cerned about is that the very best new PhDs have been having significant difficulties finding permanent employment at the same time that many people, including physicists of Scott's generation, have been led to believe that a shortage of scientists—defined as a surplus of permanent jobs—exists. At the time I wrote my letters (October 1990, page 13; May, page 99) very few older physicists were convinced that there was any problem with the job market. My letters were designed to educate young and old physicists about the employment problems facing my generation.

The general public still believes that there is a scientist shortage; just ask your closest nonscientist friend. Better yet, ask your representatives in Congress who passed the Immigration Reform Act of 1989 based, in part, on a belief in that shortage. Although I don't think that particular law is bad, I do think that Congress should make decisions based on accurate information.

Scott's comparisons of physics PhDs to philosophy and English literature PhDs and jugglers leave a lot to be desired. First, the government spends a great deal of money on each physics PhD produced. Second, I doubt very much if the public and Congress would pay much attention to a projected shortage of PhDs in philosophy and English literature, or of jugglers. Finally, according to a philosophy professor I know, philosophy departments send all applicants for graduate school a letter that explains the poor employment prospects for PhDs in philosophy. It would be nice if physics departments would do the same.

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Scanning Tunnel Vision

I recently attended a meeting that included presentations on scanning tunneling microscopy. Almost every talk used a different method of presenting the STM images, and many of them used more than one form within a single talk. Moreover the majority of these methods of presentation seemed to obscure rather than communicate the information in the image.

I write this letter to appeal for uniformity in presentation and to offer my strongly prejudiced opinion as to which method should be chosen.

STM micrographs are presented with the value of the measured parameter at each pixel represented by vertical displacement ("*y* modulation"), by color, by intensity (gray scale) or by any combination of the above. In addition, micrographs are presented at normal incidence, in isometric projection or in perspective view; they can have shading as if obliquely illuminated or not.

The reasons for some of this confusion are clearly historical. Early images from scanning tunneling microscopes were recorded using repeated traces on a pen recorder. In this case there is no choice but to use vertical displacement to represent the signal. Very soon, however, computer graphics presentation took over and that has led to the present mess. Two things seem clear to me. First, *y* modulation has been retained for no good reason—except that the pen-recorder plots established the habit. Second, many of the other tricks have been introduced because the computer permits them, not because they aid scientific communication.

The data consist, after all, of a two-dimensional array of scalars. The natural way to present such a data set, and the method that would be used in any other field, is a monochrome image in which the intensity at each pixel represents the value of the scalar. Replacing the gray scale by color contouring (as in geographic maps) is useful when the dynamic range of the data is too great for reproduction or visual perception in monochrome. Any additional tricks seem to me to be counterproductive.

Now I concede that for presentations to managers or to funding officers, it may be appropriate to use an image that is visually spectacular, but for scientific purposes, can we please agree that communication of information and consistency are more important?

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Central Bureaucracy Stifles Good Research

John J. Gilman's generally perceptive article on research management (March, page 42) ignores basic changes in the structure and *de facto* purpose of research organizations that have taken place over the last 20 years. The structural change has been the rise of a permanent central administrative bureaucracy, funded by overhead. These days researchers, projects and even sponsors may come and go, but the central bureaucracy remains. The *de facto* purpose of the research is to provide sufficient overhead to support the central bureaucracy. In their quest for stable support, the central bureaucracies favor Grotesque Large Useless Projects (GULPs) over Single Investigator Projects (SIPs), to the detriment of us all.

The central authorities also cope with the "rule of tens" in a characteristic way: They give support and assign tasks "to each according to need, from each according to ability to produce," with all decisions about "needs" and "abilities" made by the central authority. The best way to thrive is to curry favor with the central authorities while always being on the verge of an important development, never actually achieved. This distribution method does not enhance the productivity of the productive.

Our present bureaucratized research laboratories are less similar to Thomas Edison's Menlo Park laboratory than to Aleksandr Solzhenitsyn's Mavrino Sharaska (in *The First Circle*). The public sees occasional results of this shift in organizational principle in the form of an exploding space shuttle or a Congressionally humiliated university president. They do not see the opportunities missed, except in the form of high-technology imports. However, the present situation of the part of the world that originated this sort of research organization (30 years before we adopted it) should give us all pause.

It may be an easier matter for the Federal government to alter procedures to encourage success, punish failure and emasculate the central authorities in government-supported laboratories than for industry to reform its own procedures. The businessmen who control all the companies large enough to afford research programs understand that their overseas competitors are more capable of profiting from innovation than are their own organizations. Thus stifling innovation is actually a valid competitive strategy so long as most innovation comes from American laboratories. The Mavrino organizational paradigm succeeds at this admirably. Rational reform of industrial research requires re-energizing the entrepreneurial capacity of American industry.

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GILMAN REPLIES: Many researchers can sympathize with the emotions implied in the author's first paragraph. Research administration has come a long way from Edison's first system for keeping books, which consisted of spearing invoices on a spindle and paying them as he found the time. This first in–last out system infuriated his creditors, so he soon had to complicate it. Minimalism has suffered ever since.

