EDITOR'S COMMENT

"Wait a minute. I think I see the problem."

Picture the scene. A modern version of the French Revolution. Two people are condemned to death. One, an ordinary citizen; the second, a typical NIH Peer Review panelist. The ordinary citizen is led to the guillotine first. He demands the right to confront death directly and is allowed to position himself faceup. The signal is given and the blade begins its descent, only to stop halfway down. Custom demands that he be released unharmed.

The Peer Review panelist mounts the platform and, noting the outcome of the first attempt, also demands to be positioned faceup. But just as the executioner is about to release the blade, the Peer Reviewer shouts out: "Wait a minute. I think I see the problem."

Bob Pollack, writing in this month's *Life Sciences Forum*, pp. 725–731, wonders why so many people practicing basic research in modern biology seem so unhappy, frightened, and even angry about the future. Morale is at a low level, he argues, but not for the reasons people usually cite. Low morale is not a matter of too little money, as many might claim, nor of too few new ideas, as critical reviewers might suggest. Pollack believes that the root causes of this malaise are matters of the spirit and psyche. Scientists have failed to appreciate the need for humane ways to evaluate the worth of each other's efforts. Weighing books and counting publications in prestigious journals are hurdles too high to surmount over an entire career. And we are too tough on each other, he asserts. We have created our own Hobbesian world.

Vincent T. Marchesi
Editor-in-Chief

Readers are invited to comment on or question Pollack's essay, which appears in this month's Life Sciences Forum, and he agrees to offer replies, all of which will be published in future editions of this Journal.

Hard Days on the Endless Frontier[1]

ROBERT POLLACK[2]
Department of Biological Sciences, Columbia University, New York New York 10027, USA

"Biology is in a sense broken. I come from a gentler time when the joy of puzzle-solving was enough reward, when it took much time and little money to accomplish a partial solution and move on to the next step. Material rewards, the intellectual climate, competition for grant funds and self esteem were not huge problems. If one manifestation of the current climate is the stifling of academic freedom, another is the demoralization of graduate students and even some undergraduates as they watch their struggling professors. We have few answers, and some of us no longer feel comfortable in telling them simply to follow their hearts." —Karen Artzt, Professor of Zoology, University of Texas, Austin, *The New York Times*, July 9, 1996.

Professor Artzt's letter to the *Times* was part of an ongoing passionate, even hyperbolic, exchange by politicians and scientists, on the behavior of Congress, the NIH and the scientists themselves, initiated by the question of reproducibility of data that has come to be known as the Baltimore Affair. In passing, she also described—with precision and accuracy—the current situation in basic biomedical research, the field that has been at once most productive and most important to the greater society in recent decades.

Why have the neuroscientists, and the molecular biologists, geneticists, and developmental biologists whose discoveries have forever changed the way we understand ourselves, become so uneasy, so unsure about their own futures? Scientists usually call out either one or another of two incommensurate hypotheses to explain the unease discussed by Professor Artzt. These models of what is causing poor morale are both plausible, but neither is convincing.

The first is the developmental model. The life sciences, like other sciences, have followed Kuhns' well-described path from accumulation of data that do not fit a current hypothesis, to a great new insight that places troublesome data in a smooth context consistent with all previous data, to consolidation of a new hypothesis as the current, comprehensive version of scientific reality. The current, aged paradigm—in this case, the notion that information contained in the DNA of living species can be mined for all that there is to know about life—has reached an inevitable moment of evolutionary divergence.

Life-scientists do not yet know what new model will supplement this one. This makes them uneasy—demoralized—because they are betwixt and between, neither working in the moments immediately after a great insight when everything becomes clear, nor participating in the subsequent, inevitable victories chalked up by the panzer divisions that roll across adjacent fields, re-interpreting old, established worlds of science.

They are stuck in time instead, waiting for the invasion by the next scientific generation, conquerors softened by conquest hearing the motors of the guerrillas in the hills. According to this argument, the current demoralization in molecular biology is simply a stage in the development of this science. It is natural, and there is nothing one can or should do about it; the next paradigm shift will fix everything.

