

Design Area Two:
Resource Allocation Among Scientists

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COLE: I think we're off to a good start. I can't say we have stuck to our timing, but we'll see whether we can make that up. And one of the ways in which I would propose that we do so is that I limit to almost non-existent time the introductions of our speakers, because they are so distinguished that they really need no introduction (laughter), and the biographies of them appear in your materials.

It's a great pleasure to introduce these speakers, who will be talking about resource allocation among the sciences. There are many different mechanisms for allocating resources among the sciences. Three of these are notable. And the designers here will consider how the United States should utilize various historically useful mechanisms. They will engage issues that surround their use, and will consider possibilities of new allocation mechanisms. And our first speaker will be Ralph Gomory, President of the Alfred P. Sloan Foundation whom you all know, I believe. Ralph?

GOMORY: Ladies and gentlemen, it is certainly a pleasure for me to have the privilege of having your attention for an estimated 20 minutes. Let me start by saying that probably the reason we are all here, and why people are looking for some rethinking of the scientific and engineering enterprise, is a budgetary matter. Were budgets larger, we would be going on without much thought.

I must admit that I am actually an optimist about future budgets for science and technology. So I don't share this concern quite as much as many of you. Nevertheless, I do believe that there are some new elements that it is probably useful to inject into our thinking about science and technology funding.

The present funding system, which for many years has worked well, reflects, of course, the mandates of the various agencies, the needs of DOD, of NASA, DOE, NIH, and the broader mandate of the NSF. The emphasis given to these agencies and their programs can, of course, vary greatly over time, and we have seen that. So, in my opinion, it is useful to augment this approach – and I think it's realistic to talk about augment rather than displace – by bringing in other considerations, such as economic prosperity, which are not well represented, in fact, by any one agency.

To consider, in addition to military security and health, economic prosperity and to attempt a simple rationale that encompasses both agency and non-agency goals, I am going to talk about

technology as well as science, because I'm quite convinced that if we don't do well in technology, we will not get the payoff from science.

Much of what I'm going to say today is consistent with the 1993 COSEPUP report, "Science, Technology and the Federal Government: National Goals for a New Era," of which I was one of the authors. In spite of these respectable connections, I do not expect everything I say to be non-controversial.

Let me start by asking, what degree of agreement do we have today on the support of science and technology? I think we have a degree of unanimity in our own community that science and technology clearly deserve funding, and preferably on a larger scale than now. There is agreement that the budgets of NIH and NSF should increase. But that level of agreement does not distinguish scientists from any other special interest group.

What is lacking and what is needed is an accepted rationale showing that support of this entire enterprise, and not just the parts of it that are obviously useful or fit an agency's needs, is in the national interest. And I said national. International is much easier. National is the hard part, as some of our previous speakers have pointed out. And more than that, we must also answer the question of what level of support is right for this enterprise.

How much university science and engineering is enough? An accepted rationale for a level of support will certainly not solve all the problems of the support of science and engineering. In real life, there is always the effect of the attitudes of committee chairmen and of real personalities and real politics that are discussed day in and day out in Washington.

But a rationale would help. And I suggest that we adopt in a serious way – and I'll explain what I mean by serious – the goal of being world leaders in science and engineering research. Taking this seriously means drawing the consequences of this world leadership goal, not leaving it as a largely rhetorical or political device.

But if we are to adopt this as a goal, we must also answer this question. Why should we as a nation – not just the people in this room – care if we are leaders? The answer is empirical, in my opinion, rather than theoretical. (laughter)

This leadership that we have had has worked for us in the past and is working for us now as a nation. Scientific leadership has given us, in addition to the obvious contributions to military security and health, it has given us much.

Scientific leadership in solid state physics gave this nation – not some other nation – first the semiconductor itself, then the semiconductor industry, and then the rapid development of the computer industry. Fundamental knowledge in molecular biology – and the people who have that knowledge, a point I will return to later – gave us a significant edge in biotechnology. And also on the engineering side, university-based work has given us many of the concepts and the people who have given us a leading position in software and in networking.

These industries alone have benefited the country far more than the investment in university-based work has cost. Certainly other nations – and Japan is the usual example – have succeeded without this type of leadership. But different nations are different. They have different systems, they have different strengths.

The Japanese strength has been downstream in the manufacturing and development processes, and not upstream. We should learn, and indeed we are learning, to do well what the Japanese or any other nation does well. But we should not give up what has always been or at least in recent years has been our advantage.

Another question that should be asked and hopefully answered is this: why should we bother to be leaders in everything? Why not be leaders only in the fields that have demonstrated their usefulness, the ability to contribute to national or agency goals, or more generally, to the world outside of science and technology? I know that's an unpleasant thing to bring up, but people outside our community do bring it up, and we must have an answer.

And to discuss it, I need to say a few words about the research process, something I was exposed to for a long time. Although the actual work of research is full of ups and downs and is unpredictable in detail, we do make progress.

We do steadily understand more and more. It is not surprising that when you start to understand things in a fundamental way – whether it's how man-made materials hang together, or how living beings function at the molecular level – that at some point, this understanding will let you do useful things you couldn't do before.

I say that is not surprising. On the other hand, there's something we don't know. In those areas where the practical linkage of this understanding has already occurred, steady applied progress will usually accompany steady research progress. But in areas where no practical connection has yet been established, even the most expert researcher does not – and I assert, cannot – know in advance how this practical impact will occur, or if it will occur.

Nothing indicates this difficulty – or perhaps I should say impossibility – better than the history of quantum mechanics. In the 1920s, there was no subject more pure and more esoteric than the then brand new quantum mechanics. There was the uncertainty principle, the baffling puzzle of electrons that behaved like waves one moment and particles the next.

