Clark.

**Russell G. Groshans** has been appointed staff engineer for product engineering at RCA. Groshans, who was a systems engineer at RCA, Hightstown, had been a US Air Force physicist until 1967.

**Warren Proctor** has been appointed manager of market development laboratories for the Varian analytical instrument division. Proctor was professor of physics at the University of Washington until he joined Varian in 1955.

**James T. Shipman** is the new physics chairman at Ohio University, and **Roger W. Finlay** and **David S. Onley** were promoted to professor. **Jacobo Rapaport**, who had been at Oak Ridge National Laboratory, was appointed associate professor.

State University of New York at Binghamton has two new assistant professors of physics. They are **Noel Yeh**, formerly at Columbia, and **Robert Pompei**, who had been a research associate at Binghamton.

**Robert G. Breene Jr.** has been appointed as professor and **Mohindar S. Seehra** as assistant professor at West Virginia University. Breene was formerly with Physical Studies, Inc., and Seehra was at the University of Rochester. The physics department also announced that **T. Tietz**, chairman of

the department of theoretical physics at the University of Lodz, Poland, will be visiting professor for 1969-70.

**Frank J. Blatt** is acting chairman of the Michigan State University physics department, succeeding **Sherwood K. Haynes** who will remain in the department. New members of the Michigan faculty include **B. Hobson Wildenthal**, formerly of Texas A & M University, who will be associate professor and **William P. Pratt**, formerly of Los Alamos Scientific Laboratory, assistant professor. **Rubby Sherr** of Princeton and **F. C. Barker** of Australian National University will spend this year as visiting professors at Michigan State.

New director of the University of New Hampshire Space-Science Center is **William R. Webber**. Webber, formerly at the University of Minnesota, succeeds **Lawrence Cahill**.

**Langdon T. Crane** has been named research professor and director of the Institute for Fluid Dynamics and Applied Mathematics of the University of Maryland. Crane was formerly program director for atomic and molecular physics at the National Science Foundation. **Frank W. S. Olver**, formerly of the National Bureau of Standards, was also named research professor at the institute, while **R. Bruce Kellogg**, **Herbert Lashinsky** and **Thomas D. Wilkerson** were promoted to that position. **James Yorke** was promoted to research associate professor.

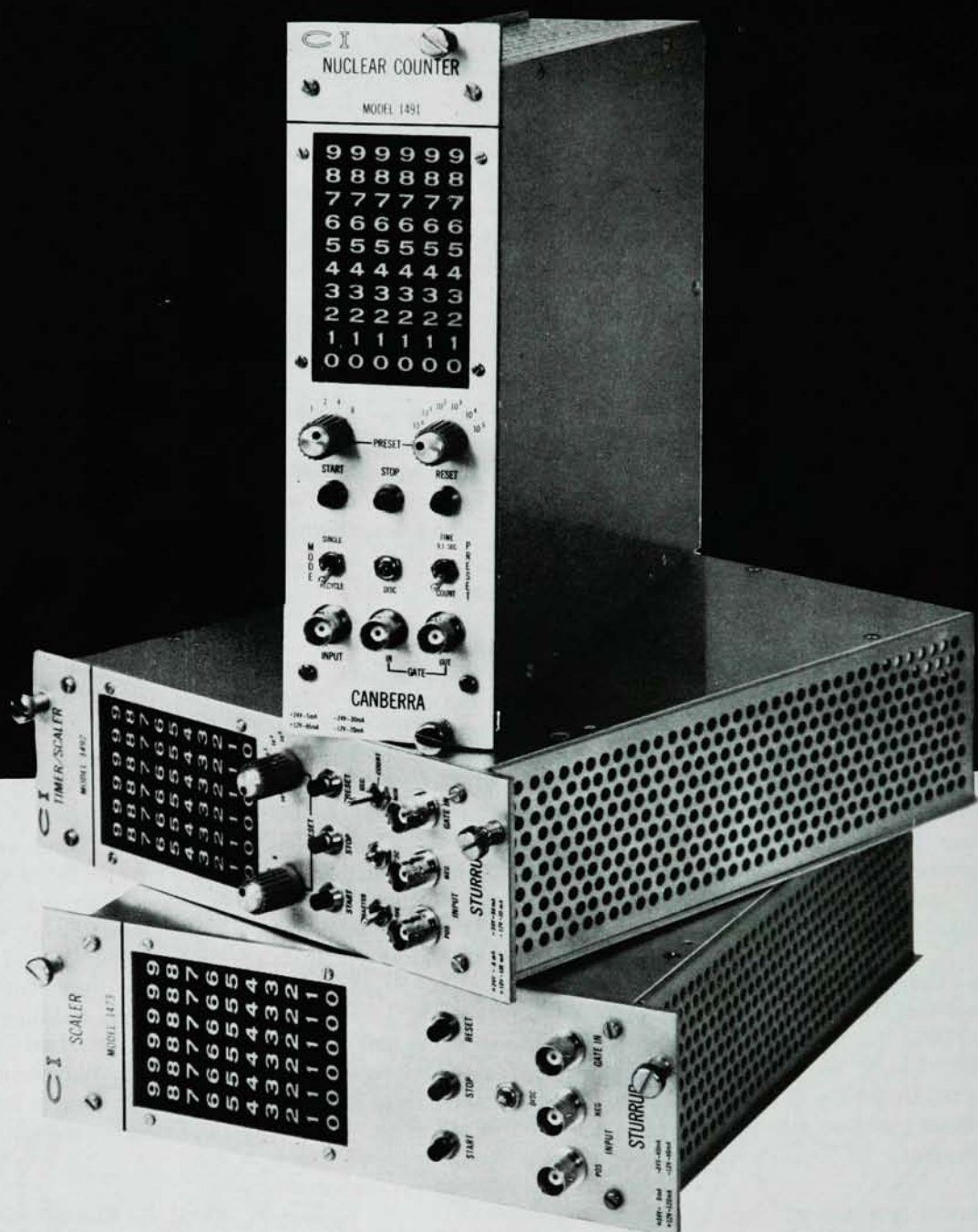
**Virgil B. Elings**, on leave from the University of California, Santa Barbara, will spend this year as a senior research scientist at Sigmatron, Inc. in Santa Barbara.

Yale University announces that **Daniel E. Rosner** is associate professor of engineering and applied science. Rosner was formerly with the Aerochem division of Sybron Corporation.

Florida State University announces that **Edward Desloge** and **Steve Edwards** have been promoted to professor and **James Skofronick** and **Gerald Speisman** to associate professor. An-



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thony Colleraine, who was at the University of Maryland, has become assistant professor at Florida State.

### Piel of Scientific American Wins Arches of Science Award

Gerard Piel, publisher of *Scientific American*, is the 1969 recipient of the Pacific Science Center Arches of Science Award. The \$25 000 awards are given for contributions to the public understanding of what science means to man. In 1947, Piel, along with Dennis Flanagan and Donald H. Miller Jr, acquired *Scientific American* and began to revitalize the magazine, which now has a circulation of more than 400 000.

