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 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.

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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 frontline 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.

In 1959 our laboratory (Melvin

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Calvin's lab at the University of California, Berkeley, more specifically) carried out the measurement of blood flow in mice.[1] This experiment was an important basis for showing, for the first time, that nmr could be usd to obtain biological data from living creatures. By 1970 we had carried out nmr measurements of blood flow in humans,[2] showing the feasibility of nmr measurements within the living human. The first use of surface coils for nmr observations within the body was successfully demonstrated in the same paper.

By 1979 our group had designed and built the first nmr imaging system that could practicably provide clinically useful images of the human body. To accomplish that goal we used the spin-echo technique invented by Erwin Hahn (University of California, Berkeley).[3] Without the use of Hahn's spin echoes and his discovery of free induction decay,[4] there would have been no mri. In addition, we had to develop some completely new techniques,[5] including spin-echo sequencing, multislice imaging, specialized radiofrequency pulse sequences and rf coil designs. These were all incorporated into a system that provided whole-body images with millimeter resolution in minutes (rather than days, which was the best possible hope with other imaging methods).

One of our other innovations, which turned out to have a major effect on the development of physics, was to use a superconducting magnet—in fact, what was then the largest and most homogeneous superconducting electromagnet in the world, designed and built to our specifications by Oxford Laboratories. When our system went into production, it induced very significant commercial interest in superconductivity and stimulated (by an indirect path) research on superconductivity and high-temperature superconducting materials. (The cross-fertilization of physics research is a personal fascination of mine.)

Because of mri, many people benefit from accurate diagnoses that circumvent exploratory surgery and permit expedient disease treatment. Mri also has provided medical *research* with an enormously powerful weapon. With mri, it is now feasible for physicians to make frequent or even continual observations of disease development and the effects of prescribed treatments on patients without incurring the risk of radiation problems. Such time sequencing of disease observation is not very practicable with x-ray or radioactivity-based imaging techniques. By contrast, mri provides a detailed look at soft tissue and is completely harmless. (In the course of our research, I have personally been in our mri systems for many hours at a time.)

Another aspect of mri that is of major importance to medical diagnoses and research is the measurement of blood flow.[6] With mri, blood flow can be imaged and measured in detail, even to the extent of determining the absolute values of flow as it changes with the heartbeat cycle.[6,7] While the crystal ball is always cloudy, it is my belief that blood flow measurements will prove to be one of the major benefits of mri.

References

1. J. R. Singer, Science **130**, 1652 (1959).
2. O. C. Morse, J. R. Singer, Science **170**, 440 (1970).
3. E. L. Hahn, Phys. Rev. **80**, 580 (1950).
4. E. L. Hahn, Phys. Rev. **77**, 297 (1950).
5. L. E. Crooks, J. Hoenninger, M. Arakawa, J. R. Singer, US patent 4 297 637, issued 27 October 1981 (assigned to U. Calif.); US patent 4 318 043, issued 2 March 1982 (assigned to U. Calif.); US patent 4 471 305, issued 11 September 1984 (assigned to U. Calif.); US patent 4 599 565, issued 8 July 1986 (assigned to U. Calif.).
6. J. R. Singer, L. E. Crooks, Science **221**, 654 (1983).
7. J. R. Singer, in *Cardiovascular Radiology*, D. F. Adams, ed., Springer-Verlag, New York (1986), p. 172.

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Felix W. Wehrli's article focuses on the development of clinical nmr imaging from the early 1970s on. One paragraph of the article might be interpreted as implying that a radically new component—the generation of spatial maps of spin distributions—was first added to nmr technology in 1973 with the superposition of "magnetic field gradients onto the main magnetic field to make the resonance frequency a function of the spatial origin of the signal." A radically new component *was* introduced in the 1970s, but it was not the basic concept of spatial localization and spin maps, which had already been introduced for one (spatial) dimension in the early days of nmr.

