

LETTERS

Choosing a Future for Physics Proves to Be Highly Debatable

In their excellent, provocative article, "What Future Will We Choose for Physics?" (December 1995, page 25), Sol Gruner and his colleagues prescribe as the principal remedy for the present ills of the physics profession the diversification of physics research into areas outside the currently defined standard subfields. Their prescription no doubt has merit. Nevertheless, some things in the article deserve comment.

By not applying the same standards of care to their work on social science and policy issues that they apply to physics research, the authors run the risk of having this work taken less seriously than it deserves to be. In their figure 3, for instance, they join data on physics employment from two sources in a way that makes it appear that, starting in about 1970, total employment in physics dropped by some 15% in one or two years—a change that makes no sense. They go on to call this "a wrenching discontinuity." In this same figure, they show no data after 1979, although such data are available. Further, they say: "There has been little change in the distribution among physics subfields since the early 1970s because there has been little turnover in personnel." This assignment of cause and effect is surmise not backed up by objective data.

The authors say nothing of a need for possible control of PhD production rates in physics and nothing of ways to add to the demand for physicists in areas outside pure research. Instead, they focus on a call for physics to gain more market share (my term, not theirs) in science research. Although one can applaud their advocacy of diversification and their excellent rationale in a section called "What is physics?" it is hard to endorse the position that expansion of opportunities through new kinds of re-

search is sufficient as a remedy for too many physicists, too few jobs and too few dollars. At least two additional remedies deserve consideration: (1) control, difficult though it may be, of PhD production and (2) expansion of faculty opportunities through more vigorous efforts to attract undergraduates into the classroom.

I may be misreading the article (and one of the authors has already assured me that I am), but I see in it the subliminal message that the corporate watchword "Grow or Die" applies to physics. If one believes, as I do, that humankind has a reasonable future only if it can achieve a steady state, one should be able to imagine a vigorous, healthy future for physics without growth. After all, there are now far more people and resources in physics than in past eras, when no one would doubt the field's vigor. The future of the species may depend on our ability to outgrow the notion that viability and vitality depend on growth. When we can discard the Grow or Die fantasy, we can think rationally about steady state for a little piece of the whole, such as the field of physics.

KENNETH FORD

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It is refreshing to hear academicians recommending that graduate students not be forced into being narrowly specialized and that departments encourage more interdisciplinary research to which physicists can contribute.

It used to be that doctoral candidates had to show proficiency in a foreign language before they could qualify for the PhD. I'd like to suggest that an analogous requirement be instituted for the purpose of broadening the technical education of physics graduate students. PhD-granting physics departments should make the passing of one-to-two years of course work in nonphysics science or engineering subjects a requirement for obtaining a physics PhD. Some significant fraction of that course work should have lab work associated with it so that graduate students cannot satisfy the requirement by taking only lecture courses.

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Undergraduate physics majors, too, should be required to broaden their nonphysics technical background through course work or research projects. Biology, chemistry and engineering are particularly valuable subjects for such broadening.

Finally, industrial physicists need to be invited to visit academic departments, on a regular basis, to give firsthand accounts and advice concerning professional work that doesn't fit into neat physics categories.

JEFFREY MARQUE
Palo Alto, California

Gruner and company raise serious issues and express a concern for our common discipline that is clear enough. However, as a physics faculty member with a fairly recent PhD (1984) whose work has been in superconducting and magnetic materials, I argue that they are wrong in their characterization of our discipline, some of the solutions they suggest and certain of their proposals for educating physics majors.

With regard to the discipline, the authors describe physics faculties as having been frozen into place, along with their areas of specialization, starting around 1970. Within condensed matter physics, this description is incorrect. Consider that (1) cuprate superconductors, now the single largest component of the American Physical Society's division of condensed matter physics (DCMP), did not exist until 1987; (2) polymers have grown to the point of constituting a separate APS division; (3) "soft matter" (for example, shaving cream) is a respectable and intensively studied class of materials that was virtually absent in physics departments ten years ago; (4) magnetic multilayers—barely studied until ten years ago in the US—are now widely studied. Further, here at the University of Wisconsin at least, we have interdisciplinary efforts—including physics faculty—studying cuprate superconductors, chemical vapor deposition growth of materials, magnetic oxides and thin films, and DNA computing. I suggest that the authors do not know their own APS well enough.