Ronald J. Sladek is acting head of the Purdue University physics department. Sladek succeeds Richard W. King who died 12 Aug. (PHYSICS TODAY, October, page 105)

The physics department of the University of Virginia, Charlottesville, will have a new chairman, Judah M. Eisenberg, as of February. Robert V. Coleman is acting chairman until Eisenberg returns from his sabbatical leave at Tel Aviv University. W. Dexter Whitehead, former chairman of the department, has become dean of the Graduate School of Arts and Sciences at the university, but will continue as professor of physics and director of the Center for Advanced Studies. Several other changes have occurred at the physics department: Vittorio Celli was promoted to professor; Michael Coopersmith, formerly of Case-Western Reserve University and Dieter Drechsel, formerly of the University of Frankfurt, have been appointed associate professors; John Ruvalde, of the University of Chicago and Stephen Thornton of the University of Wisconsin are new assistant professors.

Jerry L. Peacher and Alexander O. Animalu are new assistant professors at the University of Missouri, Rolla.

Stevens Institute of Technology has promoted Earl L. Koller, a particle physicist, to professor. Edward A. Friedman, Bela M. Mecs and Norman J. Horing have been promoted to associate professor.

The MIT instrumentation laboratory will have a new director on 1 January, Charles L. Miller, now head of the civil-engineering department. The present director, Charles Stark Draper, will continue to serve as senior adviser and director of major projects. Draper, who founded the instrumentation laboratory, retired from the MIT faculty two years ago. The laboratory will be renamed the Charles Stark Draper Laboratory in recognition of his contributions.

William O. Statton has been promoted to professor of materials science and engineering at the University of Utah.



KNOX

Robert S. Knox has succeeded Morton Kaplon as head of the physics and astronomy department at the University of Rochester. Kaplon will remain as professor. Joseph H.

Eberly and Thomas Ferbel were promoted to associate professor, and G. Badhwar and P. Slattery are now assistant professors. John Krizan of the University of Vermont is a visiting associate professor this year.

Michael J. Moravcsik has succeeded Marvin D. Girardeau Jr as director of the Institute of Theoretical Physics of the University of Oregon. Girardeau is on sabbatical leave.

Promotions in the physics department at Indiana University include Walter E. Bron and Guy T. Emery to professor and Delbert W. Devins and Richard M. Heinz to associate professor. New assistant professors are Lloyd L. Chase, Shu-Yuan Chu, A. W. Hendry and Peter Schwandt.

Robert Vessot and Martin Levine have joined a newly formed research group at the Smithsonian Astrophysical Observatory to develop and adapt hydrogen maser clocks for space and geophysical applications. Vessot and Levine were formerly at Hewlett-Packard, Beverly, Mass.

New additions to the physics faculty of Southern Illinois University at Edwardsville are Hadi H. Aly, visiting professor, from the American University of Beirut; Ika-Ju Kang, associate professor, from Southern Illinois at

Carbondale; Thomas O. Baldwin, associate professor, from Oak Ridge National Laboratory; and Padmanabha Narayanaswamy, assistant professor, from the American University of Beirut.

Irvin A. Miller, assistant professor of physics, has been named acting dean of the Drexel Institute of Technology College of Science.

At the University of Wisconsin nuclear-engineering department Harold K. Forsen has been promoted to professor and John M. Donhowe and Wesley K. Foell have been promoted to associate professor. Charles W. Maynard will spend this year on leave at the Sandia Corporation.

New York University School of Medicine announces the promotion of Bernard Altshuler to professor of environmental medicine.

The physics department at Illinois Institute of Technology announces that Porter Johnson, formerly of Case-Western Reserve, is now assistant professor, and Val R. Veirs, a recent graduate of IIT, is visiting assistant professor. Robert L. Warnock and Thomas Erber have been promoted to professor and Frederick J. Ernst to associate professor. A former visiting associate professor, Fritz Herlach of EURATOM, is now associate professor, and a former visiting assistant professor, Cheuk-kin-Chau, is now assistant professor.



HAMERMESH

New physics department chairman at the State University of New York, Stony Brook, is Morton Hamermesh. Hamermesh had been head of the school of physics and astronomy at the University of Minnesota since 1965; Walter H. Johnson is now acting chairman at Minnesota. Hamermesh, a theoretical nuclear physicist, is a fellow of the American Physical Society and was on the board of trustees of Universities Research Associates while at Minnesota.

Alexander J. Dessler has been appointed science adviser to the executive secretary of the National Aeronautics and Space Council; the council is a US



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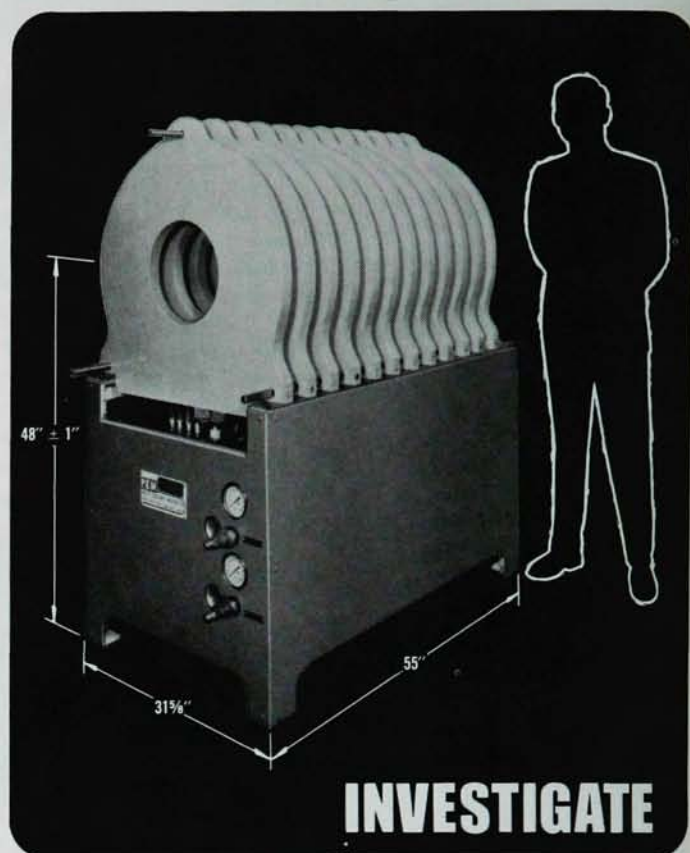
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government advisory group headed by **Spiro Agnew**. Dessler has been chairman of the Rice University space-science department since 1963.

### AEC Cites Three Men for Outstanding Contributions

The Atomic Energy Commission has awarded Lauriston S. Taylor, George B. Darling and Paul M. Gross citations for outstanding service to the national atomic energy program. Taylor, special assistant to the president of the National Academy of Sciences, is honored for his work in radiation protection; Darling's award is for his studies on the delayed effects of radiation on men (he is director of the Atomic Bomb Casualty Commission in Hiroshima); Gross, who helped organize the Oak Ridge Institute of Nuclear Studies and is president of Oak Ridge Associated Universities, is being honored for his work at Oak Ridge.