The new component of the 1970s is perhaps best described as the vision that a useful spin map as complicated as an interior medical image was in principle obtainable and was a goal worth pursuing. The remarkable achievement of excellent medical images in two and three spatial dimensions resulted from the foresight and determination of a small group of persons including Paul C. Lauterbur, Peter Mansfield, Raymond Damadian and others, who were soon helped by a rapidly increasing number of creative colleagues from a wide range of disciplines. This development was also grounded in the spectacular gains that had recently been achieved in signal sensitivity as well as in computer speed and memory.

To the best of my knowledge the idea for superimposing a magnetic field gradient onto the main homogeneous magnetic field had its origin in the self-diffusion effects Erwin L. Hahn observed on his spin-echo envelopes as nuclei diffused through the small residual inhomogeneity of his main magnetic field.[1] Based on this clue, Edward M. Purcell and I intentionally superimposed a strong magnetic field gradient onto the main field, giving a linear dependence of the resonant frequency on the spatial position of the diffusing nucleus.[2] The enhanced diffusion effect then enabled us to make accurate quantitative measurements of the self-diffusion coefficient for suitable fluids.

In my 1952 Harvard thesis I described a one-dimensional phantom. It was constructed to produce an nmr response similar to the newly discovered chemical shift in ethyl alcohol. I used the imaging concept, with its superimposed gradient and Fourier transformations, to relate the one-dimensional frequency-encoded spatial structure of the phantom to the nmr response in the time domain. Similar one-dimensional phantoms are currently used in mri textbooks to introduce the imaging concept. The most notable physics application of a simple one-dimensional spin map was in the discovery[3] at Cornell University of the superfluid states of helium-3. Today's two- and three-dimensional medical images, envisioned by the pioneer workers of the 1970s, involve complex pulse sequences and transformations now attainable with modern computers.

References

1. E. L. Hahn, Phys. Rev. **80**, 580 (1950).
2. H. Y. Carr, E. M. Purcell, Phys. Rev. **94**, 630 (1954).
3. D. D. Osheroff, W. J. Gully, R. C. Richardson, D. M. Lee, Phys. Rev. Lett. **29**, 920 (1972).

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I enjoyed Felix W. Wehrli's article very much. Wehrli's thorough review emphasized the important role that mri is playing in diagnostic medical imaging. I was particularly delighted with his observation in the section labeled "Early history" that "this fascinating branch of science and technology has its roots in work performed ... in 1938 [by] I. I. Rabi and his colleagues." Wehrli's brief account of the techniques used in Rabi's historic nuclear magnetic resonance experiment for direct measurement of nuclear magnetic moments, for which Rabi received a Nobel Prize in 1944, contains some factual errors, however. (This should in no way detract from Wehrli's otherwise excellent review of nmr imaging.)

As one of Rabi's first graduate students in the Columbia University Molecular Beam Laboratory and as a collaborator in the 1938 molecular-beam magnetic resonance experiment, I am happy to describe briefly the sequence of relevant events that led up to that experiment. The "atomic beam of silver atoms" cited by Wehrli was not used in the 1938 experiment. It was originally used in the Stern–Gerlach experiment in Otto Stern's laboratory in Hamburg in the mid-1920s. As a physics graduate student at Columbia University, Rabi was very much intrigued with that experiment. During the two years he went abroad on a fellowship (1927–29), principally to learn the new quantum mechanics as it was evolving, he spent some time in Stern's Hamburg laboratory. At Stern's suggestion Rabi performed an experiment with an atomic beam of potassium that was similiar to the Stern–Gerlach experiment but used a novel magnet configuration. Rabi returned to Columbia in 1929 as an assistant professor in physics and soon thereafter started the Columbia molecular-beam lab. His first student, Victor W. Cohen, used a sodium atomic beam to measure the nuclear spin of Na and the hyperfine structure of the ground state of ^{23}Na, from which the nuclear magnetic moment could be calculated (approximately). I did a similar experiment in 1935 with potassium atoms, determining the nuclear spins of ^{39}K and ^{41}K and measuring the hyperfine structures of the ground states. These experiments used beams of atoms and required relatively weak magnetic fields to deflect and study the behavior of the beam as a function of the applied field.

