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Discussion on the Evolution of Research
Columbia University
June 9, 1995

Only Lederberg remarks are recited here

LEDERBERG: Oh, thank you very much, Jon [Cole]. It is always a pleasure for me to come back to what is literally my alma mater. I can't help but reflect that most of my teachers 50 years ago didn?t think I?d be addressing you here on this occasion. (laughter)

I could use at least ten of my 20 minutes in reviewing all of the accomplishments of basic research since World War II. Of course, any such recitation would heavily overlap the resumes that were presented by my colleagues this morning, especially from health research. In spite of, indeed because of, the applied mission that it has in promoting health, NIH has always given very strong emphasis to the support of basic investigation.

It proved to be rather difficult to devise far-reaching answers, for example, to cancer when w didn't have the foggiest notion what cancer was and, before that, about the dynamics of normal cellular growth. The most important pathfinder for cancer did not come initially from studies within the field but from work on pneumonia, with the discovery by Avery, MacCleod, and McCarty just over 50 years ago that DNA was the core of the genetic material. We then understood that cancer is a lesion of the cell?s DNA.

There're several lessons that one might reach from some reflection on that very hasty, I won even call it an overview, the view that I just presented. Physics and chemistry had their revolutionary upheavals, their major discontinuities even before World War II, symbolized in a way by the crowning event of that war; the atom bomb.

In physics, we know the deep philosophical cleavages - one example being Tom Kuhn?s original definition of a scientific revolution. That is, the change of paradigm that accompanied the introduction of the relativity theory and quantum theory. An era when the older and newer generations were just talking right past one another, and the issues of theoretical predominance had more to do with generational succession than persuasion by specific experimental data. That was Kuhn's original definition of a scientific revolution.

If chemistry had such discontinuities, I would place them in the latter part of the 19th centur with the emergence of concrete theories of molecular structure. I suppose Kekuli might be a reasonably symbolic figure for that point. I don't see sharp discontinuities in the history of

chemistry since that time but an enormous enrichment, accumulation, vitalization, and introduction of new technology. We now know the chemical composition of almost everything there is around. We're not mystified by structures of polymers and so on and so forth. But I don't see conceptual discontinuities having arisen since that time.

You don't always need Kuhnian revolutions to have extraordinary progress in science and technology. Perhaps some of you want to argue that point with me a little further on. But the organic chemist of 1901 could talk to the organic chemist of 1995 reasonably intelligibly. There is of course a considerable overlay of physical interpretation, the theory of balance, the physical foundations of the structure of molecules, and so forth, that might differentiate them But that's a gulf nothing like that between contemporary and classical physics, for example.

Biology is somewhere in between, but 1944 really was a revolutionary shift, and whereas there was no DNA in biological discourse up to that time, there is no biological discourse without DNA at the present time. Some people might say, perhaps to a fault, we are seeing in some areas reintegration of what might at one time have been called the classical biological disciplines with the insights that can be obtained by the very relentless reductionism of an interpretation through DNA.

A further observation is that the most important discoveries, and the ones I know best in biology, came unbidden and were unexpected by-products of other inquiries. Avery and his colleagues were not looking for the nature of the genetic material. They were certainly not looking for DNA. They were absolutely startled when this came out as the result of their inquiry. They were not looking to solve the problems of cancer. They were no dummies - they did have more inkling than they indeed led on in their first publications about the broader biological significance of what they were into - but they didn't even come from the disciplines that had been the main carriers of the tradition of that kind of research.

They did not regard themselves as geneticists. And geneticists didn't regard them as geneticists either. People remarked about the prematurity of that discovery, but it was no more than ten years before it became part of the solid rock of biological discourse.

But over and over again, one could find that real discoveries - now here, I'll have to speak primarily from the areas I know that are in biology - came as part of a process of inquiry and very often by no means the things that were defined in the project.

God forbid, you might find something that was not in the research project that you had outlined when you went after your grant from NIH or the NSF, that you got something unbidden. And you even dare hint that it might be what you're really after when you write up a project. God forbid, a reviewer will say the author has not demonstrated the feasibility of the experiments that he intends to make. It's a line probably repeated more often in pink sheets than any other single one that I can think of.

Another historical lesson: NIH, the National Science Foundation, and all the other wonderful descendants of Vannevar Bush's vision did show that research could be fertilized with federal funds and its pace greatly accelerated.

We discovered the institutionalization of research as a major governmental responsibility and function, and the universities and other institutions are more than eager to come in as the instruments for those kinds of development - but always with an uneasy alliance of academic commercial, and governmental interest. Sometimes in concert, sometimes in conflict, sometimes sub-texted with the divergent interests of the different parties to those agreements

Most of this research was justified in the halls of Congress by its instrumental applications, that research would give us better tools for dealing with health, would provide technologies that would be in support of industry, would provide one form or another of economic payoff and that promise has been, I think, quite reasonably fulfilled. No one has argued that historical investments in these programs did not pay off in the national and global economy many times over.

As the scale of those investments increases, as the world gets ever more competitive, and as people question some of the tacit values that were being furthered, more questions are being raised. And we have a pretty commonly shared sense of gloom about the slash and burn incentives that seem to be going around in Washington at the present time.

