

FACING PAINFUL TRUTHS OF SCIENCE IN TODAY'S MARKET

During the past several years I have read with unease letters and articles about the anticipated 1990s "physics job market." Some writers have been optimistic, noting that many physicists in academia are (like myself) reaching retirement age, and predicting a shortage of qualified replacements. My own view has been less rosy. I have sadly read personal accounts of several young scientists who thought a PhD would lead to a satisfying career—and have been unable to land a decent job.

John Rowell's article "Condensed Matter Physics in a Market Economy" (May 1992, page 40) has now provided a public service—a "reality check" for one major area of physics. It is painful to read but is honest about where things have been heading in the past two decades so far as solid-state physics (and electronics) is concerned. For other physics subfields the details would be different, but many of the forces at work would still resemble those pointed out by Rowell. I hope that any reader who bypassed the article as not relevant to his or her specialty area will dig out the May 1992 issue and read Rowell's analysis and prognosis. He provides food for thought—and maybe even some action—by the entire physics community.

To the examples Rowell gives of the kinds of roles a large industrial company can play or not play in R&D, I can add another that seems relevant to me. The late Robert Noyce worked for William Shockley in the mid-1950s, then became one of the founders of Fairchild Semiconductor toward the end of that decade. About ten years later, he was a major force in the founding of Intel Corporation, and he made a statement then that still worries me, more than 20 years later. I don't remember the exact words, and hope I am not impugning his memory by an incorrect quote. My recollection of the essence of what he said about Intel as a brand-new company was that it would be different in not spending on research, only

on development, since one could always acquire anything new that looked to be of future importance by going out and hiring the key people. This viewpoint expressed in the late 1960s seemed to me to be a new signal that "not invented here" could become a term of pride rather than of derision.

Rowell's article also addressed the need for graduate schools to exercise restraint in admissions to doctoral programs. I don't want this to sound smug, but for me it has been quite satisfactory to work with just one or two graduate students at a time. This form of "birth control" may be advisable for many more of us.

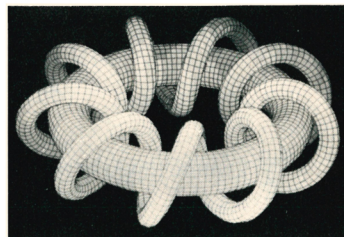
Let me not end on a note of unrelieved gloom. I do believe, along with Rowell, that the numbers of solid-state physicists in US industry will continue to decline. Ironically, the "peace dividend" from winding down cold war activities will probably make this situation worse in the short run, and persons whose positions vanish for that reason will generate more resumés for those who advertise job openings in PHYSICS TODAY. It would seem to me that the leadership of all the major scientific societies in the US, including that of the national academies, needs to work assiduously on plans for the constructive use of our national resources (personnel, equipment, national labs, universities) for the nation's benefit, based on spending some achievable combination of government and private funds. I am not referring to pork-barrel funding or other handouts, but to a cohesive plan for putting our best foot forward. We expect this of the Ministry of International Trade and Industry (MITI) in Japan, so why not here?

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5/92

John Rowell's article accurately diagnoses many of the problems facing condensed matter physics and science in general in our economy. The

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LETTERS

scientific and educational communities would be well advised to consider the points Rowell makes in any future restructuring.

My only real point of departure from Rowell's recommendations concerns the training given to those physicists destined for the industrial sector. I disagree with the idea that a terminal master's degree should be the "working degree" for these people. A better approach is not less education but more.

Before elaborating, I should sketch my background to establish my reasons for advocating a different approach. I received my PhD in condensed matter physics in 1975 from the University of South Carolina, based on research at the Savannah River Laboratory. I spent two years as a research assistant at Case Western Reserve University before teaching physics at North Georgia College. For the last 14 years I have served as a senior physicist at Philips in an R&D laboratory; I have several journal articles, patents and proprietary processes to my credit. Recently I have received an MBA with specialization in marketing and product development.

I agree with Rowell that one of the main problems with physicists in industrial research is their emulation of the academic model of how and why research is conducted. Most new PhDs emulate the academic approach because it is the only one they have seen. I propose that the PhD track be split: There would be an academic route for those wishing to teach or conduct basic research. For those entering industry, specialized training in industrial R&D methods and business theory could be added. Theses on industrial topics with dissertation committee members drawn from relevant industries would help insure that an industrial focus rather than an academic one is maintained. In the ideal situation the research topic would be one of interest to industry, and the research would be performed in an industrial lab under the direction of industry scientists.

The Japanese and others have beaten us in bringing technology to market because we teach our scientists that applications are not worthy of the talents of a true scientist. I and others in industry can attest that PhD-level people are needed there to understand the basic research being done in universities and government labs. High-level understanding and practical skills must be blended if we are ever to become the world leaders in commercializing our excellent basic research. Less education does not

seem to be the answer. We need university faculty who understand and work with industry and teach those skills to their students.

JAMES L. STEVENS
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5/92

John Rowell has written a marvelous article that should be required reading of all university professors and lab directors and managers. I think he is absolutely right: There is now a glut of knowledge that far exceeds the demand. Following Rowell's recommendations I would thus urge my faculty colleagues to reduce the number of papers that they write (and that nobody reads), the number of proposals that they prepare (and that don't get funded) and the number of PhDs whom they graduate (and who can't find jobs). Of course, I have no intention of adopting such a policy myself.

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Sharper Images of Mri's Origins

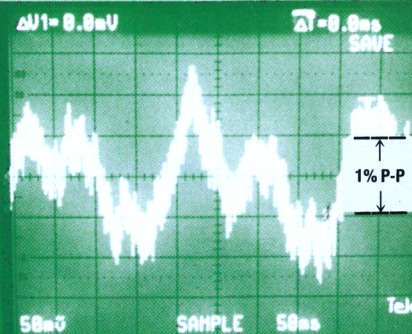
Felix W. Wehrli's article "The Origins and Future of Nuclear Magnetic Resonance Imaging" (June 1992, page 34) is an excellent overview of the field; however, there are significant omissions in the discussion of the historical development. In fairness to Wehrli, the history of nmr imaging, which is now termed magnetic resonance imaging, is not well documented.

Wehrli states that "numerous technological hurdles had to be overcome before nmr could progress to clinical practicality." He is certainly correct about that, but without discussing those hurdles, in the next sentence he states, "By 1980 whole-body experimental nmr scanners were in operation." Between those two sentences are ten years of intensive research and development to bridge the gap between the concept that it might be possible to actually achieve useful magnetic resonance images and the achievement itself. In fact, none of the early concepts for methodology were possible routes to achieving a practicable imaging system.

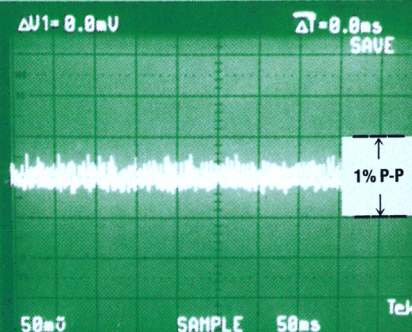
Since my students and I were front-line participants in overcoming the "numerous technological hurdles" and in designing and building the first clinically useful mri machines, it is an easy matter for me to provide some of the history missing from Wehrli's article.

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