The 1938 experiment, however, was the first molecular-beam experiment that used molecules (for example, LiF, LiCl and NaF) for direct measurement of a nuclear magnetic moment. Since nuclear magnetic moments are very much smaller than atomic moments, one had to use a magnetic field with a strong gradient (approximately 100 000 gauss/cm) to deflect the beam (by about 0.01 mm) in one direction and another magnet of equal and opposite gradient to bring it back. Resonance was observed when a radio-frequency source placed in a homogeneous magnet between the two deflecting magnets was tuned to the Larmor precession frequency of one of the component nuclear moments of the molecule (for example, fluorine). The details of this experiment were described not in the 1938 publication cited by Wehrli but in a subsequent publication[1] in March 1939.

Reference

1. I. I. Rabi, S. Millman, P. Kusch, J. R. Zacharias, Phys. Rev. **55**, 526 (1939).

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WEHRLI REPLIES: Jerome R. Singer, Herman Y. Carr and Sidney Millman all take issue with my delineation of the history of nmr and mri.

First, let me point out that PHYSICS TODAY's limit on the number of references in articles makes it virtually impossible to do justice to the many pioneers whose works were milestones in the evolution of nmr from a physics experiment to clinical mri. There remains the exceedingly difficult and awkward task of appropriately weighting individual contributions in retrospect. The problem is exacerbated by the many parallel developments, particularly those in the evolution of mri during the 1970s.

I am very much indebted to Millman for pointing to a factual error in my brief account of the very early history of nmr, covering the era before the phenomenon was first described in the condensed phase. I apologize for the misquote about the "atomic beam of silver atoms." I chose to cite a 1938 letter to the editor (reference 1 in my article) because it shows what I believe was the first nmr resonance line obtained by sweeping the field across the resonance.

Carr takes issue with the paragraph where I wrote: "A radically new dimension was added ... in 1973 when Paul Lauterbur at the State University of New York at Stony Brook first proposed generating spatial maps of spin distributions by what he called 'nmr zeugmatography.' Key to this method was the idea of superimposing magnetic field gradients onto the main magnetic field to make the resonance frequency a function of the spatial origin of the signal." (I cited Lauterbur's 1973 paper in that same paragraph.) I agree with Carr that the idea of making the resonance frequency a function of spatial position by superimposing field gradients on the main magnetic field precedes Lauterbur's zeugmatography experiment. To the best of my knowledge, one of the first to describe the behavior of the nmr signal in the presence of a field gradient was Robert Gabillard, who showed that the beat envelope of a rapid passage signal is the Fourier transform of the distribution of the magnetic field across the specimen.[1] I am indebted to Carr for his mention of his 1952 Harvard thesis, where he says he showed what could in modern language be termed a "one-dimensional projection image of an object." The innovation in Lauterbur's work is that he was the first to create a two-dimensional map of spin density from a plurality of projections produced by rotating a gradient in angular increments, and to combine this novel idea with already known back-projection techniques. Whether or not the combination of these diverse concepts should be ranked as a "radically new idea" seems conjectural.

Singer states that there were "significant omissions in the discussion of the historical development" I sketched in my article. I have high regard for the work by Singer, Lawrence Crooks and Leon Kaufman that led to the scanner at the University of California, San Francisco, to which he refers, and thus I had no intention to willfully ignore these significant contributions. Singer says that he and his students "were frontline participants in ... building the first clinically useful mri machines." While they certainly were significant players in the quest to turn zeugmatography into a clinically practical imaging procedure, many others deserve to be mentioned as well. The first description in the open literature of the UCSF whole-body system that I could find was a 1982 paper by Crooks and colleagues,[2] although I believe whole-body images from the UCSF scanner were shown at the Bowman Gray School of Medicine meeting on biomedical magnetic resonance in the fall of 1981. At that time, clinical trials were already in progress at several centers.[3,4] In the mid-to-late 1970s there were, in fact, several parallel efforts that led to the design and construction of whole-body mri machines. These include at least three concurrent but largely independent programs in the United Kingdom—at Thorn EMI Ltd and Hammersmith Hospital,[5] at the University of Nottingham[6] and at the Royal Infirmary at Aberdeen[7]—as well as the early work conducted by commercial companies like Technicare, Siemens, Philips and, somewhat later, General Electric. To do justice to all these contributors I would have had to discuss and cite much of this work, which would clearly have been beyond the scope of my article.