The second model is the non-historical, non-developmental, Carville hypothesis: it's the scientific economy, stupid. Money is bad, that's all there is to it. When Federal money is tight, grants are harder to get, peer-review is harder to carry out with fairness, training is harder to justify and pay for, and medical science is, for these reasons and no others, less fun than it was when the money was better. Want to make

[1] Presented at the annual meeting of the Association of Directors of Neuroscience Departments and Programs, which met April 20, 1997, at Georgetown University, Washington, D.C. An earlier version of this paper was presented at the George Washington University symposium "Science in Crisis at the Millennium," held September 19, 1996 (Washington, D.C.), and will appear as "A Crisis in Scientific Morale," in *Annals of the New York Academy of Sciences*, in 1998.

[2] Dr. Pollack is the author of *Signs of Life: the Language and Meaning of DNA* (Houghton Mifflin, 1994) and *It's About Time: Science and the Future of Medicine*, a forthcoming book, also from Houghton Mifflin, on the future of biomedical research.

0892-6638/97/0011-0725/$02.25 © FASEB

biomedicine more fun? According to this model, the way is easy, and obvious: put in more money.

I find neither the economic nor the paradigmatic hypothesis adequate to explain the demoralization of molecular biology. Both models serve scientists themselves well enough as spurs to one or another action, but neither can explain why it is now, when so much is happening in molecular medicine, and when so many new opportunities for doing molecular biology are opening up, that so many people practicing basic research in these fields seem so unhappy, frightened, and even angry about the future.

The number of new NIH grants—the best kind of peer-reviewed, investigator-instigated, free-inquiry-supporting money—have gone up, not down, for the past few years; the amount per grant has gone up at a rate well above the rate of inflation, although to be fair, much of that increase gets eaten by rising indirect overhead costs. In the past decade or so, the Howard Hughes Medical Institute and the private, for-profit world have both poured billions of additional dollars into just the fields that we and Professor Artzt are in; and the current administration of the NIH is headed by a model—a paradigm if you will—of success in basic science, the Nobel Laureate molecular biologist of cancer genes, Harold Varmus.

I have another model to explain low scientific morale, but this is a tricky and complicated matter for me to discuss, because about five years ago I gave up my laboratory, one that had been funded without interruption by the National Institutes of Health and other government agencies for about a quarter of a century. Few scientists decide to close their labs as an act of free will, while it is still their choice, and not simply as an early escape from impending low priority scores.

Grants were harder for me to get in the last years of my lab, it's true, but it was the time it took to get them—time that had to be cut from the time that should have been spent doing the work itself—that led me to give up my lab. There was precious little point—and no fun at all for me—in spending time pawing through the results of expensive, careful experiments just to find bits of bait to put on the hook of another grant application. Nor was success at this any part of my idea of free science: experiments done to get a grant, and grants gotten to pay for that sort of work, were at once both a whirlpool and a rock.

Seeing no way through, I decided to stop playing Ulysses, tied up my boats, and walked back home to a life in science that made sense to me. Though anyone with a funded lab might reasonably object to me continuing to call myself a scientist once I had given my lab to my department to be distributed among my colleagues, I would argue that I am indeed a scientist today, and that as a scientist, my morale has never been higher than since I stopped asking for funds to keep my lab going. I continue to write about the medical sciences, to teach the subject to undergraduates and graduate students, to review manuscripts and grants. In place of directing the work of a lab of my own, I have begun to consult in the private sector, helping the research program of a pharmaceutical corporation.

My own experience is a data-point that argues against both the economic and the paradigm models of morale. It is, however, fully consistent with a third hypothesis, one that has the added value of being consistent with the current and the root meanings of the word morale. The dictionary says that the noun "morale" means "the state of the spirits of a person or group as exhibited by confidence, cheerfulness, discipline, and willingness to perform assigned tasks (my emphasis)." All the ways of exhibiting high morale require a social structure in which someone gives you a task, and then you perform it; if you do so with "confidence, cheerfulness, and discipline," you have shown your high morale. The word "morale" comes from the feminine of "moral," the French word for morality or good conduct. Looking backward in time, the common root of "morale" and "moral" is the Latin "mos": "manner, custom."