Visiting professor at the University of California, Riverside, this fall is **Richard J. Eden** of Cambridge University.

**Kenneth C. Clark** is the new program director for aeronomy in the atmospheric sciences section, division of environmental sciences of the National Science Foundation. Clark, a geophysicist, is on leave from the University of Washington.

**William S. Porter** has been promoted to professor at Southern Connecticut State College. **John W. Snyder** of Ohio State University and **Lee T. Matthews** of the University of Vermont are new assistant professors.

**L. Eric Cross**, professor of electrical engineering, and **Heinz K. Henisch**, professor of physics, have been appointed associate directors of the Materials Research Laboratory at Pennsylvania State University.

**Craig J. W. Gunsul**, formerly at the University of Delaware, has joined the physics department at Whitman College as assistant professor. **James G. Pengra**, of Whitman, is currently visiting at the Nuclear Research Center, Georgia Institute of Technology.

**Joseph W. Weinberg** has been named Kenan Professor of Physics at Syracuse

University. The Kenan professorships, named for William R. Kenan Jr, were established at five New York universities to improve the quality of undergraduate teaching. Weinberg, a theoretical physicist, was at Case-Western Reserve University before he came to Syracuse.

**Dame Kathleen Lonsdale**, past president of the International Union of Crystallographers, is visiting professor at the Ohio State University department of mineralogy this fall. Lonsdale is professor of chemistry at the University of London.

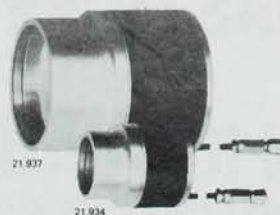
### DeShalit of Weizmann Institute Dies at 42

It is our sad task to report that Amos deShalit died of acute pancreatitis on 2 Sept. at the age of 42. His untimely passing is a great loss to his family, to the world of physics, to his institute and country and to the entire world. A brilliant physicist, deShalit was one of the very few who are at home with both experiment and theory. He was a brilliant administrator; while he was head (1954-66), the nuclear physics department at the Weizmann Institute, Rehovoth (Israel) developed into a leading center for the study of nuclear and particle physics, rivaled in its impact by only a handful of other institutions. He was a brilliant educator; since 1963 he had been actively involved in improving science education in Israel, particularly in the



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secondary schools. This activity was recently made formal by the creation at Weizmann of a department of science teaching that was headed by deShalit. Deeply committed to his country, he was much concerned with the problems of the Arab population.

DeShalit's involvement in world affairs motivated a great many of his activities; he did much to bring together physicists of all countries and political persuasions and helped to organize many international conferences. In July he gave the summary talk at the Heidelberg conference on heavy-ion induced nuclear reactions, and he had been scheduled to participate in a round-table discussion of the future of nuclear physics at an international conference that took place late in August.

He was one of the principal architects of the biennial international conferences on nuclear and high-energy physics; the proceedings of the most recent of these conferences, held at Columbia University, is to be dedicated to his memory. Involved for many years with the problems of developing countries, deShalit was a member of a United Nations advisory committee. At the time of his death he was host to a conference at Rehovoth on "Science and Education in Developing States" and had been scheduled to address it.

This partial list of his activities is a pale reflection of his personal impact; his greatest and unique contribution came from his direct and indirect influence upon friends and colleagues all over the world. Physics became more interesting and exciting to everyone who came in touch with him. His presence made a discussion more fruitful, a seminar more instructive, an experiment more significant. He raised questions and challenged ideas. He brought life and excitement to physics; this was not only because of his great insight, which enabled him to point to the essential ideas and relations, but also because of his readiness to listen and to follow the work of others, his openness to questions, his interest in any thought or idea, his enthusiasm for every new insight and his ability to recognize the significance of an idea.

So many friends had his help in developing their own ideas, help that he gave freely and unsparingly. When he visited laboratories, he left behind the seeds of many successful theories and

experiments. His remarks and suggestions spawned many papers.

DeShalit's own publications are in nuclear physics, although his master's thesis, done in 1949 under Giulio Racah's direction, was on the self-energy problem. An experimental thesis in 1951 with Paul Scherrer at Zurich began a series of experimental and theoretical papers in which nuclear structure was probed through electromagnetic and weak interactions. DeShalit's fundamental contributions to the understanding and exploitation of the shell model culminated in 1962 with a seminal book that he wrote with Igal Talmi, titled "Nuclear Shell Theory."

He had been interested more recently in the application of tools developed in elementary-particle physics to studies of the nucleus. And, vice-versa, he was using methods developed for the shell model to extract from the electromagnetic properties of elementary particles some of their underlying structure. At the time of his death he had just completed the first volume of a two-volume book on nuclear theory that he was writing with one of us. He was scientific director of the Weizmann Institute from 1962 to 1966 and director-general from 1966 to 1968.

He was a member of IUPAP, one of the correspondents of *Comments on Nuclear and Particle Physics*, and on the editorial board of *Nuclear Physics*, *Annals of Physics*, *Nuclear Data* and *Nuclear Instruments and Methods*.

A member of the Israel National Academy of Sciences and Humanities, he received in 1964 the Israel Prize for the Exact Sciences and in 1969 was elected a foreign member of the American Academy of Arts and Sciences. In recent years he was a visiting professor at Stanford University and the Massachusetts Institute of Technology.

Amos deShalit is no longer among us. We will miss his imaginative insights and bold ideas. We will miss the contagious pleasure he had in physics. We will miss his warm personality, his directness, his ability to create bonds and bridges across political chasms. Men like him are sorely needed, and they are always in short supply; our world will be colder without him.