The development of multislice imaging by Crooks and colleagues,[2] that is, the idea of using the relaxation delay following excitation and spatial encoding to excite a series of adjacent slices, to which Singer seems to allude, enormously enhanced the clinical practicality of mri. Although this multiplexing scheme was an important innovation, which improved scanning efficiency by an order of magnitude, it is by no means the only efficient imaging mode currently practiced. (An alternative is three-dimensional volume imaging in combination with short radiofrequency-pulse repetition times.[8]) Singer's comment that some of the techniques pioneered in his laboratory "provided whole-body images with millimeter resolution in minutes (rather than days, which was the best possible hope with other imaging methods)" is not accurate. In fact, most early imagers, of which many preceded the UCSF scanner, produced images in a scan time of well under an hour[3,5]—not to mention Peter Mansfield's echo-planar imaging, conceived[9] in 1977, which later proved to afford whole-body images in fractions of a second.[10]

I would also take issue with Singer's claimed innovations with respect to superconductive magnets. To the best of my knowledge, the first superconducting whole-body magnet incorporated into an mri system was the one at Hammersmith.[11] This magnet, like the UCSF magnet, was built by Oxford Instruments (not "Oxford Laboratories"). The Hammersmith researchers elected to run it, however, at 1.5 kilogauss, rather than at 3.5 kG as the UCSF researchers did. To the credit of Singer and his coworkers, it should be mentioned that they were the first to produce images at more than twice the previously used field strength—images that were widely regarded as superior to those made at lower field—and thus spurred the development of high-field imaging (at field strengths of 1.5 tesla) by, for example, the General Electric Company.[12]

Singer deserves much credit for his pioneering contributions to the measurement of blood flow by nmr. He was presumably the first to measure blood flow in a live animal and, subsequently, in humans. Again, however, this work ought to be placed into the context of other investigators' contributions, such as the ones by Robert L. Bowman and V. Kudravcev at the National Heart, Lung and Blood Institute in 1956 and the work at the Medical College of Wisconsin that led to the construction[13] of an nmr limb blood-flow meter in about 1980. It is interesting in this context that the first blood-flow effects in an nmr image were observed in what is widely regarded as the earliest *in vivo* human image.[14] In my article I chose to cite a paper by Charles L. Dumoulin and colleagues[15] because it describes a method for magnetic resonance angiography that is currently practiced (and that may be regarded as building on motion-induced phase shifts described by Singer[16]).

Finally, how differently people can view and write history is exemplified by an article by nmr pioneer Erwin L. Hahn.[17] Upon reading my article, Hahn kindly sent me a reprint of his paper, of which I was not aware, together with a note that read, "It is my impression that radiologists view the history somewhat differently." In fact, only five references in the two articles are identical. In fairness to both of us, the origins of nmr imaging were only one aspect of my article.