The central bureaucracies of research organizations have grown disproportionately for many reasons, so they will not easily be shrunk. For example, there are more interactions *per capita* in large than in small organizations, so more coordinators (more "traffic lights") *per capita* are required. Also, researchers want (and often need) more sophisticated services *per capita* than was once the case. Sometimes these can be contracted for externally, but not always. Furthermore, many administrative requirements have been externally imposed. Those in the area of health, safety and environment come quickly to mind. These are often desirable, but they do require larger administrative bureaucracies. To be efficient and controlled, bureaucracies require authoritarianism. But this has not been acceptable to a majority of researchers; they have opted for elitism instead. This has led to hierarchies, plus parallel hierarchies, of vice presidents, deputies, associates and assistants, *ad nauseam.* Thus budgets for indirect staffs have risen from on the order of 5% of the total to 20% or more in recent years. Concurrently these staffs have co-opted power, as the author indicates. Unpopular as it would be, the only effective recourse is probably an increase of authoritarianism, which would allow smaller indirect staffs.

Within organizations, positive interactions that improve effectiveness (people-to-people interactions, mostly) tend to increase as a power function of the organizations's size, with the exponent decreasing from 1 toward 0, while negative interactions (memos-to-people, mostly) tend to increase as a rising power function of size. Therefore an optimum size exists. This optimum varies with the nature of the task that the organization is trying to accomplish. When it is exceeded, as it has been in recent times in such institutions as steel mills, banks and governments, efficiency suffers, and the probability of collapse increases. Unfortunately, the coefficients involved are not known; only intuition is available as a guide. My opinion is that for most kinds of research (not development) work the optimum is in the neighborhood of 100. That is, 10 is too small and 1000 too large. If this is correct, it

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implies that many research organizations are already too large, and that therefore they should budget not just their costs but also their staff sizes. This would cause some of the author's complaints to be minimized.

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Solitary Wave Preoccupations

James Krumhansl (March, page 33) credits D. J. Korteweg and G. de-Vries[1] with the resolution of the conflict between J. Scott Russell's observation of the solitary wave and G. B. Airy's claim that such a wave could not exist. Russell discovered and named the solitary wave in 1834, carried out laboratory experiments in 1834 and 1835, and reported his investigations[2] at the British Association meeting of 1837. The contradiction with Airy's prediction (on the basis of his shallow-water equations, which neglect dispersion) that a wave of finite amplitude cannot propagate without change of form was resolved independently by Joseph Boussinesq[3] (1871) and Lord Rayleigh[4] (1876), who showed that the increase in local wave speed associated with finite amplitude is balanced by the decrease associated with dispersion. The seminal contribution of Korteweg and deVries was to combine the assumptions of weak nonlinearity and weak dispersion with that of unidirectional propagation to obtain the nonlinear partial differential equation that today bears their name. Their work may fairly be said to have stimulated the present-day interest in solitary waves and other localized, coherent structures.

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KRUMHANSL REPLIES: I thank John Miles for calling attention to several significant 19th-century contributions to the understanding of solitary waves that neither space nor the context of my retiring APS presidential address (on which my March article was based) allowed me to discuss in detail. The references he provides may be found discussed at further length in the books named in reference 2 of my article, particularly the volume by M. Ablowitz and H. Segur. Further amplification is to be found in the historical discussion by Alan C. Newell.[1] Certainly Joseph Boussinesq[2] made important contributions to this topic, including discovering several new conserved quantities that we now recognize as an essential feature of integrable soliton-bearing equations (which have an infinite number of conserved quantities—that is, constants of integration). However, Newell points out that Boussinesq's solution still suffered from being bidirectional, whereas the Korteweg–deVries analysis finally provided an integrable nonlinear equation that had the key properties we now associate both theoretically and experimentally with solitons.

At the same time, it is important not to leave the impression that the competition between nonlinearity and dispersion in a wave excitation is either an essential or a necessarily useful way to think about solitons in general. In any case it is limited to small-amplitude nonlinear perturbations. There is a large class of problems, namely those defining topological solitons, as in the Sine Gordon equation, whose solutions may be entirely static (not wave-like at all) and must have only discrete amplitudes. These appear prominently in quantum field theory, condensed matter physics and structural phase transitions. In spite of the fact that they are completely different in physical nature from water waves, the general soliton analyses apply.

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Socratic Pedagogy in Introductory Physics

In his thoughtful review (December 1990, page 67) of Arnold Arons's book *A Guide to Introductory Physics Teaching*, Charles Holbrow raises a few good questions: "The book is eye opening and informative, but is its program for improving the teaching of introductory physics feasible? . . . How much time is available for Socratic questioning of students? . . . Is this an approach that can be used in a course of 100 students—let alone 1000 students? . . . How worthwhile is the investment of these resources in generating understanding? . . . How much coverage are you trading for how much understanding?"

Our four years of experience[1] in using the method Arons advocates shows not only that it is feasible, but that it is extremely successful in promoting students' conceptual understanding of Newtonian mechanics as measured by the Halloun–Hestenes test.[2] At Indiana University we bring Arons to the masses in large, non-calculus-based classes for science (but not physics) majors, including prospective high school and middle school teachers, primarily by means of Socratic Dialogue Inducing laboratories. These labs emphasize interactive engagement with simple concrete experiments and promote conceptual change through "disequilibration," collaborative learning, extensive diagramming and Socratic dialogue. For the spring 1990 class of about 100 students this required an extra resource expenditure of about 6 professor-contact-hours per week averaged over the course of the semester. How much coverage did we trade? In that course we sacrificed coverage of waves and special relativity, treating only mechanics and thermodynamics in the first semester.

How worthwhile is the investment of resources? From the standpoint of most research universities it is not at all worthwhile, especially for a class of nonphysics majors. But perhaps research universities need to reexamine their priorities.[3] As Arons has written, "Were more of us willing to relearn our physics through the dialogue and listening process I have described, we would see a discontinuous upward shift in the quality of physics teaching."[4]

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