It was a subject of exciting scientific and even philosophical impact, but nothing could have been further from application. In the 1930s, quantum mechanics began to have an effect in what was then called solid-state physics. And after the war, this improved understanding of the fundamentals of crystalline solids led to a better grasp of the role of trace impurities and their effect on the flow of electrons.

The transistor was not far behind, with all its tremendous impact on computers and on electronic devices of every sort, and through these, on the everyday life of all of us. Not much more than 30 years separated the esoteric and apparently useless from its enormous everyday impact.

In view of this, I think we should adopt the following unpredictability principle. Now brace yourselves, folks. I'm gonna' say this twice, right? (laughter) We can see when some area of science or engineering is useful. We can't see that some area of science or engineering won't be useful.

All right? We can see when something is useful, but we cannot tell that something won't be useful. Therefore, if the United States wants to have the advantage of being competent and able to respond when a field starts to have a practical impact, since this practical impact is unpredictable, we should have a strong position across the board.

This is a rationale for the support of fundamental science in fields that have not yet shown their usefulness. It does not matter whether this practical impact occurs as a result of an event here in the United States or abroad. The point is to be able to benefit from it. And you cannot benefit from it if you are not a player in that field.

So I think that we immediately conclude from this unpredictability notion that we should support basic science across the board at a level that enables us respond. That is to say, at a world-among-the-leaders level. However, we may sometimes decide to do more than that.

If something really starts to happen in a practical way – practical meaning impact outside of the field – we might make a decision that we want to do more than to be just among the leaders in the world. We might feel that there is some benefit to our society, whether that is through industrial leadership, through a contribution to health in this country or around the world – but we might decide that it's not enough to be among the leaders. We might decide that we want to be clear leaders, ahead of the rest. But I've said something there that I want to bring to your attention, though you may not like it, and that is that the selection of areas of really clear leadership should often be measured by societal contribution and not by purely scientific terms.

If this seems at all abstract to you, let me show by an example that if we take these notions seriously, they have practical consequences. And so in order to demonstrate that, I'm going to go back to the SSC, the superconducting supercollider decision.

When the SSC was being considered, we could have asked, is particle physics a field where, because of its clear contribution to society, we must be out ahead of the world as clear leaders, or are we content to be among the leaders? I think the answer to this is the latter.

The record of particle physics, to the extent that I know it, simply does not support the notion of a large societal payoff so far. I would conclude this is not yet a field for clear leadership, though it is certainly, as all fields are, a field in which we want to be an important part of the world. Therefore, I would conclude that the SSC should never have been built or started. And that we ought to work out something with other countries instead, that would allow all of us to move forward together in particle physics.

I don't have time to condemn other fields. But if I did, space would be at the top of the list. But while this group may presume I mean the manned space program, I also would include the scientific program as being funded at a leadership level. If we now turn to molecular biology,

with its clear relation to an emerging industry, as well as its application to health, we would, I think, come to the opposite conclusion. This country might reasonably decide that, in the interests of national health and the interests of the emerging biotechnology industry, that this is a subject on which we might wish to be well ahead of the world.

The goal of being either a leader, a clear leader, or among the leaders in a given field, is a measurable goal. It involves a comparison of the level of science and engineering in the U.S. in a particular field with a level of that same field in other countries. We are among the leaders if we're roughly on a par with the work done abroad. But the real point, of course, is to be in a position to participate if something happens. Note that what we have here is a comparison with other countries but within a field.

Testing whether you're among the leaders in a given field of physics – for example, condensed matter physics – does not call for a comparison of condensed matter with particle physics. Or with some field within chemistry. It does not call for the usual endless arguments about whether one field is more exciting than another.

It does not call for arguments about big science versus individual investigator science. It says we should simply measure ourselves against the world standard in each of these fields.

This approach does provide an answer of how much science is enough. It does this not in terms of increases or decreases from whatever today's budget happens to be, but in terms of supporting universities in science and engineering at a level that provides the desired level of leadership. If we were to adopt such an approach, I strongly believe we would find it to be both an affordable and a stable basis for the funding of basic research.

And, by the way, I think that we are pretty much there in many fields, simply as a result of the present system. So we're talking about an add-on, not a complete re-do. However, we also have to talk about benefiting from this basic research in leadership.

And we have to face it, talk about what we have to do as a country to benefit from a leading position in basic research. And this is necessary if we're going to justify the expense of leadership to the people who pay for it in this country, a point that several speakers have already made.

The thing I think we have to concentrate on is the flow of people, not the flow of papers. I think that the emphasis that we hear over and over again about results flowing is a form of academic myopia. If you talk to people in industry, what they usually want is people, the flow of people trained, well trained in what is happening, from universities into industry. And this is a point which one of our previous commenters, Mr. Steinmuller, made in his own way.

Certainly, one of the most important mechanisms for benefiting from research leadership is through the flow of people. One person carries a lot of papers in their head, a lot of knowledge in their head, much of it that has never been put into a paper.

University people played a major role in the early days of Silicon Valley and of Route 128. And today, as I'm sure you know, the biotech companies are full of university people who play decisive roles. This seems to happen naturally enough when a science-based industry is emerging, but less naturally at other times.

It is important, therefore, especially in the areas that have shown their practicality, that we keep a steady flow of trained people into U.S. industry. This country clearly benefits from this flow from universities. And it is this flow that had much to do with placing Silicon Valley in the U.S., rather than somewhere in the British, German, or Japanese countryside.

There is, of course, since we're talking about government support, the possibility of government support of other activities, those such as advanced development in industry, or even support of the development process itself.

Most of the commonly used rationales for other forms of government support are very broad brush ones. Now I'll give you an example. Here is one of the most popular broad brush arguments. It is the familiar argument that the U.S. underspends on R&D. It is a discussion of R&D as a percent of GDP, of total U.S. output. This argument has been used indiscriminately by all administrations.

