This took 50 mins. will shorten

564 lines
4357 wds P/272.4 shortened 4/12/89 for delivery

Tonight, is my concluding ~~In this~~ lecture on the scientific career, I will take up three themes, loosely connected with one another:

a) The personal motivations for the pursuit of science and some of the contradictions and stress that then arise.

b) The life cycle of scientific discovery, and its points of vulnerability

c) The institutional setting of the academic scientific career, with special note of the overwhelming federal role through the granting agencies.

a) Motivations for science as a career

I will begin my remarks about the scientific career with some introspections about my own initiation. Newer generations may march to a different drummer; and even my own perspectives are perhaps less naive or conscientious today than they were during my own formative stages 50 years ago.

Modern science was founded as a response to the mysteries of every day life -- the motions of the stars, the forces of gravity and of magnetism, the continuity and evolution of life, the composition of familiar matter. It promptly dispelled the remaining relics of animism, and did a great deal to shatter faith in revealed religions, insofar as these misguidedly justified themselves by assertions of a scientific nature. From the time of Galileo, science could be regarded as a liberating or counter-religion, wiping away many naive superstitions. Faced with the task of clearing the residual rubble through the nineteenth century, materialistic science may have preempted the task of religious reconstruction.

My own education, starting in grade school in the early 1930's ~~thirties~~, was deeply colored by this function of science as a general world-outlook; but the reality was already one of hopeless fragmentation into innumerable specialities in very poor communication with one another. By then, the personal apprehension of nature might be achieved as well by scholarship as by experiment, by learning more of the corpus of existing knowledge than chipping a few new facts out of the unknown. Most scientific reports today, apart from an opaque jargon, comprise detailed minutiae rather than broad illuminations.

The contemporary work of scientific investigation ~~science~~ is then hard to justify in terms of an individual person's ~~man's~~ "need to know." He will never learn what is already known. Yet the body of scientific knowledge would be a sterile scholasticism if it were not constantly challenged and restructured. It still contains many inconsistencies, and merely to resolve them, as discovered, would already require constant resort to new tests. No two persons can learn quite the same material; except for rote parroting, learning is already thinking and questioning and speculating, and, without the criterion of experimental verification, accumulated learning would again revert to a medieval dry rot. It is fortunate then that the thrill of discovery, as much as of learning, motivates the researcher. We cannot ignore, as well, the motives of competition for prestige and for material rewards that help label scientists as part of the human breed.

Contemporary science, in its fragmentation, has become ever more remote from the primitive questions about nature that were its original invigoration. The effective practice of a particular science requires an unrelenting narrowness of focus, and rarely is there time for a broader education and for philosophical and social wisdom commensurate with the overall impact that science has on the human condition. Specialized talent is too precious to warrant being disturbed, but everywhere the need is also seen for another kind of scholar, the contemporary humanist, who can understand science in its original terms, without being engulfed by the detail of one specialty; the one who, to use a now banal phrase, can also bridge the two cultures. The social need for this kind of intercultural moderator is not matched by any evident niches in the prestige and career structure of the academy, perhaps because there is no easy way to measure the quality of his performance, to select the good from the trash, as we pretend to do in the established studies. We must then rely on senescent ex-specialists, knowing that age is at peril confused with wisdom.

What might systematic inquiry tell us today about motivations for entering science? Certainly it is no longer attended with quite the same image of sacrifice of marriage and family that once prevailed -- not that the hardships are much less, but our postdocs elect to marry and have children regardless. Our entire culture takes for granted a level of material success as a life aspiration that is certainly reflected in the flight of bright students to law and business. Within science, I see many youngsters still imbued with a passion to know; but I worry whether our institutional arrangements are not going to discourage that motive in favor of technical prowess, of a nose for the sure thing. This process is a paradoxical consequence of large scale social support for science, or rather how it has been bureaucratized in the last 15 years. As a university president, I regard my first task as the restoration of environments that can best locate and nurture creative talent, and thereby achieve the best social benefit from the allocation of scarce resources.