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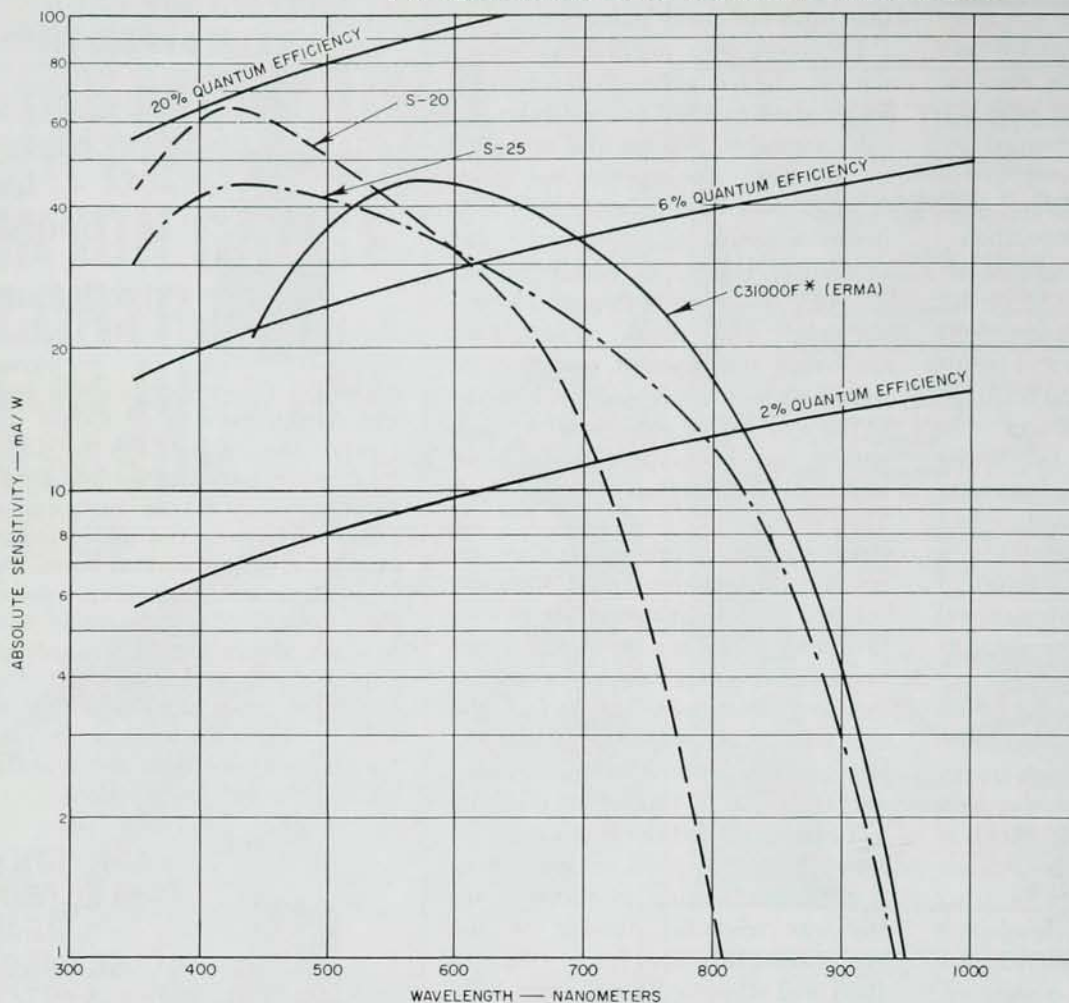
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# RCA



# CALENDAR

This is a partial calendar comprising only notices received since last month. A complete calendar is published every third month. Readers are referred to the last one, published in October, if they wish a comprehensive listing of notices. The January issue will contain the next complete calendar.

Information in the calendar is compiled from a file maintained in the PHYSICS TODAY office. Readers are invited to write or telephone for general calendar information beyond what we print. For complete information concerning an entry, readers are advised to consult the contact and the original PHYSICS TODAY reference.

## Abbreviations:

AAPT—American Association of Physics Teachers  
AAS—American Astronomical Society  
ACA—American Crystallographic Assoc.  
APS—American Physical Society  
ASA—Acoustical Society of America  
OSA—Optical Society of America  
S OF R—Society of Rheology

AEC—US Atomic Energy Commission  
AFCL—Air Force Cambridge Research Laboratories

ANS—American Nuclear Society  
AVS—American Vacuum Society  
IAEA—International Atomic Energy Agency  
IEEE—Institute of Electrical and Electronics Engineers  
IPPS—The Institute of Physics and The Physical Society  
IUPAP—International Union of Pure and Applied Physics  
NBS—National Bureau of Standards  
ORNL—Oak Ridge National Laboratory

## Coding:

date subject ☐ HOST ☐ Location (Contact) [submission deadline] *Physics Today* ref.  
• new listing ♦ new information

## DECEMBER 1969

- 10-12 ♦ **Holography and the Computer** ☐ IBM ☐ Houston (J. A. Jordan) 7/69  
18-20 • **Pulsars and High-Energy Activity in Supernovae Remnants** ☐ ACCADEMIA INTERNAZIONALE DEI LINGUI ☐ Rome (B. Bertotti, Laboratorio di Astrofisica, C. P. 67, Frascati (Rome), Italy) 12/69  
19 • ☐ N. Y. ACAD. SCI. ☐ 2 E. 63 St., N. Y. (J. Lebowitz, Belfer School, N. Y., N. Y. 10033) 12/69

## JANUARY 1970

- 19-23 • **Electrochemistry** ☐ GORDON RESEARCH CONFERENCES ☐ Santa Barbara, Calif. (Alexander M. Cruickshank, Pastore Chemical Lab., U. of Rhode Island, Kingston, R. I., 02881) 12/69  
26-30 • **Polymers** ☐ GORDON RESEARCH CONFERENCE ☐ Santa Barbara, Calif. (Alexander M. Cruickshank) 12/69

## MARCH 1970

- 11-13 • **Scintillation and Semiconductor Counters** ☐ IEEE, AEC, NBS ☐ Wash., D. C. (R. L. Chase, Brookhaven Nat'l Labs., Upton, N. Y. 11973) 12/69  
23-27 • **Progress in Sodium-Cooled Fast-Reactor Engineering** ☐ IAEA, AEC ☐ Monaco (John H. Kane, Div. of Tech. Info., AEC, Wash., D. C. 20545) 12/69

Topics: Primary components, steam generators, safety technology, hydraulic and structural-core technology.

## APRIL 1970

- 3, 4 • **Midwest Theory Conference** ☐ UNIV. OF NOTRE DAME ☐ Notre Dame, Indiana (W. D. McGlinn, Dept. of Physics, U. of Notre Dame, Notre Dame, Ind. 46556) 12/69  
6-8 • **Resonance in Conducting Materials** ☐ UNIV. OF WARWICK ☐ Univ. of Warwick, Coventry,

Partial calendar—see note at opening.

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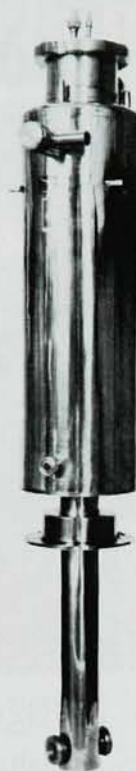
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APRIL 1970

Warwickshire, UK (R. Dupree,  
Univ. of Warwick) 12/69

- 27-29 • **Frequency Control** □ ELEC-  
TRONIC COMPONENTS LAB., US  
ARMY ELECTRONICS COMMAND □  
Fort Monmouth, N. J. (J. M.  
Stanley, *Electronic Components  
Lab., Fort Monmouth, N. J.*  
07703) 12/69

MAY 1970

- 1, 2 • **Experimental Meson Spectroscopy** □ UNIV. OF PA. □ Phila.,  
Pa. (Jules Halpern, *Physics, U. of  
Pa., Phila., Pa.* 19104) 12/69

- 4, 5 • **Transducers** □ IEEE □ Gaithers-  
burg, Md. (H. P. Kalmus, *Harry  
Diamond Labs., Dept. of the  
Army, Wash., D. C.*) 12/69