People point out that Germany spends 2.5% of its GDP on R&D, that Japan spends 3%, and the U.S. only spends 1.9% – a thought which is supposed to send shivers up and down your spine. I recommend, stop shivering, because we need to look more closely at this. First of all, almost all reported R&D is R&D done in the manufacturing sector. Banks don't report R&D. The manufacturing sectors of Germany and Japan are bigger than that of the U.S. as a proportion of their GDPs.

The numbers are, in fact, Germany, 30.6% of GDP, Japan 30.8%, the U.S. only 19%. If all manufacturing firms in all three countries were equally R&D intensive, R&D as a percent of GDP would simply reflect the size of the manufacturing sectors.

And this is exactly what these oft quoted numbers do reflect, with remarkable accuracy. These numbers do not mean that the foreign firms are more R&D intensive, just that their manufacturing sectors are larger and have more firms in them.

And more detailed analysis does show the firms in the various countries are, on the average, about equally R&D intensive. I could give you other sweeping favorite arguments, such as the externalities argument – that is, that companies do not capture all the benefits of their innovations. But – and I regret that time does not permit me to fire arrows one at a time into all of them – I believe that none of these sweeping arguments holds up under scrutiny.

To make sense of this area, we need detailed knowledge of what works and what doesn't work in bringing advanced work to fruition. However, that detailed knowledge is not easy to come by, and for a fundamental reason. We are used to the idea that there are things that are too small to see. However, we are less used to the idea that there are things that are too big to see.

But there are. A national economy is one. And the R&D system is another. We cannot see the functioning of the R&D system. That functioning is spread out in thousands of locations and depends on the efficient or inefficient actions of hundreds of thousands of people scattered through thousands of plants and labs and offices.

It would be wonderful if we had a macroscope to see in real time and in glorious detail the R&D system function. We could see what functions right and what functions wrong. But we don't have a macroscope. Statistics is, in fact, our attempt at a macroscope. And it only functions erratically. It functions erratically because if we have the right overall picture, then the statistics can size it right for us and tell us more about it.

But if we don't have it right, the statistics won't tell us that we don't have it right. The example I gave about R&D as a percent of GDP is of this type. If you have, either consciously or unconsciously, a picture of R&D as being done right across the economy, your statistics tell you that U.S. firms are underspending and give you, in fact, quantitatively the average underspending.

But if you have a picture that says the reported R&D is all in manufacturing, you get an entirely different answer from the same statistics. Realistically, today, we know very little about what works and what doesn't, what is needed and what isn't, where there's a realistic as opposed to an ideologically determined role or a non-role for the government.

We don't know where, in practice, the market works and where it doesn't. And we're also unclear of what to do when it doesn't. Basic research is one area where there's a considerable agreement that the free market doesn't work. And in this area, the government has learned to play a constructive role.

But outside of this, we know very little. We do not have in government a large cadre of experienced people who know how to work effectively with industry in the national interest. Nor do we have realistic criteria for when government support is needed or makes sense. I think that we need to learn.

And to learn, we need to experiment. Learning means, for example, that we should see where there is and where there isn't today a flow of people and ideas from our universities into U.S. industry. Is it happening? Is it happening in what areas? In what industries?

Do foreign graduate students still stay in the U.S., or do they head home to areas where there are new opportunities? Are there good ties between academic events and research areas in U.S. companies? Is the flow of new ideas going into our hospitals, or is it going into our school systems? And if we look, we will find places where this is happening, and we'll find plenty of places also where it isn't. When we see areas where we could be benefiting from our leadership but we aren't, then we should experiment.

There could be industries, for example, that are too scattered to do effective R&D either on their own or in connection with universities. A beautiful example is an industry like the powdered

metallurgy industry. They're too small firm by firm to make the leap from university-level knowledge to hard, industrial product.

There could be, as we all know, important health items for which the market is too small for the development work. And development work on things like high-temperature superconductors for which the materials work needed for products is too long-range for the potential user companies to do. These are areas we should experiment in and see what works and what doesn't.

If we do follow this path, we will develop some real knowledge and experience about what we get for certain government actions. And we need that knowledge, if the country is to benefit from government actions in this area. If we do experiment and get some knowledge, we can then have a debate, not about process, not about ideology, but whether the outcome of a given set of actions is worth the money and effort they cost.

Different people and different political parties having different social views will weigh these outcomes differently. But nothing could be more appropriate for political debate than a difference of views about the relative importance of different outcomes. This is a much better role for the political process, than a high-level debate about processes that need, in fact, to be understood in detail.

So, ladies and gentlemen, I do think that we can put together a rationale for across-the-board leadership, for clear leadership in some industries, but that we need to do work to establish the technology connection that translates that into a national benefit. And I say "national" not because I'm indifferent to the real needs of the rest of the world, but because I think that is a practical necessity. Thank you very much. (applause)

COLE: Thank you, Ralph. Let's move directly to David Robinson. You have his biography in his materials. David?

ROBINSON: The problem of budgeting for R&D has been with us a long time. Simply stated, it is this: where is the money going to come from? Today, we still do not know where the money is going to come from. My major thesis is that broad considerations of resource allocation among sciences make little sense. For most activities of government, science and technology are not goals in themselves, but are linked to major societal goals.

There is a long list of major societal goals to which science and technology contribute, including:

- improving quality of life, health, and human development
- increasing knowledge;
- expanding education and the diffusion of knowledge;
- improving personal and public health and safety;
- contributing to a high standard of living;
- creating and maintaining a civic culture;
- fostering community harmony;
- stabilizing population growth;
- nurturing a resilient, sustainable, and competitive economy;
- promoting economic growth, including increased employment and work force training;

- improving international competitiveness;
- modernizing communications and transportation;
- increasing environmental quality and sustainable use of natural resources;
- fostering worldwide sustainable development;
- enhancing resource exploration, extraction, conservation and recycling;
- securing personal, national, and international security; and
- improving social justice, individual freedoms, and worldwide human rights.