My hypothesis is that morale in the biomedical sciences is a measure of something social, something that cannot be contracted to any individual frame of mind or behavior at all. According to this model, morale is not a matter of individual funding, nor of individual success, nor for that matter of anything else individual. Low morale is not a matter of too little money, nor of too few new ideas, but of too little kindness and decency; it is a failure of custom and manners, a loss of social purpose, a diminution in the ability or the will to distinguish right from wrong and then to act rightly. At the root, low morale is just a consequence, the cause of which is the fact that medical scientists busy in their labs have allowed the social and emotional foundations of their field to rot away beneath them.

So long as individual scientists believe, and behave according to the belief, that the essence of success in science is the freedom to discover the right task—experiment—and then to do it according to one's own lights, 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. Such a world is intrinsically unhappy, and profoundly unbiological as well, in the sense that no scientist's life, or work, can possibly go on indefinitely, as this sort of world demands.

There is nothing intrinsic to science that prevents a colleagues sharing a field from being sympathetic, and amenable to friendship, empathy and mutual support. Whether or not friendliness exists in a given science may be a matter of the developmental stage the science is in; morale was certainly highest in molecular biology when it was a new field. In that sense, the morale problem in science would not be a problem in paradigm shifting, but one in developmental biology.

My own career is long enough to have given me the experience of this early stage, when collegiality has not yet not chilled into calculation, and it is possible to know someone in one's field as a friend as well as an ally. Cold Spring Harbor Laboratory was still infused by the communal, borderline-anarchic spirit of the Delbrück-Luria-Hershey phage group when I arrived there in 1969. I gave my job seminar to an audience of twelve that held three Nobel Laureates, an audience that kidded me and each other and ate their lunches all the while, as I gamely ran through my slides and tried to be serious. It was only decades later, after too many seminars that cried out for even a single breath of levity, that I realized I had been given a welcome of a very precious sort, an acceptance that was not a judgment.

The society of today's biomedical science has an altogether different, coarser feel to it. Scientists interact with each other only as they must to get their own work done: the social structures of science deal with the training of new scientists and the funding of operational ones, but not with the morale of either. It is a paradox that life-scientists, in particular, with their exquisite sensitivity to the staging of developmental processes at the molecular level, should be so completely blind to the developmental stages of their own lives and careers.

This willful miscalculation of the trajectory of a life can, paradoxically, lead to sudden demoralization in just those scientists who have been around long enough to know better. As they reach an age and situation where "peer review" means being judged by colleagues younger than their children, the absence of social structures that would validate anything about them beyond their latest papers, makes for a reproducibly sad moment of isolation that often leads to bitterness and one or another kind of weird, obstructive behavior.

Because the middle slice of a scientific career is the funded and therefore the only part to remain meaningful in the absence of structures of kindness and decency, the social structures of today's science often slight those at the vanguard, as well as those in the wake of the funded middle. Why insist that your graduate students know the larger context of your chosen field, that they learn about parts of the field that you are not interested in, that they learn how to teach, when you need them in the lab immediately, and you yourself find teaching a distraction from your work?

Why help a post-doc to get started as an independent scientist, if you are going to be competing with that person a few months after they leave your lab? Why share data or materiel—why let someone clone by phone, as the saying goes—if your stuff has the chance of being patented; why admit fears if the person you are talking to may have to decide an aspect of your fate? Why, in other words, care about anyone else, or expect to be cared about in turn?

As a result of this diminished and cynical view of human relations, the education of many young scientists is deeply defective. Graduate and post-doctoral programs are a bit like high-school and college basketball programs, with a tenured job at a major research university being the equivalent of an NBA offer. The low to vanishing probability of either the professorship or the starting position are known to college coaches and directors of training grants alike, and both manage to avoid telling the quantitative truth to the young people from whose work they and their senior colleagues maintain their productivity and reputation.

It is exceedingly rare to find an example of a mid-career, tenured University scientist who gives any students—undergraduate, graduate, post-doc—a reasonable estimate of the tiny chance they have to become a tenured university scientist in turn. It is not the student, but the mentor, who reveals his or her demoralized state by assiduously avoiding the topic. The mid-career effects of early cynicism are obvious: scientists who get the grants, and with them the positions of authority and tenure, have not been trained in how to cooperate. They are trained to be first at all costs; one of those costs is the absence of feelings of compassion toward their peers.