- 18-22 • **Materials Symposium** □ US AIR-  
FORCE, AMERICAN INST. OF AERO-  
NAUTICS AND ASTRONAUTICS,  
AMERICAN ORDINANCE ASSOC., SO-  
CIETY OF AEROSPACE MATERIAL  
AND PROCESS ENGINEERS □ Miami  
Beach, Fla. (*Air Force Sympo-  
sium '70, P.O. Box 38, Dayton,  
Ohio 45420*) 12/69

JUNE 1970

- 28-2 ♦ □ HEALTH PHYSICS SOCIETY □  
Chicago (R. F. Cowing) 7/69

JULY 1970

- 20-24 • **Dielectric Materials, Measure-  
ments and Applications** □ IEEE,  
INSTITUTE OF ELECTRICAL ENGI-  
NEERS (UK) □ Univ. of Lancas-  
ter, UK (*IEE, Savoy Place, Lon-  
don W.C. 2, UK*) 12/69

- 21-24 • **Nuclear and Space Radiation  
Effects** □ IEEE □ San Diego,  
Calif. (R. Thatcher, *Battelle Mem.  
Inst., 505 King Ave., Columbus,  
Ohio*) 12/69

AUGUST 1970

- 11-15 • **Magnetic Recording** □ HUNGAR-  
IAN OPTICAL, ACOUSTICAL AND  
CINEMATOGRAPHIC SOCIETY □  
Budapest (M.J.K., *Optical, Acous-  
tical and Cinematographic Soci-  
ety, Budapest 5, Szabadság tér  
17, Hungary*) [2/70] 12/69

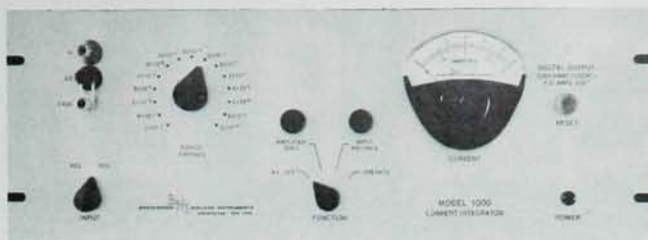
- 26-29 • **Small-Angle X-ray Scattering** □  
ACA □ Graz, Austria (O. Kratky,  
*Inst. for Physical Chemistry,  
Univ. of Graz, Heinrichstrasse 28,  
A 8010, Graz, Austria*) 12/69

SEPTEMBER 1970

- 15-18 • **Gas Discharges** □ IPFS, INSTI-  
TUTE OF ELECTRICAL ENGINEERS  
(UK) □ London (*IEE, Savoy  
Place, London W.C. 2, UK*) 12/69

Partial calendar—see note at opening.

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28-30 • Electron Devices □ IEEE □ Wash., D. C. (IEEE, 345 E. 47th St., N. Y., N. Y. 10017) 12/69

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15-21 FEBRUARY

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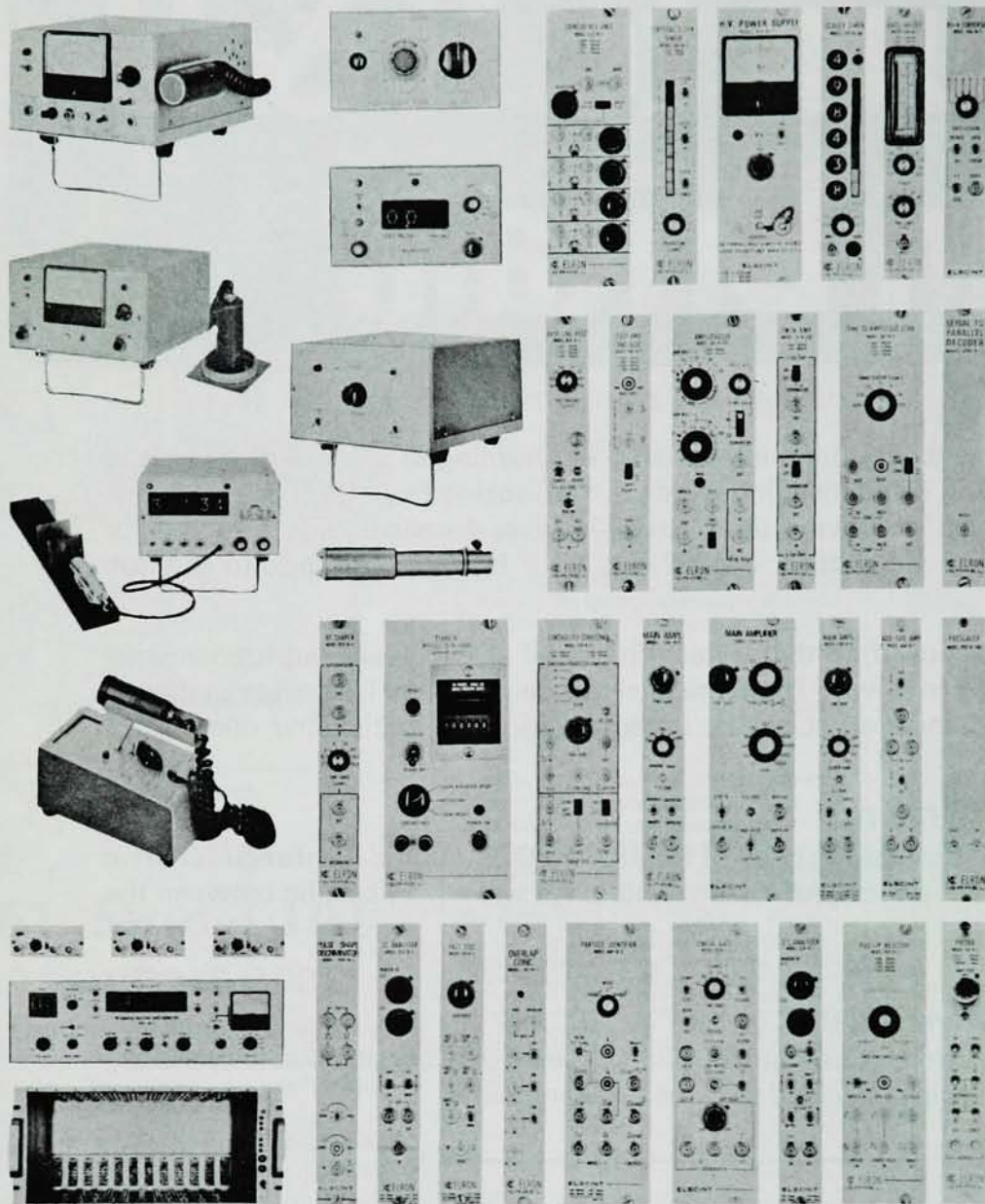
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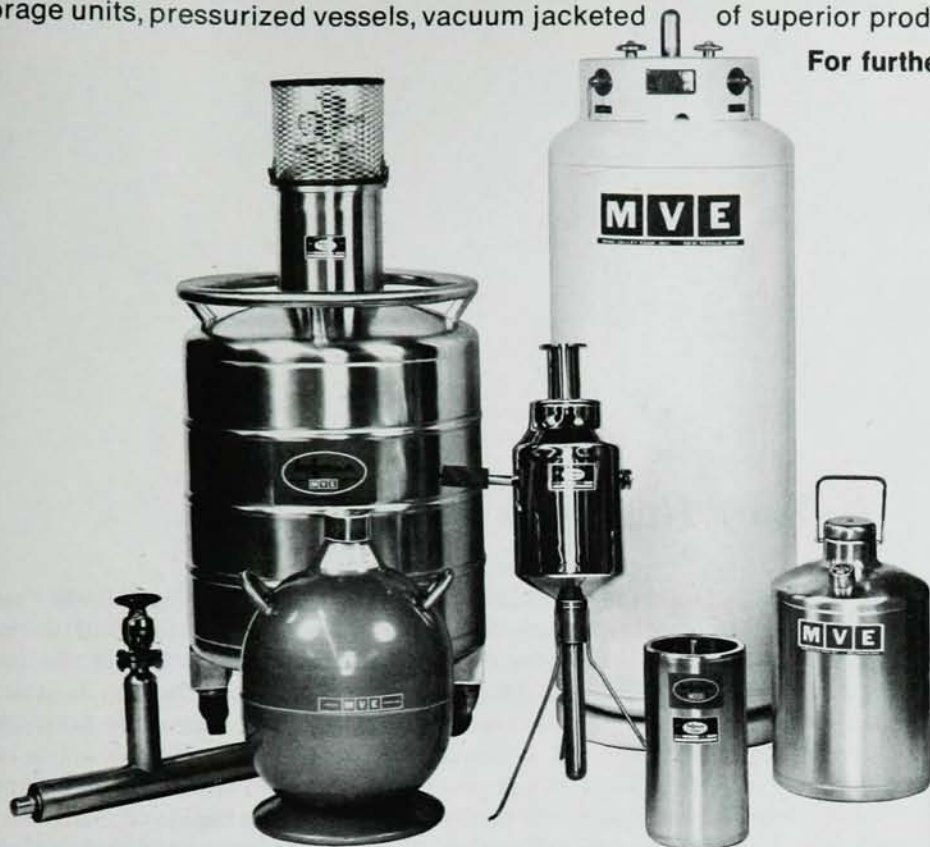
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## GUEST EDITORIAL

