

“Hard Days on the Endless Frontier” Revisited

August's LSF article, “Hard Days on the Endless Frontier,” by Robert Pollack, generated a number of responses. The following letters are published, along with Dr. Pollack's replies, in order to continue the dialogue on morale in the biomedical sciences. *VTM*

I am pleased to respond to Dr. Marchesi's request to comment on the perspectives developed by Dr. Robert Pollack in his essay, “Hard Days on the Endless Frontier.” I was asked to respond in part because of my current position as President of the American Society for Nutritional Sciences, and as a scientist whose research activities bridge both medicine and agriculture at a public university. In this regard, I share Dr. Pollack's concern that morale is low in the scientific community.

As the readers of this series of letters know, Dr. Pollack developed a model to aid in describing the basis for our low morale. What I wish to do is add additional components to Dr. Pollack's model and expand on several of the points he makes. I would assert that dealing with 1) informational technology and automation, 2) academic administration, and 3) what is often labeled as postmodernism, particularly in the context of educational philosophy, are major contributing factors to low morale.

I include informational technology and automation because both have shortened the time lines we must give ourselves to remain competitive and work at the cutting edge. The scope of our work has forced many of us into difficult choices, choices that have had a clear impact on morale. For example, to address questions in modern biology, it is often necessary to use a team approach. The team approach, however, puts many of us in the awkward position of deciding how to negotiate ‘control’, i.e., we often have to give up some level of our personal autonomy or privacy. Another factor is ‘research focus’: we are asked to narrow our activities much more than we may wish in order to remain competitive.

The changes brought on by advances in informational technology and automation have also been accompanied by changes in administrative

structure and styles of management. Academic administration has become more corporate in nature and structure. Although this change has some virtues, it is associated with the need to make a number of difficult accommodations and transitions. As an example, consider the privatization of medical schools in order to accommodate HMOs, or the accommodations that occur when an academic department in agriculture services a given commodity group. Increased emphasis has also been given to bringing in grants and gifts. In this setting, morale is not enhanced when the limited resources that are accessible are utilized to bring in an ‘academic superstar’ rather than develop or sustain an established academic program.

Regrettably, the concept has evolved that the conduct of science and its administration can be separated. At many institutions, the administration of science, once delegated to the scientifically proven or, in some instances, elite, is now left to those with marketing skills or to those who no longer wish intellectually to explore Pollack's “frontier.” It is difficult to maintain high morale when leadership defines academic success relative to a business ledger, or follows the corporate model of moving on in order to move up the administrative ladder. I also concur with Dr. Pollack that our enthusiasm is compromised by the constant necessity of having to deal with complex regulations. Our administrative infrastructures have made it essential to acquire licenses and comply with what are often arbitrary regulations.

The American university clearly reflects changes in American society. Addressing issues that center on fairness, race, and gender are essential because they are morally and spiritually correct. Nevertheless, life in academe was easier and perhaps more friendly when we were more homogeneous. Several points can be made here. One concerns the issue of diversity as it relates to gender and race. For some, increased diversity in these areas has had a significant impact on morale. However, I think it is important to develop the issue of diversity in a broader context.

Simply stated, science was easier when there was less to know in order to get started. When we were less intellectually diverse, we were more fluent in each other's language. In basic biology, many of us received similar training as chemists or physical scientists. Now, we must confront a scientific knowledge base that expands exponentially.

To reiterate, we used to be smaller in number and more homogeneous. When I took enzymology some 30 years ago, my Professor, Dr. Bernard Axelrod, asserted that he would not welcome the pressures that he perceived would be placed on us. In his career, he had worked with or met every plant biochemist who had influenced his life or career. His point was that he had an intimate sense of his identity and history as a research scientist that we would not have.

When we were less diverse, we judged ourselves by a set of rather straightforward and intuitively understood criteria. This has changed over the past 15–20 years, in part because of changes in administration and our willingness to accept certain changes in academic philosophy. For example, many of us have been asked to accept the trappings of postmodern colleagues, whose conceptual constructs arise more from rhetoric and semantics than from linearly constructed hypotheses. In this setting, we have had to place more emphasis on quantitative, rather than qualitative, measures of academic worth. That is, we 'bean count' because we perceive difficulties in understanding the quality of each others' science.

The fields of academic research, whether social or physical, have never been closed clubs. Yet, there has been some effort to level the playing field so that, figuratively speaking, all can play. However, when expectations regarding performance make clear distinctions between levels of quality, I sense that there is less insensitivity caused by arbitrariness in judgment and, as a consequence, morale is higher.