Each scientist trained in laboratory-isolation to beat out all others who succeeds, never learns what it is to share the moment of discovery with a trusted but independent colleague in another lab, a friend who knows the field well enough to properly savor the moment. The faulty social structure of our field casts a large shadow, and in that shadow mid-career scientists who might shed some light on the problems we most want dealt with, are discouraged—to put it mildly—from expressing any sentiments that might be construed as holding any aspect of life intrinsically and ineffably valuable or, shall we dare to say it, sacred.

By the time they reach mid-career, funded scientists have too often already been flattened by these early habits. Trained to suppress compassion and to denigrate both irony and wonder, they become—not by design, but by default—people whose morale cannot be anything else but a linear output of funding input. When such a person's lab funding stops for any reason, morale has no back-stop. All credit that might have been accumulated by way of intellectual rigor and broad knowledge base is so diminished in the absence of any context that might provide some ironic detachment, that it is completely without force, and cannot help relieve the pain of loss.

When they do express compassion, colleagues and departments are likely to do so only through money, infusing enough into the stricken laboratory to keep it alive for one or two more grant-review cycles. In that time all the other aspects of intellectually sound socialization that might be mobilized to give the suffering scientist some sense of place and importance are turn taken away: studying is suspended, teaching is suspended, advising is suspended; everything about being a scientist that is not the getting of a grant, is revealed to be a distraction; the job is to get the grant.

The resulting emotional isolation is of course the real agent of demoralization for those who think of themselves as scientists from within. The message is plain: what you think of yourself has no merits; only your peers can tell you if you are a scientist or not, and they will never do so to your face, only through the confessional screen of peer-review. Nor is the loss of a grant the only pitfall here; the willingness of a funded scientist to measure her validity as a scientist through the level of her grant volume is a foolish and dangerous act of demoralization, no matter what the level of funding is.

Malamud's short story about such a case—the man whose pulse was synchronized to his stock's price, and who died of a heart attack when the stock split—is no mere fable. Of the five colleagues in my small department who have died in the years since I have been at Columbia, I am certain that at least two went before their time under the stresses imposed by this inappropriately quantitative self-definition. They died with their boots on, so to speak; for the rest of us, it is worth asking, why is biomedical science in particular so insensitive?

In its disciplined way of looking at the natural world, any branch of science requires its practitioners to act as if they were observers, not participants. In all sciences, the first and last scientific instrument, the one that must be used in every experiment, is the scientist's brain. Scientists who choose the human body and mind as their playing-field cannot fully meet the requirement that they observe their systems dispassionately, without dislodging themselves from their bodies and minds.

The strain of trying to meet a standard of cool curiosity without flying apart into pieces imposes an unbearable distance between the scientist of the body and mind, and the body and mind of the scientist. To relieve this strain, medical scientists have created the myth that their instruments and procedures somehow free them from the boundaries of their minds and bodies. This is the myth of absolute rational control of the scientist over her material, the notion that the metaphor of scientist as sculptor will not break down even when the sculptor and the sculpture are the one and the same.

The conscious expression of this unconscious dilemma in the medical sciences is a novel transformation of every scientist's dream of winning. Medical science, like any science, is profoundly respectful of the scorecard, allocating recognition only for precedence in demonstrating the correctness of a new model for how a piece of nature works. The dream of winning takes on an obsessive quality in the medical sciences once the subject of scientific study becomes the mind and body, and the reality of bodily mortality becomes unavoidable. The obsessive response to the certainty of biological death is the promise that a big enough win in the game of science will beat death itself, by conferring a form of immortality on the winner.

Nothing but the thinnest membrane of denial separates the notion of scientific immortality through priority of discovery from this deeper, older, and wholly non-scientific dream of escaping one's own inevitable death. Personal mortality puts life-scientists in a bind every time denial fails: to play the game well, they must never stop asking questions about our mortal bodies, but to play it with their own lives, is to be sure that the answers—their own answers, their own models—are sometimes going to be really frightening.