On the subject of solutions, I agree with the authors' recommendations that physicists should consider interdisciplinary work. However, I dispute their assertion that "novel multicomponent materials" are among the research areas "being shunned by physics departments"; on the contrary, they constitute a very active area within DCMP. I also dispute their notion that "physics has much to offer society" in such subject areas as

"transportation, . . . consumer goods, . . . entertainment, human services and finance." I believe that the authors lack the expertise in these areas that would enable them to offer sensible suggestions.

As for the authors' proposals on educating physics majors, although I accept their "set of conceptual tools" common to physicists, I object to their assertion that curricula should not be "so crammed with physics courses" as to exclude "concomitant study in non-physics areas." First, at least at Wisconsin, the problem is rather how to ensure that students know enough physics and mathematics—especially mathematics. Second, the authors' conceptual tools are best developed in the context of solving problems that can be analyzed in detail. Third, I object to the authors' remarks that, owing in part to "simple arrogance," physicists are less willing than other scientists to participate in interdisciplinary research or learn the basics of other disciplines. The authors should speak for themselves. In the areas in which I have expertise, the synthesis of new materials and their chemistry and microstructure, as well as their physics, have been achieved largely by physicists.

One final point: The authors specifically question why astrophysics has "not been expanded within physics departments." But it has been expanded, and data are available to confirm this statement. Between 1973 and 1989, according to the authors' own table, the total number of faculty increased by 24.6% and the number of faculty in astrophysics grew by 70.7%. Further, a check of the employment advertisements in the December 1995 issue of *PHYSICS TODAY* reveals that ads for astrophysics positions accounted for 22% of the academic job ads, whereas astrophysics accounted for only 6% of all physics faculty in 1989 (source: the authors' table). Astrophysics is clearly a growing specialty relative to the other specialties. It would have helped if the authors had studied their own data.

MARSHALL ONEILLION
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GRUNER, LANGER, NELSON AND VOGEL REPLY: Ken Ford chides us, with some justification, for figure 3 of our article. Perhaps we should have chosen a clearer figure. We thought there was virtue in showing historical material: Figure 3 was reproduced, unmodified, as it appeared in the report^{1,2} of the panel established in 1976 by the APS Council to analyze what the panel termed the "turmoil and transition for the support of phys-

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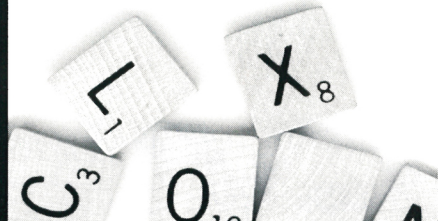
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ics and physicists" (their words) of the early 1970s. Our intention was to draw attention to this important report, and to advocate that the physics community again initiate organized discussion on the future of the profession.

We hold to our central premise, as seen in figures 1-3: 1970 marked a transition in physics. The faculty members who entered academe in the 1960s differed from the entrants of the preceding half century in a subtle and profound way—namely, they were not to be quickly followed by an even larger cohort of new faculty. It is certainly debatable whether this difference has changed the historical evolution of the profession; our purpose was to initiate the debate.

Ford also emphasizes the important issues of birth control for PhDs in physics and growth of the profession as a whole. We hold no monolithic or adamant opinion on these matters. Rather, we felt we had to say something to initiate debate because we are very concerned, even upset, about the low level of discussion within the community, especially among established physicists.

We wholeheartedly agree with Marque's comments that steps should be taken to broaden the training of physics PhDs. This is also a central recommendation of the recent National Academy of Sciences study on the state of science and engineering graduate education, which declares, in the preface, that "The recommendations in this report reflect a common theme. Many of the job opportunities of the future will favor students with greater breadth of academic and career skills, so the universities and their partners in the graduate-education enterprise should therefore cooperate to broaden curricular options for graduate students."³ In other words, change is very much needed.

By contrast, Onellion seems to feel that physics has changed adequately over recent years. Although we noted that many beautiful developments have occurred in physics over the last few decades, we wonder how physics will look relative to other disciplines when future generations of scientists look back upon, say, the 1965-95 period. Geologists will point to the revolution of plate tectonics that completely changed our understanding of Earth, biologists will note the invention of molecular engineering and how it catalyzed an explosion in knowledge of living organisms and the information scientists will point out how computers and neural nets changed the daily lives of everyone. Will physicists be able to make a comparable statement? We worry they

may not. Of course, one could argue that 20th-century physics has already undergone the relativity and quantum revolutions and that there are only so many fundamentally new things in any given profession. On the other hand, because we don't know what fundamental physics remains to be discovered, it is important never to be satisfied that the current focus of the profession is adequate.