In summary, I am certain that we have not changed intrinsically and become more insensitive. However, all of us have limits. Informational technology, automation, changes in administrative style, and the need to operate under a different set of standards—that, on occasion, can compromise our integrity—have tested such limits. Poor morale comes from having to adjust to too much. The good news is that since we can articulate reasons for low morale, we can address at least those features that are easy to resolve; I hope we will learn to adapt to the others. Regrettably, our reasons for poor morale are multifaceted, interconnected, complex, and in some cases self-inflicted. The process of resolution is going to take a while and require thoughtful insight.

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Professor Robert Rucker agrees that morale is low, and adds three other causes—information overload, the splitting of the administration from the performance of science, and the appearance of unexpected diversity in the larger world's opinion of the intrinsic value of any science—to my central explanation, the retraction of personal responsibility for one's behavior.

These three additional problems are all real, but only one is extrinsic; the other two are symptoms of precisely the withdrawal from responsibility I describe in my paper. The appearance of more data per unit of time, in more different venues each year, is a real issue, one we cannot address individually at all. The Web meets the predictions for any new technology: it offers a nicely balanced set of new solutions to old problems, and new problems for which there is as yet no solution. The Web brings the latest data to the bench scientist with unprecedented speed and focus; at the same time, it threatens the notion of fair peer review of any bench science, because reviews can be ignored when every PC is its own journal, with its own table of contents on its own home page.

If there is a solution here, it seems to me to lie in the willingness of practicing scientists to put some time into constructing the formalism for peer review of results on the Web. That would, of course, be an aspect of administration, which brings us to the first of Rucker's other two causes: the professionalization of administration and its subsequent isolation from practice. The only honorable way for a scientist to serve in an administration would be for the administration to acknowledge in advance that the scientist would be free to see all relevant data and draw her own conclusions, regardless of the larger administrative agenda. Surely it is for any practicing scientist simply to say no to committees and central administrative appointments, the agendas of which are to co-opt the presumption of scientific objectivity by having scientists sign off on matters about which they have no expert knowledge. I lasted 7 years as Dean of Columbia College, and left when I was no longer free to maintain a scientist's standard of objectivity and honesty.

The third cause is old news. Bright people from new backgrounds, who neither like scientists nor trust science—or the other way around—are what the academic world out there is largely made of. What is new is the appearance of people holding such opinions within the world of scientific practice. Such people are telling us all something very important: it is possible to do good science without becoming orthodox in one's religious observance of its verities. As for being uneasy with new scientists who happen to be

cast in different shapes and colors than one's own: well, wake up and smell the coffee!

RP

Dear Dr. Pollack:

I enjoyed reading your article, "Hard Days on the Endless Frontier" (*FASEB J.*, August 1997) enormously.

Although a funded scientist working in free radicals and antioxidants, I, too, have wondered whether I should not give up applying for more and more grants and concentrate on 'learning by teaching'. Our best ideas are not always the ones we put in the grant applications (because we might not have them at the time of writing the application). Having to follow the aims of the work we stated in our grant may hinder research. An advantage of our more relaxed, 'Mediterranean' ways of doing things is that we do not have to follow the work plan of the grant application as strictly as you do in the U.S. In Spain, as in the U.S., grant applications are subjected to peer review. The peer review process in Spain, which is serious and preserves the anonymity of the reviewers, asks the reviewers to concentrate primarily on whether the applicant has published good work in previous years and not whether he/she achieved the aims of past grants.

Regarding the question of morale, I find that you are completely right, but I do not think it is a new problem. My teacher and thesis supervisor, the late Sir Hans Krebs, told me that in Germany, in the 1930s, team spirit was almost absent (people, he said, even mislabeled lab bottles intentionally). This was in great contrast with what he found in Hopkins' lab in Cambridge, England, when he was expelled from Nazi Germany. However, he was able to produce great work in both situations (the urea cycle, Freiburg, Germany, 1932, and the 'Krebs cycle', Sheffield, England, 1937).

I liked the explanation of the French roots of the word 'morale' and also the fact that in Middle English 'to profess' meant to take vows. Indeed, in modern Spanish, as the *Diccionario de la Real Academia de la Lengua* (the official Spanish dictionary) shows, 'profesar' still means to take vows. I hope that all university professors will be able to live up to the standards of those vows.

Let me once again congratulate you on the excellence of your paper.