Discoveries that set the agenda for the future work of a large group of other scientists do this after a fashion, permanently associating a lower-case version of a scientist's name with an aspect of nature: think of darwinian evolution or the watson-crick model of DNA. But in the medical sciences, belief in winning immortality of any sort is problematic, since it denies the biological reality only too plain from the data, that the eventual loss of self is inevitable.

I first saw how the life sciences could be driven by a fantasy of institutional scientific immortality quite early in my career, about a quarter-century ago, in a formative, odd conversation with Al Hershey, the Nobel laureate geneticist who had used a food blender and some radioactive bacteria in the late 1940s to show that the DNA of a virus—and not its protein coat—was its genetic material.

Hershey revealed to me his vision of heaven: you come into the lab every day forever, you do the same experiment you did the day before but with one small variation, and the results are just as exciting, important and new as they were the first time. Francois Jacob also knew about Hershey's heaven, describing it in his autobiography as an addictive, manic state: ". . . I found myself at ease in this world of dream and of doubt. I could quietly surrender to my manias. Cultivate the idée fixe. Wallow in it at leisure. Prepare every day my dose of stimulant for the next. Start over again without ever getting winded. The endless desperate pursuit of a goal that retreated as one approached it. Anxiety, the need for living in the future eventually served as a motor. In this world, even my defects found a function."

This notion of a model so interesting that testing it over and over again wins the game without any further creative thought, is the root fantasy of the biomedical sciences. Hershey's Heaven cannot exist for students of life because they must deal with the facts of life, and the first of these is the inevitability of death, the personal mortality of the experimenter. Hershey's myth serves the same function for those biologists who believe in it, as other myths about death serve in other religions: to keep their believers from having to confront this irrational, unbearable reality.

Science as a world-view began with the Greeks, and so did science as a religion. Asklepios, the demi-god of medicine, was the son of the immortal god Apollo and a human princess named Coronis. The centaur Chiron—a physician of consummate medical skill who also happened to be an early human-horse recombinant hybrid—taught Asklepios the arts of medicine, and he became so skilled at healing that he was able to resurrect the dead. Hades, the immortal god who ruled the underworld, complained to Zeus the father of the gods that he feared the loss of future subjects if humans were no longer to die. Zeus's response—he killed Asklepios with a thunderbolt—explains our present-day mortality, and leaves us with the fantasy that by rediscovery of the skills of Chiron and Asklepios, we may yet one day escape death.[3]

Only faith or obsession—if they are not the same—can expect a method of observation of nature and the knowledge it yields to set a person apart from the passage of time with its inevitable instant of personal ending. The underlying fantasy, that omnipotence of thought will bring immortality, is a notion inadmissible to rational analysis. This is why the conscious, operational, agenda of the life sciences has to mask its fantasy in the cloak of lower-case, institutional immortality. But institutional immortality itself, born from the unconscious will that one's name not be scattered, is just an older version of the same fantasy, the ancient impulse which the critic Harold Bloom has called the will of the strong poet, and which Freud himself recognized it as the one "higher superstition" he himself believed in: "My own superstition has its roots in suppressed ambition (immortality) and in my case takes the place of anxiety about death which springs from the normal uncertainty of life."

Though today's biomedical science claims its intention is to cure us of nature's defects, too many of its practitioners behave as if their real purpose were to understand nature only to gain from first knowledge the mythical immortality of precedence, at whatever cost to themselves or anyone else. Denial of the fear of nature's terrible power of mortality; projection of the suppressed wish to not to be subject to nature, into a vision of nature as capable of bestowing immortality: these are the marks of a masked unconscious operating to create a biomedical science at war with its own stated purposes.

Visions like Hershey's Heaven can have unexpected power over scientists and also over the rest of us, especially when they are believed by the people who set the priorities of basic biomedical research. For example, many excellent scientists—their eyes fixed on their place in Hershey's Heaven—have become completely averse to the idea that their work should be directed toward any need beyond their own need to know the answer to their next experiment. Filled with this conviction, and thereby sustained in their faith that they have avoided the fact of mortality, they are easy prey to the habit of making promises to themselves and to the rest of us that they cannot keep, promises that hint that death itself may be put off indefinitely.