Science and technology contribute to all of these societal goals, yet most discussions of fund allocations ignore them and focus only on the economic and competitive aspects. One of the important national goals we have agreed upon is the advancement of science itself. In this area, we can talk about resource allocation. But if 90 to 95 percent of the federal expenditures on science and technology are discussed in the context of other goals, then it is the priority and balance among those goals that should be the major factor in the choice. In short, budgeting for science and technology is a major part of the political process. Instead of looking at fields of science as competing against each other, we should look at what our national goals are and how we make decisions regarding the allocation of funding for them including their science components.

Expenditures on science and technology are going to uncover new knowledge. They're aimed at improving things in the future, often the very far future. When preparing budgets, mission agencies have to balance funds they need to address today's problems vis a vis funds that will (or may) make their job better in the future.

Today, how much is the nation spending on cancer treatment? How much on prevention and education? How much on cure? If we develop a cure for heart disease and cancer, can we let kids start smoking cigarettes again? Technology fixes are always something we're interested in.

To summarize, my thesis, is that there is not a single science and technology budget. There are science budgets linked to various societal goals (as defined through the political process), and the budgets should be determined by how they fit those goals. The priorities should be attached to the programs, and should bring along the appropriate science and technology budgets with them. It should be left up to the agency or the research lab to make the case that the funds spent on science and technology are worthwhile and are going to make measurable progress towards these goals.

In the 1960's, I saw how this case was made at the National Institutes of Health. James Shannon – the brilliant leader of NIH when I worked in the White House – had a long-term, three-step plan for supporting scientific research. Year after year, he inveigled more money from Congress than the administration had proposed.

Shannon's first step was to promote both non-governmental and Congressional support. Enlist non-scientists like Mary Lasker and private, disease-oriented organizations. Cultivate Congressional committees. Shannon was wonderful with Congressman Tom Foley and Senator Lister Hill.

Second, demonstrate that immediate breakthroughs are possible. Be disease-oriented rather than health-oriented. It is much easier to list the diseases that you hope to cure rather than to explain the connection between current appropriations and the long-term health of the nation.

Third, invent special institutes. Every time you focus on a new disease, set up a new institute. Describe the budget by working from the specific to the general. Shannon would always talk about how much he was spending on heart catheters, for example, and then expand from the arteries of the heart to the heart as a whole, to the body as a whole, to other diseases as a whole. In this way, he could justify his budget.

Shannon also invented a research project category which, as a physical scientist, I had never heard of when I came into the White House. It was called “approved, but not funded.” Scientists would apply for grant funding and would have their applications approved. Shannon would then go to the Congress and say, for example, “We approved research grants worth \$500 million. But we only had \$400 million to spend. So we have \$100 million of grants that were approved, but not funded.” On hearing that, Congress replied, “We better give you the extra \$100 million to enable you to fund everything that you've approved.” The next year, Shannon would come in with an additional \$100 million of projects that were approved but not funded. So it went.

The other strategy Shannon perfected was funding multi-year programs "subject to availability of funds." He would approve a five-year grant, but only fund the first year. Since the federal budget is for one year at a time, the next four years would be "subject to the availability of funds." Shannon would go to Congress the next year and say, "We have \$400 million in grants we've already promised subject to availability of funds." Congress would start from that spot and vote additional money.

The other major point Shannon looked at was expanding the infrastructure. He started development programs and research in undeveloped areas of the country. He started a whole computer program in the 1960's, before anybody thought that computers would be important in biomedical or biological research. He supported proven investigators long-term and junior investigators short-term. He invented "Training Grants." He was allowed to fund research only, but he supported graduate students by calling it "research training." In sum, by having a program that he could justify to the American people over a long period, starting in the 1950's, Shannon built an NIH which spends significantly more than the National Science Foundation (NSF) spends on research.

One could give similar examples in other agencies. The Department of Agriculture started out as a research-oriented agency in the 19th century. The DOA used its support of science and technology through field stations and agents to develop general public support of science and technology.

I started out by saying, look at the institutional goals, look at the science and technology needed to meet those goals, and try to develop programs to justify that science and technology. What's wrong with this picture? Why can't we just look carefully and frugally at all of the government

missions and opportunities, put together the required science and technology budgets, and go home?

For a first approximation, that's fine. But in the second approximation, these mission activities often overlap. I was involved in a situation once where three agencies, the Air Force, the Weather Bureau, and the Geological Survey, were all interested in research on hail. All three agencies were sending airplanes to the same part of New Mexico at the same time of the year, because that's where most hail storms seem to be.

We have to coordinate and rationalize between and among the departmental budgets, and we must do more to eliminate unnecessary duplication of research. Cooperative activities should be encouraged. Some programs contribute to more than one goal. For example, computing for Defense can be valuable to the nation's pursuit of other goals, such as commercial technology and economic growth.

And there is a special situation with regard to fundamental science and technology. The NSF mission is to support basic science and engineering. In allocating its budget, the NSF has to be aware of scientific opportunities in what other agencies and the private sector are doing, then try to exploit the gaps in research.

This balance wheel function is troubling to some. In general, it appears to me that the NSF has to strive for continuity and balance, trying in all areas to respond to the highest quality proposals and to produce the people we need for the country.