### *Is Your Research Moral?*



The author of this invited editorial is chairman of physics at Stanford. He has three degrees from Toronto, and before going to Stanford he worked at Research Enterprises, Columbia University and Bell Telephone Laboratories. His research has been in rf, microwave and optical spectroscopy, solid-state physics and quantum electronics.

Nowadays science and scientists are being attacked from all directions. On the one side there are those in Congress, the military and the general public who say that if science were doing what is expected, it would have won the war in Vietnam and produced horrible new weapons to terrify all possible enemies. On the other side many students and intellectuals, even including some scientists, say that if scientists were doing their job properly, they should have produced an end to poverty, racism, air pollution, overpopulation and war. Strangely, both opposed groups of critics argue that since science has not performed according to their specifications, either scientists are misdirecting their efforts or perhaps science is irrelevant and has little to contribute to the solution of important human problems. Both groups equally fail to understand the real nature of scientific discovery and the ways in which scientific knowledge eventually makes possible the goals people desire.

A fuller understanding of nature is esthetically pleasing and deeply satisfying, but its social significance comes because it is also useful. It makes us prophets so that we and our successors can predict what can happen and can

even tell something of the consequences that will follow if we make something occur. Part of the attraction of physics is that simple laws and concepts have extremely far-reaching consequences and apply in a very wide range of situations. For just this reason the ultimate applications of physical discoveries are almost never apparent at the beginning. We all know something of the long history extending from the nuclear atom and the mass-energy equivalence to atomic power. We who know such history should recall it and tell it to those who question the nature and utility of science.

Let me give two examples from relatively recent technological history. The first I know personally, for when Charles Townes and I were trying in 1957 to see whether the maser principle could yield a generator of coherent radiation in or near the range of visible wavelengths, we gave almost no thought to applications. I had never heard of a detached retina, and yet one of the earliest applications of lasers was for eye surgery to prevent retinal detachment. Although lasers are still quite primitive and many of the more obvious applications remain



impractical, they have been applied to a wide range of needs, most of which could hardly have been foreseen except by a person who specialized in the particular area of application. But if we had tried to attack these needs head on, as might have been done by a specialist in eye surgery, we would never have been thinking about stimulated emission from atomic systems.

Considerably more important consequences have come from Felix Bloch's discovery of the concept of energy bands in solids and their influence on conduction of electricity. In the 20 years after Bloch's 1928 thesis, the band ideas guided the whole development of solid-state physics. And yet, as late as 1953, 25 years after the discovery, one could have said truthfully that these ideas had not led to greatly improved metals nor to any other important practical consequences. But a year or so later, there began serious applications of the transistor, a device that really could not have been invented without the conceptual framework of the band theory. Now, the impact of the use of transistors and other semiconductor devices on human life is already enormous. To take a few examples, there are cardiac pacemakers

and the ubiquitous transistor radio, which is playing such an important role in unifying some developing countries. Without semiconductor devices the entire space program would be nearly impossible. It is hard to conceive of either the human aspects of space flight (such as environmental and weather-observation satellites), the scientific aspects (such as astrophysical observatories and moon landing probes) or the military aspects without large-scale and lightweight semiconductor computers. In industry, it seems quite possible that semiconductor logic will eliminate a large part of the routine drudgery that seemed for a while to be an inescapable consequence of mass production. None of this could possibly have been foreseen at the time of the original scientific discovery. Yet from all our experience we should have faith that scientific ideas do have consequences, important consequences that greatly increase the range of decisions that man can make. It is the nature of man to make choices and to master his environment. With science and its consequences we have the tools to make decisions, good or bad. If we sacrifice scientific research for immediate social gains, we might have a

short-range benefit, but we are surely mortgaging our future.

After applications of science become apparent, the people and their representatives must decide whether the applications are good or bad. Here scientists must play a part by sharing knowledge of the possible courses and their likely consequences. If the facts are known, we can be optimistic that the people will more often choose courses to their own benefit than the reverse. Every thinking scientist must have faced this question and concluded that, broadly, scientific discoveries do eventually open up badly needed alternatives from which more good than evil will be extracted.

Whatever the grounds for such faith, whether from a religious conviction or from a knowledge of scientific and technological history, we must put these concerns aside when we confront the mysteries of the universe. In the light of this belief that good things do eventually come from new knowledge, I am convinced that good scientific research is a highly moral activity. The only kind that is not moral is that which can be characterized by a phrase of Wolfgang Pauli's: "It isn't even wrong."