Today, as a consequence, about half of all medical bills are paid in the last year of a person's life, while funds for preventing childhood disease are in ever shorter supply. All of us have already paid a considerable price in lost opportunities and bad judgment, because when the leaders of our biomedical research programs, and the leaders we elect above them, have not been able to deal honestly with the facts of mortality in their decision-making. The result is widespread cynicism and disappointment, as medicine and science's inability to deliver on the promise of good health forever becomes clear.

It is not that science and medicine wish to avoid finding cures, it is that they are too strongly motivated by an irrational, unconscious need to cure death, to be fully motivated by the lesser task of preventing and curing disease simply to delay for a while the inevitable end of their patient's lives and, by extension, their own inevitable end. The irrational, obsessive promises made by scientists and physicians in the past few decades have amounted to an institutionalization of the denial of death. These promises are not sustainable, and as that fact became evident at the level of national policy, it contributed to today's state of nervous collapse about health care.

How can biomedical science be returned to its place as a free-standing profession pledged to understand, but not always mortgaged to change, our bodies? We cannot hope to go back to a state of ignorance in order to escape the uncanny sense of knowing something we don't really want to know. All we can do is understand ourselves better, using the information of the life sciences, but not stopping there, applying the lessons of the rest of our lives to the task of placing the knowledge of science in a context that will protect us all—scientist and citizen

---

[3] The rest of the myth as it has come down from the Greeks tells us that Apollo, the immortal god of song and light, took such offense at this act of Zeus that he slew the Cyclopes, the makers of Zeus's thunderbolts, in revenge. Revenge came too late to help poor mortals then or now, but it raises an interesting question: why did Apollo care so much that humans could no longer become immortal? The Romans, always happy to add a god to their Pantheon when it might make things better for them, elevated Asklepios on the instructions of an oracle during a plague in 293 BCE.

alike—from the most damaging consequences of this unexpected knowledge.

No biomedical scientist—nor any scientist, no matter how rich or creative—can look at all the data that all possible experiments might generate. Choices are necessary, and it is at the moment when choices are made, that the scientific method departs from the wholly conscious tool of scientific experimentation, and enters the human world in which all choices are made in a personal and social historical context, replete with emotional affects and barely remembered feelings.

A humane version of the biomedical sciences would, for instance, acknowledge that it is not about to transcend the limits of the human body, and so it would no longer make promises it cannot keep. Beyond that, the science that can acknowledge the presence of an unconscious component in its operations if not in its methodology, will be the science that can best serve its practitioners as human beings, and the one most likely to generate an agenda of lasting value for the rest of us. Such a change in the agenda cannot happen unless scientists themselves first take the time to consider the consequences of their own behavior toward each other. They might begin by recalling the two-thousand-year-old advice of Hillel the Rabbi—"What is hateful to you do not do unto others."—and asking themselves whether any data is so good, as to allow this notion to be set aside.

I would like to close with some personal reflections. For those who do survive to get there, the third stage of a scientific career—life after the grant—can be fun all over again, although to get there in one piece they will have to resolve to break out of the press that flattens so many funded scientists into spreadsheets and priority scores, by somehow defining themselves as scientists in ways that no longer depend on peer-reviewed funding. The mix that has worked for me involves writing, teaching, consulting and advising.

My first book on science for the general public, "Signs of Life," came out in 1994. It takes as its argument the notion that DNA is not merely an informational molecule, but also a form of text, and that therefor it is best understood by analytical ways of thinking commonly applied to other forms of text, like books. I am now completing my second book, this one on the difference between the outward, stable, inexorable time of science, and the inward, multiple, flexible time of consciousness, and the consequent problems science has making sense of consciousness, and of that nasty diamond at its center, each conscious person's knowledge of inevitable, personal mortality. These books have both been exercises in seeing patterns from insufficient data, and in that sense they are much like the science I used to do; the main differences are that I now write for the public rather than for a star panel of secret reviewers, and that I get paid by a publisher who pays taxes, rather than by a government agency spending tax money.