References

1. R. Gabillard, C. R. Acad. Sci. (Paris) **232**, 1551 (1951); Phys. Rev. **85**, 694 (1952). E. R. Andrew, *Nuclear Magnetic Resonance*, Cambridge U. P., Cambridge, England (1969).
2. L. Crooks, M. Arakawa, J. Hoenninger, J. Watts, R. McRee, L. Kaufman, P. L. Davis, A. R. Margulis, J. DeGroot, Radiology **143**, 169 (1982).
3. R. C. Hawkes, G. N. Holland, W. S. Moore, B. S. Worthington, J. Comput. Assist. Tomogr. **4**, 577 (1981).
4. F. W. Smith, J. R. Mallard, A. Reid, J. M. S. Hutchison, Lancet **1**, 963 (1981). G. M. Bydder, R. E. Steiner, I. R. Young, A. S. Hall, D. J. Thomas, J. P. Marshall, C. A. Pallis, N. J. Legg, Am. J. Roentgenol. **139**, 215 (1982). R. J. Alfidi, J. R. Haaga, S. E. Yousef, P. J. Brian, B. T. Fletcher, J. P. LiPuma, S. C. Morrison, B. Kaufman, J. Richey, W. S. Hinshaw, D. M. Kramer, H. N. Yeung, A. M. Cohen, H. E. Butler, A. E. Ament, J. Lieberman, Radiology **143**, 175 (1982). F. S. Buonanno, I. L. Pykett, T. J. Brady, P. Black, P. F. J. New, E. P. Richardson Jr, W. S. Hinshaw, M. Goldman, G. Pohost, J. P. Kistler, J. Comput. Assist. Tomogr. **6**, 529 (1982).
5. I. R. Young, A. S. Hall, C. A. Pallis, N. J. Legg, G. M. Bydder, R. E. Steiner, Lancet **2**, 1063 (1981).
6. P. Mansfield, I. L. Pykett, J. Magn. Reson. **29**, 355 (1978).
7. W. A. Edelstein, J. M. S. Hutchison, G. Johnson, T. Redpath, Phys. Med. Biol. **25**, 751 (1980).
8. C.-M. Lai, P. C. Lauterbur, Phys. Med. Biol. **26**, 851 (1981). D. Matthaei, J. Frahm, A. Haase, W. Hänicke, K. D. Merboldt, Magn. Reson. Imaging **4**, 381 (1986).
9. P. Mansfield, J. Phys. C **10**, L55 (1977).
10. R. Rzedzian, P. Mansfield, M. Doyle, D. Gulfoyle, B. Chapman, R. E. Coupland, A. Chrispin, P. Small, Lancet **2**, 1281 (1983).
11. I. R. Young, M. Burl, G. J. Clarke, A. S. Hall, T. Pasmore, A. G. Collins, D. T. Smith, J. S. Orr, G. M. Bydder, F. H. Doyle, R. H. Greenspan, R. E. Steiner, Am. J. Roentgenol. **137**, 895 (1981).
12. H. R. Hart, P. A. Bottomley, W. A. Edelstein, S. J. Karr, W. M. Leue, O. Mueller, R. W. Redington, J. F. Schenck, L. S. Smith, D. Vatis, Am. J. Roentgenol. **141**, 1195 (1983).
13. R. E. Halbach, J. H. Battocletti, S. X. Salles-Cunha, A. Sances, Med. Phys. **8**, 444 (1981).
14. W. S. Hinshaw, P. A. Bottomley, G. N. Holland, Nature **270**, 722 (1977).
15. C. L. Dumoulin, S. P. Souza, M. F. Walker, W. Wagle, Magn. Reson. Med. **9**, 139 (1989).
16. J. R. Singer, J. Phys. E **11**, 281 (1978).
17. E. L. Hahn, Philos. Trans. R. Soc. London, Ser. A **333**, 403 (1990).

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A Place to Publish Unorthodox Thought

I write to comment on the letter from Troy Shinbrot that appeared under the heading "A Journal for Unorthodox Thought?" (March 1992, page 102). I founded the journal *Speculations in Science and Technology* here in Australia in 1978 and was its editor for its first six years. It then passed to England and has since that time been edited by Alan L. Mackay of Birkbeck College at the University of London. My accounts of the many features, peculiarities and difficulties of running such a journal have been published in several other journals[1] as well as in the many editorials I wrote that appeared in the pages of *SST.* Emilio Panarella's journal *Physics Essays*, published by the University of Toronto, plays a somewhat similar role.

I support Shinbrot's suggestion and advise that almost all the operating tasks involved are unique. In my six years as editor *SST* published about 500 papers from a submission list of about 2000. About 40% of the papers submitted were on one subject: the refutation of special relativity. I found the tasks sometimes depressing, sometimes exhilarating and always a liberal education on the peculiarities of the human mind.

The question I am often asked is, What new ideas have come out of the journal? This has always been difficult for me to answer. During the