The Stever taskforce of the Carnegie Commission on Science, Technology, and Government pointed out that the science and technology base must be built for the future (National Research Council 1992). We have to support: general science and math education; the science literacy of the public; higher education in science, engineering, and the social sciences; human resources; facilities; and institutions. These are long-term, national needs that must be supported by the federal government. Therefore, we need to have moderately stable science budgets. We also need to ensure that young scientists are trained well, and that the institutions that train our scientists are healthy.

Most scientists agree that they need money, but very few scientists believe that their institutions need money. This is why agencies must think about building the institutional base even as they carry out their missions.

So, what is wrong with this decentralized system? The problem is that our national goals change rapidly. If particular fields – and physics is one – are linked to specific goals, then the nation as a whole can be in trouble if the goal changes or disappears.

Our goal on health research has not disappeared. The NIH is moving along, though perhaps not as rapidly as we think. But biomedical research consistently gets more money from Congress than the administration requests.

Fields such as physics, computers, and communications are important for our economic competitiveness. Congress has been willing to support them bountifully for defense purposes, and commercial industry has reaped the gleanings from that harvest. Without the harvest, there would be no gleanings. These fields are suffering as defense budgets decline.

In principle, if we agree that defense spending is going to decrease but these other sciences are very important to other goals, and the NSF could provide the balance. In practice, in an era of tight budgets, NSF will be hard put to do very much extra. Making the best budget decisions in this complex situation is extraordinary difficult, and I do not believe there are simple criteria or simple answers.

We have to focus on major changes. Sometimes we will keep on supporting things we shouldn't for political reasons. In many cases, I don't believe that fighting to be number one makes any sense. I do think being competitive in all major fields is very important.

Within agencies, program managers have to balance one field with another and the present versus the future. After they've done this balance the best they can, I believe that the White House Office of Science and Technology Policy and the Office of Management and Budget should review the situation as a whole.

These reviews must pay attention to what is happening internationally and in industry. And I endorse completely the method of experimentation, review, and seeing what works. Adjustments may have to be made. I wish there were simpler ways of dealing with this, but we have a very complex system. And I believe that we have to look at it in its complexity before we can make any useful decisions.

COLE: Thank you, David. We have two distinguished panelists, and we'll move on to hear their initial comments, and then we'll hear from the floor. Dorothy Zinberg will speak first.

ZINBERG: As I moved back so as not to interfere with David's perfectly terrible slides, I had an absolutely paralyzing thought, because I realized, yes, I am here to discuss the federal aspects of funding. But my livelihood and that of most of my colleagues is based on private foundations. Who was I to comment on David Robinson, the Carnegie Foundation, the Carnegie Commission on Science, Technology and Government, and Ralph Gomory of the Sloan Foundation?

First of all, my memory was when McGeorge Bundy became the director of the Ford Foundation, John Kenneth Galbraith said to him, "Mack, you'll never hear another honest word." So, if some of my comments are orthogonal, it's Darwinian. And accordingly, part of it is made very easy, because I totally agree with what David Robinson has said, though he'll be shocked to hear that.

But I did have some disagreements with Ralph Gomory's comments. And I thought I'd try to put them as a null hypothesis rather than in a disagreement, because if you look at things differently, you might begin to come up with some different solutions or even ways of beginning to think of how you resolve a dilemma.

And it would be very valuable at this conference to look at this issue: what if there is not as much linearity in the future as there has been in the past about funding and about thinking about the role of science in society? And for those of you who were not present at the creation after World War II, it is within memory for many of us of what a very, very different world it was and how the changes now are not small but major.

And let me just name a few. One is a reigning value or a reigning opinion that was given voice by someone like Bruno Brunofsky: "Look, we scientists are simply terrific. We are honest. We are smart. And the public doesn't understand us, nor do they have to. Just give us the money, and we will give you great science. We need no outside interference." That could have been written in the Middle Ages, in terms of where public attitudes and expectations are now. And yet it's only 40 years ago.

To put it in its wildest sense, I think it was in the '60s that Dan Greenberg, who's been the gadfly of the scientific community forever, invented a character he called Dr. Grant Swinger. It came out of a wonderful ad for Scotch whiskey, which said, "As long as you're up, would you get me a Scotch." But Greenberg said, "As long as you're up, would you get me a grant."

Dr. Grant Swinger had created the Center for the Absorption of Excess Federal Funds. This was a sense that there really was so much money floating around that all you had to do was ask for it. In a more serious vein, that was echoed by Jim Watson at his 60th birthday discussion, where he said, "In those days, science was fun. If you wrote an alpha proposal, you got funded. Now even the alphas are not funded, or only a small percentage of them."

So, we're beginning by the late '70s, early '80s, the disjunction between talent and available funds. And this has only been exacerbated in the numbers of scientists we have been educating and some of the dilemmas that come from this.

If we had had this meeting in 1990 and talked about where we need funds, I think there would have been very few voices talking about the radical shifts that are coming about through the Internet, through all the interactive media and where that's going to take us with funding and emphasis.

So I think that is one way we have to look at how quickly things are changing. And perhaps in terms of taking this conference into its next phase, it might be worthwhile to think about what is afoot if it isn't going to be linear funding from the government with some kinds of tinkering up and down.

Now, let me just say briefly, when Ralph asked how much science is enough, I think that is essentially an unanswerable question. (laughter) Some of us did get an answer in part to a question like that. A few weeks ago, George Soros gave a lecture in which he said, "After I made \$12 billion, I decided that was enough." And so we finally learned what is enough money.

What is enough federal funding or what is enough science obviously is not answerable. I think this is a big issue, and I hope we'll get to that in the university part, because there is where federal funding can play, has played a major role. And yet we haven't been able to support the

institutions that are employing these people relative to the jobs that have been implicit in this federal funding. And I think that speaks a lot to what a science education is and what science is about, if it isn't a career.