—Arthur L. Schawlow



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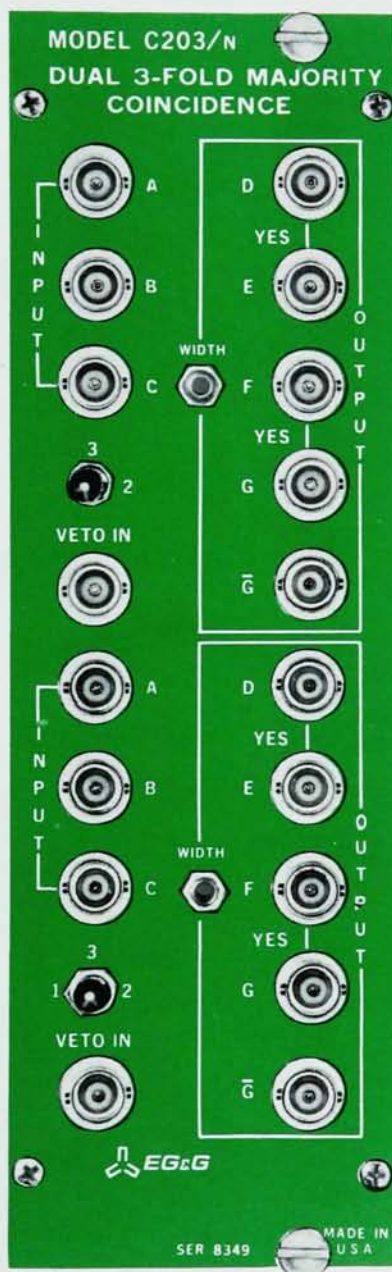
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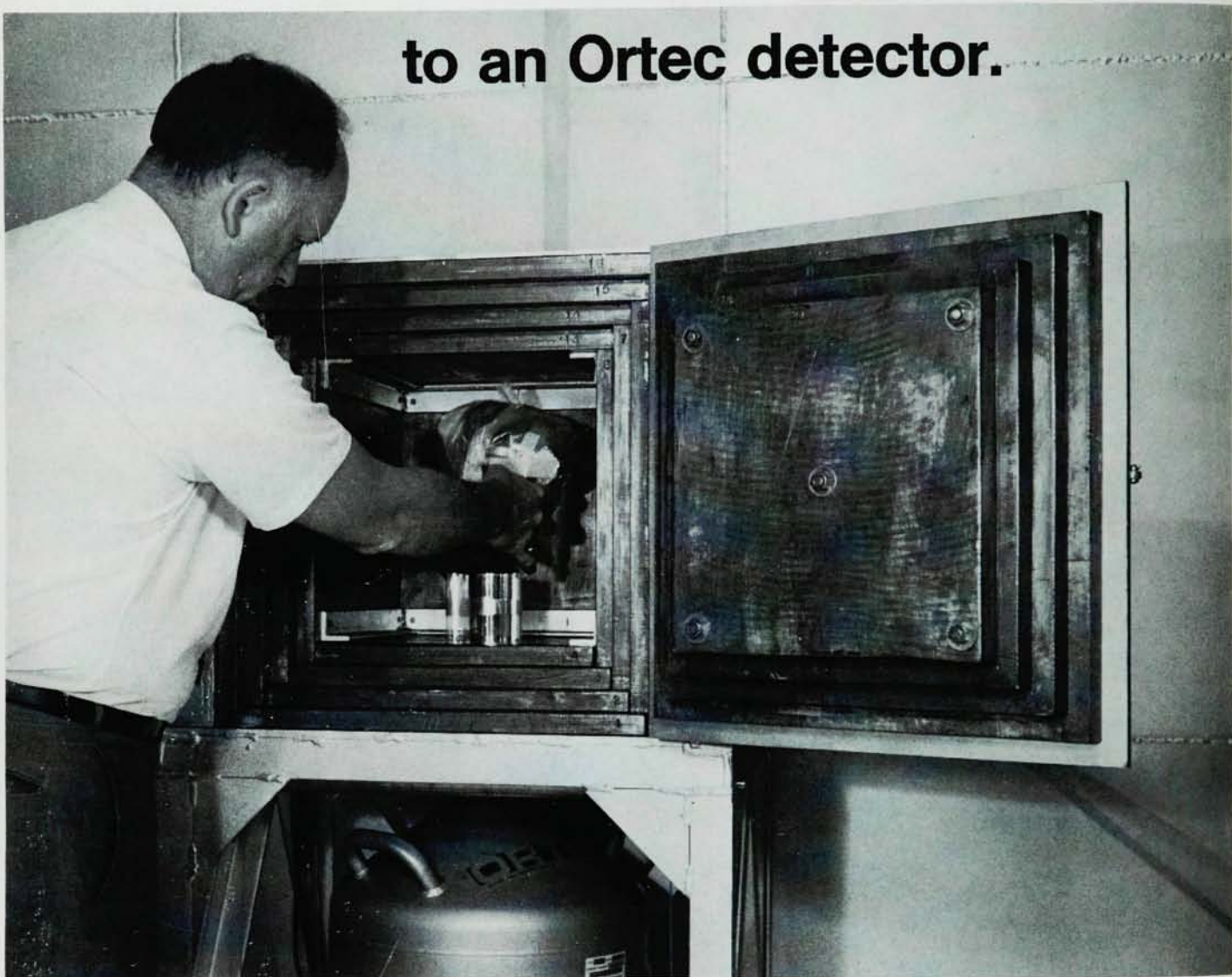
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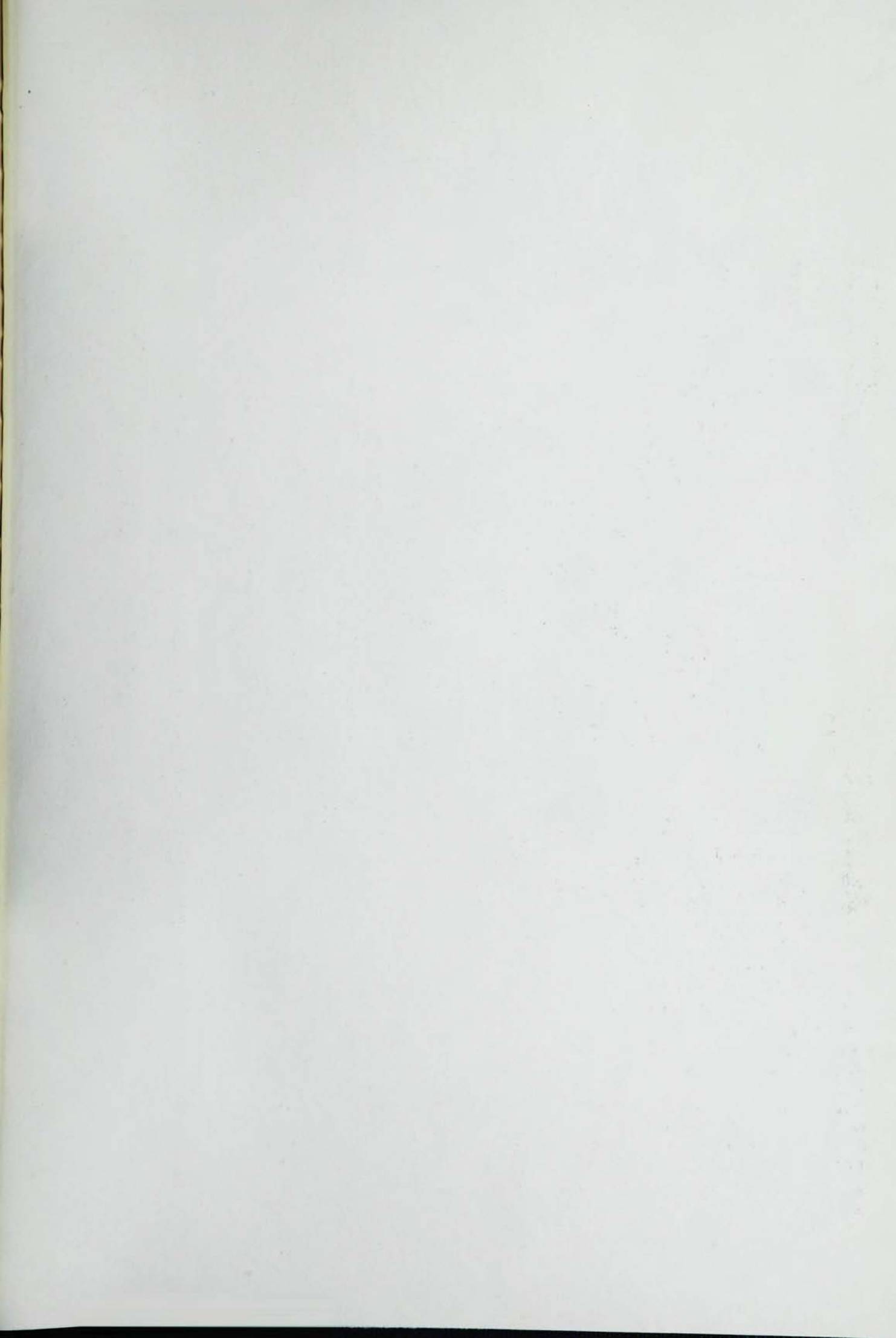
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probability distribution