I would classify the initial motivations of the scientific career as including:

SLIDE

MOTIVATIONS

Curiosity - an appetite for knowledge and understanding "how things work"
Virtuosity - the prestige and self-satisfaction from the practise of extraordinary skill; intellectual agility; perserverance; manual dexterity
Power, vanity and influence - the fruits of "success" social reinforcements; admiration of others
Illumination - compulsion approaching the religious associated with peak discovery, or with esthetic contemplation of a natural order
Service - in reaching other minds and in generating technical fruits

The social justification for supporting science has little to do with private motivations, except where these might interfere with the public expectation of integrity and efficiency in the practise of science. Subject to the norms of the profession, the search for truth is melded with, and only rarely in contradiction to a system of reward of personal ambition. But where scholars demand privileges that go beyond the raw economic models of regulated greed, we have a special burden to sustain the operation of those norms and explain them to the public.

In assessing the performance of young scientists, I marvel at their ability to sustain many contradictory norms. Each one, in proper time and measure, is expected to exhibit each of the following:

CONTRADICTIONS SLIDE

imagination	vs. critical rigor;
iconoclasm	vs. respect for established truth
arrogant audacity to nature	vs. humility and generosity to colleagues
efficient specialization	vs. broad interest
doing experiments	vs. reflection, reading, speculation
ambition	vs. sharing of ideas and tools
celerity (priority)	vs. deliberateness (reliability)

In my own view, the most creative scientist must be a tempered schizoid, childlike in wonder and fantasy, fierce in relentless criticism, above all of one's own ideas. Our educational systems work hard to stamp out both of those attributes.

I could add the conflict of roles, going beyond the internal system of science to those of publicist, prophet, or vizier. We recall the Ingelfinger rule, that prior release to the press preempts the acceptance of papers in the scientific journal, and some of the silent finger-pointing at scientists who seek public visibility.

My own generalizations about scientific process are those of a participant observer, not a professional philosopher or historian. A more rigorous formulation of scientific thinking, in the discovery process, would help us to more explicit rationality in the conduct of science, so as to enlist the full support of computer technology, leaving to the human intellect the ultimate creative artistry. But professional philosophers of science have generally remained isolated from contemporary laboratory workers: the scientific content is daunting; and the method of science studies is so different from that of laboratory investigation. I have had great benefit from an extended collaboration with Robert K. Merton and Harriet Zuckerman.

b) Life Cycle of Discovery

All of the motivational impulses catalogued on my slide must be guided by socializing displine in the education of the scientist, a process that has received little systematic study beyond Zuckerman's study of Masters and Apprentices among the Nobel Elite. Her description, and anecdotal experience shows how the lab head and peer set collaborate to inculcate a set of values and a taste for what is both important and achievable which are indispensable for further success. These days the sheer technology of experimental work occupies a large part of the training period, which is now extended to as much as a decade of graduate, then postdoctoral study before the fledgling starts an independent lab.

It is not surprising that the primary motivations are sometimes in mutual conflict! In my own case, curiosity frequently threatens to do me in; an important part of my own socialization was to suppress a good deal of it, in order to focus my energies on the immediate research challenge. Social responsibility has been a diversion (or fulfillment) for others. I do not mention all of the rest of life that may compete with monitoring a 24-hour experiment.

All this is compressed into a single node in the following diagram: (Pilgrim's Process round the Circle) which might have been labelled "The RO1 set", to borrow the NIH's index number for single investigator projects. Besides the prior cycles of scientific effort and exposure during the apprentice role -- an exposure that is mitigated by the sheltered by the shelter of the the experienced mentor,

The reality can be complicated by different patterns of

a) division of labor

b) Separation of data gathering and interpretation

c) career distractions -- many more tangents can be drawn at each node.

CHART ✓ SLIDE transparency? grounds.

Contradicting idealized models of linear progression, from concept to experiment to public acclaim, the nodes rarely progress in sequential order; reverse loops tend to outnumber the incremental steps in response to new insights, data, opportunities and constraints.

Pre- and Post-mature discovery.

The contemplation of practical scientific method, concretized in this chart, calls our attention to numerous targets for non-cognitive, social influences on the course of science. In a review of my own work on genetic recombination in bacteria, Zuckerman and I posited how discovery might sometimes be "postmature". We were not invoking a pre-ordained rhythm of discovery. However, if a discovery can be "resisted" or "premature", it follows that it might also be "deterred", the resistance or obliviousness being internalized at any part of the cycle summarized in the chart. A "postmature" discovery is then one seen as deterred by a fault in some one step in the cycle prior to publication, most of the other ingredients being seemingly in place. And one discovery resisted deprives the intellectual milieu of precursors for consequential discoveries that may thus have been deterred. We do not discount that the fault may be simply in the creative faculty -- that we should rather marvel that some finally did surmount the obstacles to imagination. But retrospective reconstruction often leads to the observation of so many close calls that we wonder, what if some particular impediment could have been relieved at an earlier stage? Many will argue, if their grant had but been funded, ... This chart has proven helpful in pointing out the many precarious steps, at anyone one of which a career may be in peril. -- the faultpoints.

c) The Institutional Setting; the Federal Role

The investigator's relationship to his/her institution and to the federal grant system is at the core of today's scientific career. No more amateurs.