It is also worth noting that many—we dare say most—of the physicists working in some of the new areas cited by Onellion are amongst those most concerned about their position in the academic physics community. The division of high-polymer physics is now struggling to retain its divisional status within APS. The division's membership directory shows that most of the faculty listed are not in physics departments, because very few physics departments have hired polymer physicists. Soft condensed matter physicists are encountering the same problem. The APS division of biological physics is also in danger of losing divisional status. The DNA computing cited by Onellion is very exciting but is being mostly carried on outside of academic physics departments. Although physicists working in interdisciplinary areas of materials science have made important contributions, there are few positions open to them in physics departments.

We agree with Onellion that students in other disciplines would benefit from learning more physics and mathematics, but we do not agree that physicists in training encounter a sufficiently broad professional experience. By and large, physics faculty members are still training their students for the academic physics career path that characterized their own careers, even though most new physics PhDs will wind up on other paths. We are not alone in this conviction; nor is the problem unique to physics departments. The need for broader training is the central theme of the National Academy of Sciences study.³

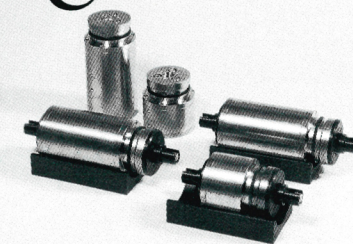
A case can be made that astrophysics is now doing relatively well in physics departments, and we suspect that the data would look even more impressive if the years since 1989 were taken into account. We thank Onellion for pointing this out.

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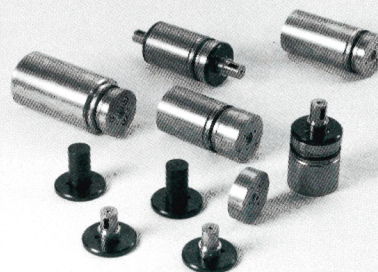
1. L. Grodzins, *The Transition in Physics Doctoral Employment, 1960-1990*, American Physical Society, New York (1979).
2. M. D. Fiske, executive summary in ref. 1.
3. National Academy of Sciences Committee on Science, Engineering and Public

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LETTERS (continued from page 15)

Policy, *Reshaping the Graduate Education of Scientists and Engineers*, National Academy Press, Washington, DC (1995).

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Apparatus Upended: A Short History of the Fountain A-Clock

In the mid-1980s, Norman Ramsey¹ and Paul Forman² wrote technical and detailed accounts of atomic clocks. However, we have found little published in the scientific literature about the early days of the atomic clock used in what is now called the fountain experiment, which was designed to measure the gravitational redshift predicted by Albert Einstein. It seems timely to recount that history, given the Clinton Administration's late-March decision to offer civilians access to much more precise location data from the Global Positioning System, and the key role played in the GPS by atomic clocks.

The atomic clock has its origin in the molecular beam magnetic resonance (MBMR) method and, later, the atomic beam magnetic resonance (ABMR) method conceived in the late 1930s by I. I. Rabi and developed by him with Sidney Millman, Polykarp Kusch and Jerrold Zacharias.³ The group, which then included Jerome Kellogg and Ramsey, made many important experiments, exemplified by the discovery of the electric quadrupole moment of the deuteron.⁴ The high precision of the new resonance technique led the researchers to envisage a laboratory measurement of the Einstein-predicted gravitational redshift, $\delta\nu/\nu = -(Gm_e/c^2r_e^2)z \approx 10^{-16}z$, where G is the universal gravitational constant; m_e and r_e are, respectively, the mass and radius of Earth; z is the difference in altitude (in meters) of two clocks; and $\delta\nu/\nu$ is the fractional change in the frequency of the rf transition in the ABMR. One clock could be at sea level, the other on a mountain, such as at the research station located in the Jungfraujoch, a high pass on the flanks of the Jungfrau in the Swiss Alps.