Ralph Gomory raised several questions about what was happening to the international scientists who came to this country. Did they go home? Did they stay here? Those were just the questions I wanted to answer in the proposal that was turned down by the Sloan Foundation. (laughter)

But what we do know is that the investments that Taiwan and South Korea have been making in their scientific infrastructure are really beginning to pay off in the numbers of first-rate scientists and engineers that they are producing. Their numbers who are coming to this country are beginning to drop, not precipitously, but they're beginning to drop. And the numbers who are returning, we're seeing very large numbers. More than 25,000 have already returned, because of enticements, because of cultural comfort, so that we're beginning to see the kinds of shifts that we really never anticipated. I throw all of this in to say that I'm not sure the assumptions of linearity are going to persist.

I also feel that much of what has been going on here, the questions are essentially questions of social science. If you look at federal budgets and what is most likely to go right under the axe, it's social science.

And I would say that we are asking questions that can only be answered by social science. And that if we wanted to look at the NSF budgets and the attacks on the NSF and other sources of funding, we should be thinking very hard about how we are going to protect the very groups that have the expertise to answer some of these questions which we think are of such importance.

There are books now being written about the end of science. I don't believe them. We have to take that very seriously. Does that reflect something larger that's going on? And I was struck at the beginning by Jonathan Cole's comment that what we have is a crisis of public understanding in science.

I would say that is matched by the crisis of the scientists' understanding of what's going on with the public and the Congress. And that perhaps much of our time in the near-term future should be directed, not only to public understanding of science, but of scientists' understanding of what the Congress and the larger society are about now and why federal science funding may not be as linear in the future as it has been in the past. Thank you. (applause)

COLE: Thank you, Dorothy. Our second commentator and panelist is Rita Colwell.

COLWELL: Realizing that I essentially stand between you and lunch, I will be brief. Also being an anomaly, an administrator still doing science, I came prepared for a slide-assisted lecture. But seeing the room and the visuals, I have weeded out all the tables and just have a few illustrative slides.

I forgot to bring my technology with me. I had intended to be prepared to be combative and to be disputatious, but I find that I'm in much agreement with the speakers and my fellow commentator. I'll give you my bullets first. And then I will amplify very briefly.

I'll make a bold assertion. I had originally intended to say, Ralph, that the first point I wanted to make is that physics no longer rules the world. However, in rethinking, I think the laws of physics do still apply. But physicists will not be there in charge in the 21st century, which is only a few years away. The best and brightest and largest numbers have been going into the life sciences where "the action" is.

And so the cadre of personnel in the 21st century is going to be in those life sciences. And I would also say that science, as it is being practiced now, is interdisciplinary science. And I will use an example from molecular biology of how physics and chemistry, mathematics, computer science, and biology come together to bring these advances that are occurring in the life sciences.

My third point is that the basic-versus-applied dichotomy argument is really irrelevant, because in fact, what is happening is that the basic research that ends up in science is applied immediately by companies. I'll give an example of that as well. And I do think that we are in a paradigmatic shift. That's a buzz word. But how we do science is much more in partnership with the public. We are doing science in a more visible way, because a decade or two decades ago, no self-respecting – how shall I say it? – hubris-carrying scientist would speak to the media, would be involved in public debate.

But now, we know that it is part of our responsibility. But more than that, our responsibility includes educating the public. And again, I will amplify and give an example.

Finally, I would say that the virtual university is here. I think the discussion about whether we shall be international or not: just talk to a couple of scientific hackers, and you know darn well that they're interacting with their colleagues all over the world.

In fact, we were recently doing a study where we discovered seasonality in a diarrheal disease. We put out a call to the Web, "Does anybody out there have such a result?" And sure enough, back came from New Zealand that, indeed, they observe seasonality. It was in September, October. Ours was in March, April. It meant that spring was the factor. But this call to the international scientific community immediately brought a response.

So that resource allocation as we discussed today, from my perspective, must be in those areas where we are leaders, but also those areas of social relevance. Now let me just very, very briefly go through the biotechnology as it's presently defined as an applied biological science. It's old and new technologies, any technique that uses living organisms or parts of organisms to make or modify products, to improve plants or animals, develop microorganisms for specific uses. All of this comes under biotechnology.

The seminal work in genetics was done in 1865 by Gregor Mendel, an Austrian monk whose studies on the pea plant elucidated the inheritance of traits by hereditary factors. His work was

ignored until about 1900. But once rediscovered, his findings fit very well with what by then was known about chromosomal activity during cell division or mitosis.

The early to middle portion of the 20th century was a very exciting time, with major gains in knowledge of genetic inheritance. Thomas Hunt Morgan of this university (Columbia), working with a fruit fly – *Drosophila melanogaster* – showed that genes or the units of heredity were the constructs of chromosomes. His student, Alfred Henry Sturtevant, who later joined him when he moved to Cal Tech, made breakthrough discoveries showing that genes were linked, comprising chromosomes. Thus began the science of genetic mapping, a technique essential to the new genetics and employed, of course, in the human genome project, one of the biological megascience projects.

In the 1930s and the 1940s, genetics research was inextricably moving in the direction of the upcoming explosion of knowledge at the molecular level. People such as Barbara McClintock – she was ridiculed until eventually she proved prescient and was awarded the Nobel Prize. And also the work of Marcus Rhodes, who studied linkage and mutable characteristics in maize and corn and provided a new view of genes as being more mutable and variable than the Mendelian genetics allowed.

Meanwhile, research into what comprised genetic material moved forward very rapidly. In 1928, Frederick Griffith found that a "transforming principle" was able to alter traits in a bacterium – *Streptococcus pneumoniae*, as it was then known. By 1944, Avery, MacLeod and McCarty of the Rockefeller University identified the transforming factor as DNA, deoxyribonucleic acid. And from that moment, the scientists in many laboratories labored to determine the chemical structure of the DNA molecule.