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Response to Professor Viña

Professor Viña agrees that a peer review of grant applications that fails to reward past productivity is the source of considerable pathology among scientists in the U.S., and provides us with a valuable counterexample in the Spanish version of things.

His observation that Professor Krebs was able to be productive in both competitive and cooperative lab venues is not surprising, but neither is it determinative. The fact that someone can transcend a bad situation is not cause to put off changing that system, for the sake of unnamed younger scientists who ought to be spared unnecessary pain.

In passing, I do have to wonder whether in 1932 Professor Krebs' concentration was not focused greatly by the looming threat of fiscal, psychic, physical, and, finally, mortal punishment from the Nazis. As we know, that threat did cost the lives of a number of German scientists and physicians of Jewish extraction in the following decade, so it is perhaps unfair to use the success and subsequent emigration of Dr. Krebs as predictive of any other scientist's ability to thrive under less dangerous kinds of selective pressure.

RP

This letter is in response to Robert Pollack's essay, "Hard Days on the Endless Frontier." I found Pollack's essay insightful and a bit stirring. Since I am a graduate student whose 'light' has finally appeared at the end of my educational tunnel in the biomedical sciences, I am growing increasingly concerned with the state of biomedical research. The *raison d'être* for the Ph.D. is becoming vague in this age of decreased funding and decreased morale (to which Pollack eludes). With a small number of academic appointments relative to the number of new Ph.D.'s, alternative careers are a necessity. One possible fate of the doctoral recipient could be in the field of scientific mediation. The following analogy can easily illustrate the need for scientific mediators as well as many of the problems in the biomedical sciences that Pollack discussed.

Until now, the accumulation of biomedical knowledge could be viewed as building an upside-down pyramid. Each scientific problem that was solved brought with it many more questions, so each successive tier of the pyramid gets larger. Each tier also brought with it more researchers to answer these questions. The pyramid has become increasingly large; successive tiers result in its constitutive blocks becoming relatively smaller, and seemingly less significant. Drawn from this analogy, a decrease in morale is inevitable, and researchers encounter difficulty finding meaning and purpose in their work. Funds become more and more scarce, so fewer blocks can be filled in on each tier. Without organization of

the blocks, the structure will eventually become unstable. With unlimited funds, the pyramid would continue to expand; however, with only limited resources, the shape of the structure must change from pyramidal in order to adopt vertical borders that are parallel—similar to an upside-down Washington monument—or the structure will become diffuse and lose meaning.

Pollack describes the state of biomedicine in his essay, "All the social structures that connect scientists to one another will be based solely on each scientist's latest piece of individual work: a Hobbesian world of each against all." This notion of a Hobbesian world is particularly scary. In the early days of science, it was more commonplace to view the works of an individual scientist as significant. But early scientists, even as early as the middle of this century, were low on the pyramid and more noticeable. As time goes on, it becomes harder to be noticed from the outside. We need to remove the word 'recognition' from our definition of significance, and our career goals should shift from satisfaction of personal aims to a contribution to the greater good. The Hobbesian philosophy even states that the fruits of selfish aims will be relinquished to an absolute monarch if it is for the greater good. After all, the individual fibers of a rope are much stronger when woven together.

Many have expressed the need for scientific mediators and a more integrative emphasis on our research efforts. Researchers who don't necessarily participate in bench research, but try to tie together the numerous findings and then place them in a palatable, cohesive context, could perhaps help the morale problem in biomedical science. Navigators who help us to plot our course and engineers who pave the road behind us are needed to maintain meaning and shape in our work. As a physiologist-in-training, it has been emphasized to me that this would certainly be one of the roles of the physiologist in the future. However, I haven't seen any 'help wanted' signs for the position yet. Who would fund this new entity? At what level should their placement be: government, institutional? The existence of such positions may even be scorned by the many hard-working bench scientists whose very definition of scientist implies the use of beakers and flasks. However, since the advent of molecular biology, complaints have been lodged about the lack of cohesiveness in the scientific realm. Logistical problems will no doubt complicate the establishment of these mediators, but as the papers flood in, it is obvious that mediation is becoming a very real need. To suggest that networking between labs and groups does not occur would be inaccurate, but the mechanism by which networking occurs is disorganized and chaotic. Competition between groups also adds to this problem, where certain knowledge is excluded.

It could also be argued that scientific mediation would not be a cure-all for the present morale problems. Strict adherence to the Carville hypothesis would suggest that mediation is a stupid idea since it will undoubtedly cost money, detracting from pure research. However, the concept is a potential treatment for what ails the field of biomedical research, and it is worth finding out whether its benefits outweigh its costs.