$$W_{fg} = |C_{fg}|^2$$

Suppose that the eigenfunction  $\phi_{fg}$  is written in terms of certain real functions  $R$  and  $S$  as

$$\phi_{fg}(x) = R_{fg}(x) \exp \{ -iS_{fg}(x) \}$$

where both  $R$  and  $S$  depend on the indices  $f$  and  $g$ . The first step in the measurement then involves converting the wave function  $\psi(x, t_M)$  of the system into a new wave function  $\psi_s(x)$  given by

$$\psi_s(x) = \psi(x, t_M) \exp \{ iS_{fg}(x) \} \quad (7)$$

by applying a pulse potential

$$U_2(x, t) = -\hbar S_{fg}(x) \delta(t - t_M)$$

to the system. Then the probability amplitude  $C_{fg}$  of equation 6 has the value

$$C_{fg} = \int R_{fg}(x) \psi_s(x) dx$$

which is just the overlap integral between the new wave function  $\psi_s(x)$  of the system and the real normalized wave function  $R_{fg}$ . From the earlier discussion of state preparation we know how to find a potential  $U_{fg}(x)$  in which  $R_{fg}(x)$  is an eigenfunction of energy  $E$ . This potential is given by

$$U_{fg}(x) = E - [R_{fg}(x)]^{-1} T R_{fg}(x)$$

Hence, the second stage of the measurement process involves the sudden application of the potential  $U_{fg}(x)$  and removal of the potential  $V(x)$  from the system with a wave function given by equation 7. The next task is to find the probability that the particle is in the state of energy  $E$ . This could be accomplished by the usual kind of Stern-Gerlach procedure. In this manner, we find the desired probability that the operators  $F, G$  have values  $F_f, G_g$  for the state  $\psi(x, t_M)$  of the system of interest. It will be noted that the measurement problem is now more complicated than for Hamiltonian operators, as the potentials  $U(x, t)$  to be applied depend on the values of  $f$  and  $g$ , and hence a series of measurements has to be made for each set of  $f, g$  values.

### Limitations

Some concluding comments are in order.

• We have assumed that all classically describable potentials  $U(x, t)$  are available to us experimentally. This is quite similar and closely related to an assumption made by Niels Bohr and

Leon Rosenfeld<sup>13</sup> in their discussion of the measurement of electromagnetic fields, that test bodies of very great mass and charge density were available to them, whose quantum-mechanical fluctuations of position and momentum could be neglected.

• Our discussion of measurement considers the observation of any one dynamical variable,  $A$ . Instead, we could measure another one,  $B$ , for the same state  $\psi(x, t_M)$ . For each operator we could determine the dispersion measure  $\Delta A$  or  $\Delta B$ . The product of the fluctuation measures would obey the Robertson<sup>14</sup>-Schrödinger<sup>15</sup> generalization

$$\Delta A \cdot \Delta B \geq 1/2 | \langle [A, B] \rangle |$$

of Heisenberg's uncertainty relations. It should be noted, however, that such uncertainty relations do not refer to measurements, whether simultaneous or not, of a pair of observables. In special cases, involving commutability, it might be possible to measure first one observable and then another, but in general the thoroughgoing measurement of the first observable will so disrupt phase relations that it will serve no physical purpose to subsequently measure a second observable on the resulting mixture.

To measure simultaneously two non-commuting observables  $A$  and  $B$  (for example,  $x$  and  $p$ ) one would have to find a potential  $U(x, t)$  that was determined by both  $A$  and  $B$ . In general, I do not believe that this can be done in such a way that the desired information emerges from the measurement. One could, of course, form a single Hermitian operator out of the two Hermitian operators  $A$  and  $B$ . Some examples are  $(AB + BA)$ ,  $-i(AB - BA)$ ,  $A^2B + BA^2$ ,  $ABA$ , etc. Any one of these Hermitian operators could be measured, as already indicated, but this would not be the desired simultaneous measurement of  $A$  and  $B$ .

• It is possible to extend the methods outlined so that measurements on many-body systems can be made.

• I do not see how to apply procedures of the kind outlined above to the relativistic quantum domain, or to field theory. In the absence of such generalizations, it may well be doubted that the story that I have given provides any significant insight into the real meaning of quantum mechanics. However, it is true that almost all expositions of quantum mechanics make use of the fictional notion that some kinds of measurements are possible. I have

described certain experimental procedures for making them. There may be other ways. If they cannot be made in some fashion, either as I have suggested, or otherwise, then it appears that our understanding of the meaning of the quantum theory is correspondingly diminished, and it is only likely to be increased when a better theory of measurement for the more general relativistic and field-theoretic cases can be given. Of course, it may be that a system of rules for calculation can exist despite the absence of an operational interpretation of the kind I have attempted. For the teaching of quantum mechanics now, it is certainly a convenient fiction to pretend that the usual textbook assumptions about measurement have a meaning, even if from an operational point of view they do not. The mathematical formulation of quantum mechanics by Dirac beautifully matches the assumed notion of measurability. However, there is clearly much more for us to learn.

\* \* \*

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