And finally in 1953, James Watson and Frances Crick's short paper in *Nature* was the breakthrough everybody was writing for. Well, since then, the applications of biotechnology have simply exploded. The ability to clone genes into plants, for example – what was a pyrotechnic kind of experiment, to clone the luciferase gene into the tobacco plant. Probably the only good value of tobacco: it lights up by itself, instead of being lit up and smoked by someone. But in any case, by cloning that gene into the tobacco plant, we were able to utilize that as a tool, as a reporter for the functioning of genes.

And since then, we have all kinds of applications in diagnostics and medical treatment. I will use one example, that of protoplast fusion: the ability to take two plants that do not form hybrids but by manipulation, by fusing protoplasts, producing, for example, the "pomato," where one has both your french fries and your ketchup in the same plant. The applications of genetic therapy, as we well know, are enormous, and they come from this huge capacity to use genetic material as the new manufacturing.

So, what has happened? The first U.S. biotechnology company, Genentech, was founded in 1976. Now, barely 20 years later, it is joined by more than 1,300 companies in the United States alone. In 1981, the first U.S.-approved biotechnology product reached consumers, a monoclonal antibody based diagnostic test kit. The following year, the pharmaceutical, Eli Lilly's recombinant

DNA human insulin, was approved for sale in the U.S. and Great Britain. And the Humulin sales just a few years ago were \$560 million.

From 1981 to 1987 was a watershed period for the United States in biotechnology. An average of 90 companies were formed every year, for a total of 600 companies just during that six-year period. Although most biotechnology companies still are not consistently profitable, an increasing number of products have entered the market. The market value just in the last year or so of biotechnology companies was somewhere between \$40 billion and \$50 billion, with R&D expenditures of \$7 billion and more than 100,000 employees. This is an industry that did not exist 20 years ago. In comparison, the U.S. pharmaceutical industry, which is heavily invested in biotechnology, had R&D expenditures in this area of only about \$13 billion in 1994, so that we see a very rapid movement of the discoveries from the laboratory to the field.

Now, I would like to speak about the interdisciplinarity. This slide is the three-dimensional structure of the T-cell receptor. The molecular structure of the T-cell receptor – the T-cell, as we all know, is very, very important in AIDS immunology – has preoccupied immunologists for a long time.

Now, recently, researchers at the Biotech Institute at University of Maryland have made contributions to the understanding of the three-dimensional structure of the T-cell receptor and its correlation with function. The three-dimensional structure here shows the beta chain of the T-cell receptor as revealed by x-ray diffraction. In this particular photograph, the complementarity-determining regions at the top are shown in yellow and green and orange. And this is the part that comes in contact with foreign antigen and the histocompatibility antigens. This is one of the component chains of the heterodimer that makes up the T-cell.

More recently, the researchers at the Center for Advanced Research in Biotechnology (part of the University of Maryland Biotechnology Institute) determined the 3D structure of the variable part of the alpha chain from a T-cell receptor. And so with this model and the one that I just showed you, the complete receptor could be modeled as shown here. Next, the researchers crystallized and determined the 3D structure of a complex between a bacterial superantigen and the beta chain. And the 3D structure of this complex, which will appear by the way in *Nature* in a few weeks, explains how superantigens elicit non-specific, useless immune responses to allow infecting microorganisms to proliferate in their invertebrate hosts.

What's the message? The message is that all of these studies use physical techniques and concepts such as x-ray diffraction, thermodynamics, data from x-ray diffraction experiments stored in the computer. The computer graphics are essential for the representation and study of the 3D structures. The material for the studies comes from biological systems and samples. The molecular biology techniques, such as expression of vertebrate genes, are all part of the experimentation.

Where does one discipline leave off and the other begin? It all weaves together to make the discoveries that are so important, which then very quickly go into industry. For example, the researchers at the same laboratory have been working with Proctor & Gamble on the structure of

the protease that breaks down protein molecules. They were able to genetically engineer it, so that it would be more functional at high temperatures.

The basic research appeared in *Science*. And within the next week, the Proctor & Gamble scientists were incorporating the protease in their detergents. What is basic? What is applied? It is meaningless. It is irrelevant. We are moving so quickly in this area that the science we pull from the laboratory today as basic, funded as an RO1 at NIH or as an NSF grant, moves quickly into application and for the betterment of the human condition.

Let me speak to the social issue. Now, one of the areas is marine biotechnology. In 1800, the world fisheries were about a million metric tons. By 1980, the fishing accommodated about 100 million metric tons. The maximum sustainable fisheries of the world oceans are 100 million metric tons. Our harvesting has begun to decline, and that is because we are fished out, as *Newsweek* put it a year ago and as *The Economist* put it in an article recently, "The tragedy of the oceans."

To feed the 1.6 or the 1.8 billion increase – that is, from 6 billion to 10 billion, whatever the population increase will be – in order to provide the protein, we cannot look to the oceans and wild fishing, because the maximum has been reached. The expected need is somewhere between 135 and 165 million metric tons by the next century, a few years away.

That impedance mismatch, that gap can be closed only by aquaculture and biotechnology applications, producing transgenic fish that reproduce rapidly, grow to a larger size. The first transgenic fish with a growth hormone introduced from one species to another has already been accomplished. This technique will provide us with a capacity to meet the needs – so the social relevance is important in establishing our priorities.

Now, one other aspect I would like to amplify is that we have just opened a new laboratory, a new research laboratory in Baltimore. It's a new kind of laboratory, and I believe it's a laboratory that is of the future. Two-thirds of it is basic research, with scientists doing research in marine biotechnology, molecular biology. But the other third is for education of school children and kindergarten children and also a public exhibition area that allows the public to see exhibits that explain what is going on in the laboratories. And the building is built such that the public, as it goes through the exhibits, can see scientists at work.