The cynicism of funded scientists toward undergraduate teaching is so pervasive, that my decision to make such teaching an aspect of my life as a scientist, was perhaps the most radical of the many I made in order to stay a scientist. When the undergraduate classroom becomes a debtor's prison for place for science faculty without grants, any serious university undergraduate quickly learns that a certain fraction of her professors will look the part, but be hollowed out, bitter, and often sadly uninformed about recent developments in a field. Any graduate student in a good science department has no such problem, because such faculty are rigorously kept from contact with graduate students, unless the graduate student is a teaching assistant; in that case the contact is kept as perfunctory as possible.

As one such non-funded professor, I am asked to teach three courses a year: an introductory course in evolutionary and environmental biology, a graduate course in the molecular biology of disease, and an entry-level course in science for non-scientists. This is a bit more than twice the load of any of my funded colleagues. By funded, of course, I mean funded for lab work; grants received in the past few years from the Guggenheim Foundation, the Ford Foundation, the Sloan Foundation and the Howard Hughes Medical Institute did not count in a way that would have, for instance, given me a lab-funded colleague's teaching load, to let me write more intensively. I have used the obligation to teach, as a chance to learn; I am, in that sense, my own student. This notion seems to have resonated with the Ford Foundation, which recently awarded me a grant that enables me to bring together a group of colleagues from medicine, law, public health, the humanities and the arts, in a faculty seminar on human diversity.

Beginning with the Reagan Administration's decision to encourage NIH-supported faculty researchers and their universities to patent and market their discoveries, the distinctions between private and university research programs have lessened, until today R&D scientists in bio-tech companies publish their work in the best journals, and it is no longer clear whether the speaker at a meeting who cannot share reagents or sequences because of patent restrictions, is from a company or a university.

Setting aside the question of whether this blurring of boundaries between the non-profit and the for-profit sectors is good or bad for science, it has certainly been good for the morale of scientists from both sides of the fence who have been able to take advantage of new opportunities. This is especially true for scientists who wish, as I did, to move on from the business of competing for Federal grants: for the past few years I have been a Director and the chair of the Scientific Advisory Board of Applied Microbiology, Inc., a small bio-technology company developing new classes of antibiotics and medically useful dietary supplements.

Though advising, the last of my four laboratory-surrogates, is the least structured aspect of my professional life today, it is the part that most clearly reconstitutes the humane context I so sorely missed as a lab scientist. The iron rule of career development in this country is that young people must constantly make fateful choices about their futures without enough experience upon which to base these choices. A mixture of circumstance and history has put me in a position to lend an ear and give advice to dozens of young men and women each year as they move through college, trying to choose among various options including basic research, medicine, public service, and the private sector. My advice carries a certain weight, attached as it is to the considerable number of letters of recommendation I write to medical, law and graduate schools each year.

Having assembled this life for myself in the absence of any structures within the scientific community, I firmly believe that the crisis in morale among today's scientists—in my field, at least—stems not from money problems, nor from the stage of development the field is in, but from a failure by the scientists concerned to form themselves into proper, humane communities. It is never to late to begin the task of forming these communities. In all three stages of a scientific career, it should be possible to introduce social structures that ameliorate the morale-puncturing competitiveness and anomic individualism of today's basic science, without in any way diminishing the intellectual rigor of the science itself.

No matter how porous the boundaries get between university and private science, universities are likely to remain the main if not the sole source of the next generations of scientists for a long time to come. For that reason alone, any morale-building changes in the social structure of a science, will need to take place among university professors in their departments of sciences, or else they will not have any lasting effect at all. We might as well begin our reforms in the most conservative way, by rediscovering and rededicating ourselves to the meaning of the title we hold. "To profess" has a spectrum of meanings: to affirm openly; declare or claim; to make a pretense of; pretend; to claim skill in or knowledge of; to affirm belief in; to receive into a religious order or congregation; to make an open affirmation; or to take the vows of a religious order or congregation.

Rather than choose among all these current meanings, we might begin by returning to the root word from which they all evolved, the Middle English "professen," to take vows. Professors do not deserve the title, unless they are willing to take the time and make the effort to openly affirm something beyond their data, since data speak for themselves and need no affirmation. Affirmations and vows are not data-dependent; they are matters of the heart. To be a professor, it seems to me, one must first have something of importance to oneself that needs affirming, and then one must affirm it. Morale among professors in science will remain low, until they decide that their strongest feelings, as well as their best data, should determine professional behavior, and professional status.