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Response to Mr. Crist

Mr. Crist makes two good points; I wish him well in his career. First, so long as the organized ideology of practicing scientists holds that every available penny for science ought to be allocated for peer-reviewed research, it will be impossible to set up any new structures that recognize and build upon the actual curve of a career path in science. We will not see our senior colleagues get paid to help our junior ones until we ourselves ask that the money be taken from basic research and tithed to this central task.

Second, so long as government agencies like the NIH and the NSF are willing to accept lip service to the greater good in the 'significance' part of a grant application as sufficient, however hackneyed and cynical it may be, there will be no reward to the applicant for helping another person except by sheerest happenstance. I see no third honest alternative to the two straight-talking paths open to practicing scientists today: either to make an honest attempt to link their work to human needs or to admit that the work is not so linked, and then to ask for the money anyway. **RP**

Is there life after the grant? Has life become the grant itself? Is science suffering because scientists are focusing on the wrong issues? Has science lost its way? Pollack's rather depressing view of the life sciences suggests that all of the above are, to some extent, true. From his viewpoint, science has gone sadly awry: morale is low, grants are very difficult to obtain, and scientists, instead of being mutually supportive, "have allowed the social and emotional foundations of their field to rot away beneath them."

He argues that this sorry state of affairs exists because of a tendency to define one's own identity with the grant, so that the loss of funding is a kind of death, even an execution since the decision not to renew a grant is made by other scientists. The world of ideas is the victim. To combat this "anomic indi-

vidualism" and establish a more balanced, intellectually healthy scientific environment, professors need to rededicate themselves to their responsibilities as mentors to their students and younger colleagues by creating a more interactive workplace.

Does this view constitute an accurate picture of the life sciences? Possibly in the nonclinical arenas it may. In my own experience, and particularly for those of us who also have responsibilities for patient care, the evidence of dysfunction seems to be less obvious. Certainly, the stress of attempting to do research while still finding time to fulfill one's allotment of patient care is tremendous. Grants are very hard to get, time-consuming to put together, and it is often tempting to do the experiment that will get the grant rather than the one that answers important questions. But for all that, I'm not convinced that morale is especially low, or at least any lower than it has been in the past. In fact, I'm not convinced that the past was so very different from the situation that exists today. In my own experience, there have probably always been those workers who hugged their ideas jealously to themselves and those who shared them freely; there have been mentors who were enormously encouraging and helpful, and some who seemed to have no interest in anything except promoting their own advancement. This last point has always been an issue with prospective graduate students and postdocs.

It may be that as technology has changed, those of us trained in the pre-molecular biology era have had to make enormous efforts to retrain in the newer techniques, which may in turn have produced a more collaborative approach, sometimes decried as 'big' science but often necessary nowadays to complete important projects. We certainly have to share resources and expertise with each other, and the loss of funding for any one frequently causes difficulties for the others. At the same time, patients serve as constant reminders that, for all the recent advances in science, incurable diseases are still very much with us and death is by no means banished. The fact that FASEB is expanding to include a scientific society such as the American Society for Bone and Mineral Research, which contains a large component of clinical investigators, is perhaps a sign of the recognition of the continued need for applied biomedical research.

However, I agree with Pollack's point that the system is capricious and that we do need to support each other, if only for the sake of stimulation. Ideas do not exist in a vacuum. I suspect that scientists have probably always complained, but better complaining than complacent.

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Response to Dr. Mundy

Dr. Mundy gives us a handy response to Mr. Crist. If you are a clinician and a good one, you ought not have a hard time explaining how it is that your work adds to 'the greater good', and so you ought not have the morale problems endemic to basic biomedical science. Fair enough, but what if one did not choose to become a physician, yet still wishes to help people live longer and healthier lives? Basic science does lie at the root of much of modern clinical practice, so why is it so hard for a basic scientist to experience any of the exhilaration that a physician must feel when—by use of that science—she saves a life? **RP**

I think that never before has *The FASEB Journal* attracted more diverse readers as the issue (August 1997) that included Dr. Robert Pollack's article. I do not know how many scientists heard him deliver the lecture, but of those who did, if most were not struck by this speech, I am sure we are already headed toward a professional catastrophe.

I have been in research since 1981 for my Ph.D. degree (India), following postdoctoral work in several laboratories in the U.S. Nationality does not make a difference in the gradual but steady disappearance of morale among scientists. It has been so rampant over the years that a calm witness urges one to predict that immoral scientific conduct will affect the future generations in such a devastating manner that everybody will fall a silent victim to it.