At the present time, federal funding accounts for a lion's share of the support of scientific research at universities. From the perspective of the individual investigator, the dependency on federal funds is even greater, since the non-federal input will be concentrated on faculty salaries and the institutional infrastructure Even a momentary interruption of support, (while it may not immediately impact the investigator's tenure as a professor) poses grave stresses on the continuity of the research, on the employment of technical staff, and on the survivability of the investigator's research career.

The predictable consequence is a confusion of responsibility for the long term career interest of the scientist: the federal government has the means but eschews the responsibility, and conversely for the institution. The loyalties of the scientist are likewise divided and confused: none can ignore the imperative of getting their research grants renewed. Then pushed aside are all other activities, including intellectual cooperation in education as well as research, risk taking in the planning of research, even reaching out for technology transfer in applying new science. New structural approaches to encouraging interdisciplinary ventures are being actively pressed, especially by the NSF in its Centers programs. In my view, the best way to foster interdisciplinary creativity is not to impose new structures, but to liberate individual scientists to reconstellate themselves as called for by the scientific opportunity. Existing academic structures like traditional departments are castigated for discouraging novel individual initiatives. But it will be no remedy to clutter the organizational landscape with still more cross-cutting rigidified "improvements" that then take on a life of their own.

Further compounding these constraints has been the trend in grants administration, during the past decade, ever more to the *project* rather than the investigator as the locus of merit. Short terms of grant awards foster the micro-management of others' research, even on the part of peer scientists. This sets up another vicious cycle, that the massive burden of grants review constricts the pace and volume of feedback between the investigator and the review process. An application for a two-year grant may take a year's lead time, and then with very short notice should difficulties arise in the prospects of renewal that would then imperil the continuity of the work. These are no longer incidental distractions; they are built in to the daily life of the scientist.

Peer review is indispensable to the integrity of science -- e.g. the gatekeeping of the refereed journals -- which provides indispensable objective criticism and public exposure of new findings and ideas. At present, however, investigators are typically spending 20 - 30% of their time and energy in sustaining the flow of grant support, and in a setting of high anxiety that can only interfere with their creative thinking.

Widely misunderstood, however, it is not the *peer-review* but the *project* system that is the root of these stress; although it is "peer-review" that has attracted vocal criticism.

The short term emphasis on projects amplifies the stresses on individual careers; This is then matched by the systemic waste that flows from intermittent encouragement and distress, the nurturing of careers that are allowed to sprout, followed by intervals of drought or decapitation: in a word, careers are being administered, de facto, by a distant bureaucracy that accepts little responsibility for this facet of the scientific enterprise.

Still embedded in the project system is the ideology that scientific research is an amateur vocation, a discretionary incidental to teaching -- to which the investigator can return after a brief fling. Research is no longer an ancillary function of the university; some say even to a fault, it is the predominant criterion of recruitment to our major universities. I have heard some agencies brag that the average duration of grant support was seven years -- that was supposed to be an index that everybody could get a ride on the trolley car. They had made no enquiry, and obviously could know little, about what happened after they had been pushed off for the new crowd, nor the waste entailed in that see-saw style.

These frictions first frustrate, then deter many young scientists. It appears that many gifted students are turning away from scientific careers in anticipation of these problems. In particular, very few M.D.'s now are willing to embrace the risks of a research career as against the incentives of a specialty practice (and against a background of debt for paying for their M.D. education which puts them under extreme burden). While most of the emphasis, perhaps correctly, has been placed on the decline of secondary and undergraduate education in science, these motivational factors should not be ignored.

Remedies

The federal-university relationship has been evolving rapidly without much attention to institutional design. It appeared to be working admirably from about 1950 - 1965, and while the high rates of annual increase in appropriations cannot be replicated, some other features perhaps can. This approach has the merit of replicating experiments already done within the corporate memory of granting agencies.