But there's a background of a relationship where social goods were generated by parties who may have had a widely divergent set of motives for pursuing them. Universities, researchers accepted funds in very large measure because they were the most effective way to garner support for more basic lines of research, and if they didn't say it, others before them correctl articulated that one way or another, there would be enormous positive fruits.

But one has to have concurrence that the technologies that develop are ones that everybody really wants. There we come into elements of social valuation and many other considerations that can lead to some controversy. Most obviously in a field of national security and what many political factions, what the majority of folks in this country regard as absolutely necessary for development in self defense - our enormous deterrent capacity, our secure base in military technology - others viewed as highly pernicious, dangerous to the future, of ourselves and the planet and so forth.

Most of us here think that prolongation of life and alleviation of disease is a positive good, but there are always nuances and wrinkles that excite enormous controversy when we come t detail. The technology of prenatal diagnosis and its implied warranty for preemptive abortion would be an outstanding example.

Less problematic is life extension, but this has presented many, many subsidiary problems in its wake. The aging of the population and its economic consequences. The costs entailed in later life in order to have the application of those technologies. The sheer fact that many live today will only terminate at somebody's will. That if the life support systems are maintained there will be an object that was heir to what had been a human being and often great controversy as to what are the legitimate criteria by which to suspend that support. Well, that's an ethical burden that many people find unbearable.

It may not always be voiced in those terms, but it's a problem that has touched innumerable people and will touch most of us as individuals at some stage. And there's a great deal of

ambivalence, quite fundamentally, about the benefit of having that power, of not having it ou of our hands to make those kinds of determinations.

So, scholarship today is in some areas somewhat on the defensive. The culture of science ha managed to survive but is under renewed assault - by the negative pressures and the temptations for operating in a direction that's more keyed to immediate technological benefit rather than the follow-the-path-as-it-takes-you kind of atmosphere that I recognize as more typical of basic scientific scholarship.

The university should be the seat of critical examination of all of these issues, starting with the engines of discovery. It's increasingly difficult to do, when support for science moves from the elicitation of creativity to contract for performance and when the tether that the institutions have is ever shortened. The subtext of economies in indirect cost recovery is in fact greatly reducing the autonomy, the freedom to maneuver, and the opportunity for interna choice on the part of our institutions. If all of the resources of the institution have to be put out there as the equivalent of the matching funds, although expressed as unrecovered indirect cost, there?s just nothing left over for things to be done from the institution?s own initiative.

There was a culture of scientific scholarship that I've described as one of the ideals - I wouldn't even want to call it the by-product - that I view as the central product of scientific scholarship. I didn't say the fruits, I didn't say the technology, I said the culture.

The conduct of science elicits and nourishes systematic inquiry of appeal to nature, of appeal to experiment in the settlement of dispute, of disclosure and exposure to a community, and I will borrow your colleague?s and mine Robert Merton's phrase of organized skepticism as a way to winnow out the truth. There's an implication that there is such a thing as truth, which others may have some skepticism about but which the scientific culture really does put very high on the standards that we try to fly. And we do try to maintain a system of publication o scholarly skepticism and of discourse as I think probably the greatest value of sustaining science at an academic base.

There are other ways of settling problems, of reaching conclusions. We see them in the political world, we see them in the courtroom, and I think it behooves us to ask to what extent our very civilization depends on the maintenance of the scientific culture as at least or of those elements.

I?ll just close with a very brief summary of what some of the problems are, or their genesis that I wrote about 30 years ago. You can tell the vintage of this writing with such medieval phraseology as "his" as a pronoun and the word "man" and so forth. I was moved to bring this out by a piece I just saw by Freeman Dyson in the New York Book Review a couple of weeks ago, "The Scientist as a Rebel." I had found it very provocative, and it resonated with some of the things that I have here. He attacks reliance on the transcendence of science in or breath, and then talks about the obligatory role of science in knocking down the icons in another, which I do regard as a transcendent function.

But this was embedded in the paper, which tried to provide some sort of systematic overview

of what grievances might be lodged against scientific and technical development on the part of some larger public. There is a fundamentalist strand of reaction to technocracy. It's being discussed this week in a symposium at the New York Academy of Sciences that Paul Gross has organized. It was headlined in The New York Times, "The Flight From Reason."

And I admit it runs fairly deep. A lot of people are frightened about the pace and the implications of scientific inquiry, that scientists are rebels, that they undermine many accepte icons in institutions at many, many different levels, and this was just an attempt to try to summarize how many there are.

We don't hear very much today about the conflict of science and theology. I guess that battle has long since been completed, except the underground is coming back again in some other versions. But many religious organizations - having gone beyond what they needed to do and staking their credibility on historical interpretation, the literal definitions of the history that's described in the Bible - from Galileo's time and right through the present have found scientific argument as prejudicial to their credibility.

I've alluded to the biological system and the threats to our equanimity or understanding or sense of how things had been, the novelty that's entailed in life and death being so much more in our hands today - if they were perfectly so, there might be less grievance, but there are always problems at the margin. There?s also the issue of how the depredations of the environment are a possible consequence of large-scale technological development.