World War II interrupted the planned experiment. In 1945, Rabi came back to the topic in his Richtmyer Lecture presented at an American Physical Society meeting, as reported in the *New York Times* on 21 January 1945. The precision of the experiment, however, was still far from what was needed, being limited by the transit time, Δt , of the atoms through the rf interaction region in a uniform magnetic field, $\Delta\nu\Delta t \sim 1/2\pi$. Magnet length and field homogeneity realities imposed severe limits on Δt . An essential advance occurred in 1949, when Ramsey published his "separated oscillating field" method.⁵ Consequently, the stringent requirement on the magnet homogeneity was lifted and, with the resulting gain in precision, atomic standards could be envisaged to replace timepieces based on astronomical measurements.

By the early 1950s, work was under way at MIT and elsewhere on the development of atomic clocks consisting of ABMR apparatus in which the Ramsey method was used. They were based on the cesium ground-state hyperfine structure separation, about 9193 MHz, known up to then to a precision of about 10^{-8} . That was still far from sufficient for a redshift experiment, even with a tenfold gain in precision obtained with the first atomic clock of Louis Essen and his collaborators⁶ in the UK at the National Physical Laboratory in Teddington. (In their article, they referred to similar, contemporaneous efforts by Harold Lyons at the US National Bureau of Standards.)

The ABMR and MBMR laboratories of Ramsey at Harvard University, Zacharias at MIT and Rabi at Columbia University, and also the nmr group led by Edward Purcell at Harvard, had close interactions. Likely as a result of this cross-fertilization, Zacharias launched a novel experiment—the fountain experiment—designed with broad brush strokes, to reach the detectability of the redshift: he set the ABMR apparatus vertically. Atoms with around 5% of the most probable beam velocity for a Maxwellian distribution, decelerated by gravity, would go through the first of the Ramsey rf loops on the way up, turn around, and fall through the second loop on the way down. The interaction time of about 1 second would give a resonance linewidth of less than 1 Hz. With good signal-to-noise ratio, the line center would be determined adequately for the redshift measurement. (At the end of 1954, when one of us, Henry Stroke, left the MIT Atomic Beam Laboratory to take a postdoctoral position at Princeton Uni-

versity, the vertical apparatus assembly was well under way.)

Only very brief descriptions of the fountain experiment appeared in publications at the time. Perhaps the first and relatively most extensive one was published in the *Christian Science Monitor* on 27 January 1955, under the title "Three A-Clocks Make Headlines." The article quoted Zacharias as expecting to measure the gravitational shift and "go to the Jungfrau in Switzerland next summer."

A year later, Ramsey provided a brief description of the experiment in his book on molecular beams,⁷ but his references were limited to a private communication from Zacharias and a talk, "Measurements with Molecular Beams," given by Zacharias at an APS meeting.

The only other contemporary notices of the experiment that we have found are in some of the quarterly progress reports of the MIT Research Laboratory of Electronics for 1956–58. The 15 January 1956 report⁸ includes a sentence that refers to "trying to observe the gravitational redshift, an effect of 2 parts in 10^{13} ." The reports for 15 July 1956 and 15 October 1957 carry accounts by Vincent J. Bates giving details of the principle and communication of the frequencies at two sites differing in altitude, as would be required in the redshift experiment.⁹

The progress report for 15 January 1958 carried a laconic note by John G. King and Zacharias on the conclusion to the fountain experiment: "One major effort of observation proved intractable and was finally abandoned."¹⁰ Scattering of the slow atoms from the beam by the faster ones was the undoing of the experiment.¹¹ This report did provide the only published data on the fountain clock; it was 10 feet in diameter and 28 feet in height.

It was not until 1989 that the fountain experiment was carried out successfully, by a Stanford University research team that obtained slow atoms by means of laser cooling.¹²

An epilogue: Einstein, of course, was interested in the redshift experiment, and early in 1955, we had a chance to discuss the project with him.¹³ It was amusing to realize back then that 30 years earlier, the Jungfraujoch had been the site of searches for an ether drift; the motivation for them, however, had been an attempt to disprove Einstein's earlier theory.¹⁴

(We thank Norman Ramsey of Harvard for sharing his recollections with us, Paul Forman of the Smithsonian Institution for providing us with notes for his history of atomic clocks² prior to publication and Rainer Weiss and Barbara Passero of MIT for aid-