Volumes could be written to jolt society into an awareness of the fact that in its search for the truth, science has gone wayward itself. Scientists are more busy than ever building their careers, no matter what it takes. The Nobel prize, grant funding based on journal publications, job security based on funded grants, etc., have never emphasized individual scientific conduct. A socially conscious person would know how it feels not to care for the overall social values in the name of personal career development.

In the future, if current science faces a revolution, Dr. Pollack's name should be engraved in gold as one of the pioneers: being a scientist himself, he could stand up to pinpoint the faults in his tribe, whether that be an effect of the pressure of life itself or the personal moral regression that accompanies science as civilization proceeds.

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Professor Kanungo makes me blush, so I will not have much to add. I do not wish to see a revolution of such magnitude and disruption that my face might appear on a medal struck by the victors. Rather, I would prefer to be around to see a slow recognition by colleagues of all ages that it is in their own interest to change the system smoothly and without disruption so that their lives, too, might be easier and happier. Now, on the other hand, a postage stamp. . . . **RP**

Due to the intensive Ph.D. program launched by the Hungarian government 3 years ago, I recently had the chance to be one of the examiners in numerous doctoral exams. Talking to other Hungarian and foreign colleagues, I think that my alarming experience can be generalized: our recent doctors in the field of life sciences are quite outstanding experts in their particular specialized field and research techniques, but know less and less about the facts and logic of, say, biochemistry, or about the general context of their own results. The increasing demand for new and newer data—the ‘publication race’—distracts them more and more from reading the literature and especially from studying articles that are not of immediate relevance to their actual experiments. Most of the time even these papers are advised (and therefore screened) by their supervisors.

Scientific efficiency carries a price not only among graduate students. Overspecialization is more and more characteristic of senior scientists as well. Principal investigators find themselves as CEOs of a small or larger ‘data factory’ instead of being a leader of a true lab where novel ideas and approaches are born. An increasing number of research papers have discussion sections lacking a broader perspective of the field and of the findings. Many reviews restrict themselves to a mantra of known major facts or to a mere listing of available data in the field. Conferences became an unbearably long series of monotone monologues, allowing less and less time for real discussions and vivid exchange of different views. Life sciences are slowly imprisoned by an increasing redundancy and an overflow of minuscule details.

What can help us to stop these dangerous trends? We must reorder our values to emphasize outstanding achievements even more over the continuous flow of low-value publications. Groundbreaking contributions should more generally balance a few ‘non-publishing’ years. As members of grant-giving bodies, we should give more credit than before to high-level attempts to synthesize a particular field of science.

We have to make the ‘scientific background’ part of Ph.D. theses and grant applications an even more important criterion of acceptance. When organizing conferences, we should reverse the 20 minute talk + a 5 minute discussion general setup (where the latter is usually absorbed by the talk itself) to a 5 minute talk + a 20 minute discussion scheme. Meeting organizers should ‘plant’ known debaters of related fields in the audience, asking them for thought-provoking interdisciplinary comments or questions. Technical advances (such as the pagers applied successfully in the recent International Congress of Stress in Budapest) enable us to devise a more flexible meeting structure that allows the formation of spontaneous workshops/discussions.

Last, but not least: fully agreeing with Dr. Pollack, I also stress the importance of teaching. Teaching of science, the mandate of ‘profess’-ors, however, does not begin at the postgraduate or even the undergraduate level. On the basis of examples from the Howard Hughes Medical Institute, the New York Academy of Sciences, and other U.S. institutions, we began a few years ago a highly successful campaign to draw talented high school students to research labs in Hungarian universities and research institutes. Introducing the ‘wholeness’ of science at an age when potential future investigators are not yet spoiled by the general trends of overspecialization may help us to preserve (or regain) the integrity of life sciences in the 21st century.

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Response to Secretary General Csermely

Secretary General Csermely’s response pleases me greatly. I have nothing but the greatest admiration for someone who has come through real revolution—without medals—and who seems so clear-minded about the value of personal integrity and the observing of behavioral boundaries in science. Apparently Hungary, like Spain, has scientists whose experience of a repressive regime has given them a more realistic perspective than their American colleagues, whose memories of strife and revolution are likely to center around pot busts and teach-ins. What is really unexpected, though, is how little it seems to have mattered that Spain’s regime was Fascist and Hungary’s Communist: real personal autonomy means more once you’ve felt its absence in your bones. **RP**