The scientists in the laboratory doing the basic research are the docents. And the experiments, the hands-on research, are actually the kind of simplified research being done in the laboratory. This is the kind of interaction that is necessary to demysticize science. It is necessary to build the scientific community, the voter support of what we are trying to do, because they will understand it. And we can translate it for them.

Well, I'm running out of time, and as you can tell, once I get started, I begin to proselytize. But I do believe the point of the virtual university is a critical one as well. We are already, most of us who are involved in university education, involved in distance learning.

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At the University of Maryland and I know other universities – Cal Tech and USC – we are involved in partnering in curriculum development with Norway and Sweden, with real time sharing of lectures, developing programs whereby students in Norway and Sweden and Maryland take courses together, take lectures together. And the faculties are combined as one university with a program in marine molecular biology. And I know this is happening everywhere.

It makes the argument about whether we should internationalize, whether we should be productive. The point is knowledge, like microorganisms, does not carry a passport nor does it respect borders. It is a shared resource, and globally. So I will close by saying that we have gone from assembly lines to fermentation vats. We have plants as factories. We are able to utilize the tobacco plant to produce compounds.

And I know that the Boyce Thompson Institute has recently cloned antigen genes, that is, the antigens for cholera and some other diarrheal diseases, into potato and into bananas, so that the new form of vaccination will not be by injectables. It will be by ingestion. And for little children, it will be a very quick and simple way to vaccinate.

I close then by saying the future is very exciting. We must be leaders, because scientific discovery and application moves much too quickly for us to lag behind. Thank you. (applause)

COLE: Thank you very much, Rita. And thank you all.

This is the moment in which I must engage in executive decision-making, since we were due to begin lunch about 20 minutes ago. Why don't we have one or two questions, and then we will break for lunch immediately thereafter.

MALE VOICE: David, I guess I'd like to ask you the following question. I agree with you that we should orient our funding paradigm according to missions. The question I have for you is, how do we arrange that the educational system will produce people that have a passion to contribute to those missions?

ROBINSON: I think the problem – how you get the education system to do that – is a complicated one, because it's a societal effect. I think we are an entrepreneurial society. And I think what you've seen in the software business, for example, is how the Japanese trying to develop a fifth generation computer from the top failed against our decentralized system, because of our society.

All I can suggest is that you work from example. If you have the professors interested in the outside and interested in the kind of thing you could do, then the students will be that way. But if you don't have the professors interested, you won't get the students.

MALE VOICE: But as you indicated, the NSF sort of sets the tradition, in terms of the support of research and education within the universities. And so if they are to remain detached from the missions, then the professors are also likely to be encouraged to remain detached.

ROBINSON: Well, I think we will move only slowly, and it will take a revolution to make a really big change.

COLE: Other questions? Paul, did you have one? Paul David.

DAVID: I want to try to ignore the very good and sound advice that Dorothy Zinberg gave about not biting the hand that feeds you. My experience with the Sloan Foundation is that they fund the proposals they like regardless of what the proposers say about them. And so I want to go after one-half of Ralph Gomory's proposals. And this is what appears to be a very attractive standard of measuring the level of science effort in a given field – not against other fields, and therefore trying to avoid internecine and unprofitable struggles within the science community, but to refer ourselves to what is happening in the world around us.

I think this is a very interesting and perhaps very practical sort of rule, algorithm to follow for the United Kingdom, but not for the U.S. And the reason that I'm led to that view is that in its absolute size, in its present, preeminent position in the world, the U.S. is not a small player. Decisions to not be preeminent in a given field can be signals that can be taken by funding people in other parts of the world to justify decisions, to cut back levels of effort in those areas.

In this kind of situation, what we can easily wind up doing is finding assurance in our reflection, our judgments reflected in the mimetic behavior of other countries who are also struggling to cut budgets and free up resources for projects with shorter term and more certain payoffs. That we will in fact enter in a race to the bottom. That is, mutual abandonment of certain areas of research. In positive feedback situations without any damping, you have a potentiality for unstable movements in either direction.

Therefore, one has to accompany this kind of program with some checks, some set of references to scientific and engineering judgment about where possible, unexploited, future developments may exist, even though the people aren't working in those areas. Without that, it seems that we would be throwing away a major part of the advantage that we've created by becoming the country whose scientific and engineering research establishment is the envy of the world.

We would decide not to listen to the judgments that issue from that community about what science is worth doing, but rather to place our faith in the political decisions that are made in other countries whose complex science policy processes are not less Byzantine or less bizarre in their outcomes than our own. Perhaps, Ralph will respond to that.

GOMORY: I'm not completely confident in modeling the world response to all of these things. I think it's complicated. But I do think that we need some rationality here. And my concern is really the opposite, that we do need to protect basic research against the assault that everything needs to be useful. And that's really the thrust of my remarks.

For example, let us take the spending of the United States on astronomy. Now, is that in balance? I think not. Much of that is NASA spending. And many billions a year are being spent. Now, of course, the manned program is a peculiar one, because it is masquerading as scientific spending. But even if we go beyond that, if we take this \$2 billion or \$3 billion a year that is spent on

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mostly planetary astronomy, I would say we have clear leadership at the cost of several billion dollars a year in that field, and we don't need it. And I think this kind of thinking will help us to sort out where we should spend our money.

I am aware of the kind of dynamics that you describe as a possibility. I have to wait and see whether that really will happen. I think that the actual mechanics of the world are far more complicated than that. And I don't think that we should give up rational thought because of an elaborate cascade of events that in fact are unpredictable. Thank you.

COLE: Thank you, Ralph. Why don't we now adjourn for lunch.