There's a lot of argument in what used to be Marxist circles about whether new technology favored the capitalist exploitation of labor or was undermined by it, and we can be attacked on both sides for those kinds of development.

And of course, science as a transnational enterprise cuts across our tribal allegiances. If you don't think so, remember McCarthyism and the way that Oppenheimer was dragged against the coals - and the notion that science was important to technology and technology was important to national defense, so there was a subversive aspect to the cosmopolitanism of science. And it was attacked in exactly those terms by most of the totalitarian regimes. That Hitler thought there had to be a unique science, and Stalin thought there ought to be a unique Marxist science. Cosmopolitanism was a very nasty word and was enough to send people to Siberia or worse during such intervals.

Well, this is a very bare outline, but I was just trying to remind you how much is shaken by scientific progress in the very name of solving problems, intellectual ones, those of understanding, and technological ones. The old order is changed, and a lot of people don't like that. I think that we need to have very careful tempering of our own views about the beneficence of scientific advance - and when we push technologies all around the world, try t think who does get hurt as well as who has benefited. And I think the universities ought to b the places that are free for that kind of discourse. Thank you very much.

COLE: We'll now entertain some questions from the floor.

LEDERBERG: I'd be particularly interested if anyone wants to quarrel with my contrast

between physics and chemistry in discontinuities.

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LEDERBERG: Well, I don't think we said different things. I agree with you. I think the only way to sell federal investment in research is to point to tangible material payoffs in the same currency as what we're asking for, wheel money in and money out. What I'm hoping is that the other values that I think many of us in this room would also share are just not thrown out the window in the process and that we try to pay some attention to the elements of process and of aspiration that can contain them.

If you give in too readily to the technological-fruits notion, then you also have to ask, "Well, who is going to decide what term of investment is an issue? Who is the wisest person to know how to get the best and the most technology out?" and, "Are you sure that these are technologies that everybody really does want?"

But the basic answer to your statement is, yes, I think the only rationale that Congress is likely to accept at a level of funding larger than it's willing to offer the Endowment for the Humanities is going to be for the instrumental fruits. It still leaves us as the guardian of the other truths that have to be protected.

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LEDERBERG: I'm not sure I've reached the final thought I'm going to have on that question, but let me try a few notes on that score. My own belief is that the economic payoff from investment in training and in research is undiminished and that there are grave social losses that will ensue from a failure to recognize that point, but that's my own opinion on the matter.

I don't know the extent to which we can convey that to Congress at a time when so many of its current constituency feel that they're anarchists, that government should be abolished. So, despite our own views as to what would be the best for society, how do we cope with the circumstances now presented?

I have to make the following observation, which cuts across a number of levels of education. One thing for sure - and it's a surety that has led to some of our current economic predicament - is we live in a global economy. There is no way we can separate issues of our productivity from those of competing economies. And that says that if we are to have a competitive advantage in the price of labor, then that labor has to be more productive than it is in other countries or we'll not be able to sustain the economic structure to do that.

There will be a migration of jobs for the cheapest bidder, so that the quality of our labor force becomes an absolutely necessary pre-condition, in my view, for the maintenance of our standard of living. And I'm not sure if graduate education is a key issue in that spectrum, but I don't think it should be excluded.

On the question of what kinds of jobs will be available, they won't all be the high levels of

academic research. But there will be technically skilled jobs requiring ever deeper capacities if we are to be able to be competitive in our labor force with that of the rest of the world. I think most of us would say, the first thing that needs to be cleaned up is grade school and high school education, but it's not the only thing. So, those are considerations.

What might we have downstream? Well, maybe something not too far from what it was when I was growing up. Many are called, and few are chosen. Perhaps not everyone can expect to get the kind of job that they would like to. Maybe we need to find ways of at least selecting out the most motivated and the most able for those kinds of situations.

I think the market will take care of itself. If there are not jobs enough available for graduate students, you're going to get fewer applications to graduate school. Exactly where else they're going to go, I'm not absolutely certain, because things are drying up all over in a certain sense. I think we need training grants to be sure that we have a way in which the best talent still has an opportunity to rise to the top, and the rest of our system should support that as well.

Universities at large - I never mean this one, but every other one - are over-extended, have overgrown. We know that every teacher's college has become a university and wants graduate programs. We know there has been serious over-inflation in those dimensions, and perhaps some way needs to be found to reduce that. Eventually the market will do it, but that's got lots of lags in it, lots of externalities, and it may not be the most intelligent approach.

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LEDERBERG: Well, don't expect consistency in public reaction. We're going to be damned for making too much technology and damned for making not enough of it at the same time, and sometimes by the same people.

If our younger faculty could have more autonomy in making their own decisions about how to spend their time - if they're working in a highly competitive project environment, though, they have no alternative but to put every imaginable energy into getting their grants in, getting their projects approved, and I think education at the margin does suffer from that monomania.

I think highly skilled researchers, most of the ones that I know, are eager to teach but not too much, providing the right balance between their allowance for doing research and being accessible to students so that a university can come through on what it promises its undergraduates: an access to these skilled minds.