Some essential features include:

a. Above all, recognition that an institution('s administration) is a *processing center* for flows of resources, not a primary *fount.* The "partnership" simile (of government & university) is a constructive image, but it may be misleading about the relative revenue-raising capabilities of the partners.

b. Lengthening the period of award.

I suggest one managerial reform that does not entail the reeducation of hordes of effector agents, namely a mandate that grant awards again be typically for 5 to 7 years. This would reduce the administrative load of grant review, and on the investigators, especially if there were a period of grace for the more gradual phase-down of a non-renewable project. Reducing the now intolerable workload of review would conserve the precious resource of competent peers. It might also enable a discourse between applicant and reviewers that is now rigid and full of mutual misunderstanding. Our current practice is vicious beyond imagination. If questions arise in the review of a project application, the supplicant will hear about them only after the peer panel has met, and often only after a deferral that will have caused incalculable trauma. The straitened bandwidth of communication, the fantasies that too often underlie the judgments of the peer review group without correction, these badly need reform with the help both of more human-scale procedures, and of technologies like electronic mail and file maintenance. Our other gatekeeping systems, those of refereed publications and of faculty appointments, generally give more intimate contact with the submitter, or more timely feedback and access to other options -- other reviewers, or other gates. Meanwhile our current research project system may be crowding out creative imagination in favor of managers whose primary skill is in ruling large empires. When scientist managers become so distanced from the bench, they might as well be conducting "telescience" -- if we carry that to its logical conclusion, the laboratory teams should be as well socialized, rather than "owned" by one PI, and their work directed by distant operators over electronic networks, according to who has the brightest idea on a given day.

Finally, if we are really forced to accept that the typical scientific career is going to be truncated in seven, even in fifteen years, we really had better attend to all of the other insidious implications this has for the tenure system of the university.

The extreme alternative, of lifelong tenure of research support, I do not advocate, even though that works reasonably successfully in systems like the British MRC, and de facto the intramural programs of government and of industry. Some interval of recurrent accountability must be optimal in balancing the stress of performance with the leisure and security for careful reflection; a seven year cycle should be about right to keep track of the changing seasons of a scientific life.

I am glad to note that several NIH directors have initiatived experienced investigator awards, with longer terms of grants for some new investigators, and other simplifications of their procedures.

We must share responsibility

The entire burden of renovation of the research environment should not and cannot rest solely on federal reform. There is much to attend to in our own houses.

All too often, the department has become the largest unit that sustains much intellectual and academic cooperation. Students funded from one project can spend some time in another lab in the same department; there is rarely a comparable facility across broader reaches of the university. Above all the project funding system has further bolstered the imperatives of specialization; many able professors have little experience and little culture beyond the domain of their discipline [projects]. The project system further preempts the loyalties that might be directed to one's colleagues and one's institution in favor of the nationally centralized fount. In that milieu, there is little incentive or latitude for leadership of any breadth even within science. Many able scientists will properly balk at involving themselves in formal administrative responsibilities: chairs, deanships and other executive positions are going begging, or are being filled with a well-founded sense of sacrifice on behalf of one's colleagues. This deprecation of leadership is part of a vicious cycle of anarchy and its associated ills of splintering what ought to be a community of scholars. We all share responsibility for the exertions needed to restore that community, one that includes the teachers, the researchers, and the administrators.

Undergraduate education.

Graduate education has been the focus of my discussion. I would not budge it from its remaining the seat of specialized learning. Nor do I advocate an advanced degree in general science and culture no more than I could see one in citizenship. I do deplore that at many places undergraduate education has become relegated to a prep school for graduate science and the professions. At no other stage of study do I see the possibility of integrating the sciences with one another and with the problems of society. The Human Biology program, an undergraduate curriculum at Stanford, addressed those goals. I do not believe they can be met fully satisfactorily in a four year year program -- this has to embrace a range of disciplines like biology, psychology, economics, sociology, not to mention core subjects like history and literature. But you can imagine how a proposal for a five year course was received! In that setting it has become one of the most popular majors; and its alumni are already making a mark based on a broad general education that includes rigorous science. (Its faculty alumni also include two university presidents, one of a major foundation, and an NIH institute director.) It is probably almost impossible to replicate, given the rigidities of departmental suzerainty. But it may not be too much to ask that undergraduate education be revived as the seat of broad learning. Yale is perhaps the last place where I would have to preach that sermon.