



Consortium of Social Science Associations

20th Anniversary Annual Meeting

Edited Transcript

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Keynote Remarks: David Ward, American Council on Education

Janet Norwood, COSSA President: COSSA has been in existence for two decades and we have an unusual program to celebrate the 20th anniversary. As the program unfolds, I hope you will agree with me that it represents an extraordinary effort that has been well put together; we all should learn a great deal from it.

I'd like to acknowledge Howard Silver [COSSA Executive Director] who is, in large part, responsible for where COSSA is and how COSSA has grown and developed. I will now introduce Ron Abler from the Association of American Geographers, who will introduce our speaker.

Ron Abler, Association of American Geographers: You have the details of David's biography on the yellow handouts in your packet, so I won't repeat what's there. I'd like to simply welcome David to Washington and to the social science and behavioral science community in Washington.

Some years ago I read a paper at the annual meeting of the Association of American Geographers called "The Red Coats Kept Coming" which dealt with the influence of British geography on American geography, which has been strong in many instances and in most instances beneficial.

I'm delighted that a Fulbright Fellowship brought this particular British influence to the United States. He has been a powerful force both intellectually within geography and, of course, administratively at the University of Wisconsin and elsewhere.

David, I'd like to thank you for joining us, particularly on short notice, and welcome you to the COSSA annual meeting. We look forward to your remarks.

David Ward, American Council on Education: I'm delighted to be here because, in a way, it's a homecoming. I remember 1982 and I was an associate dean of the graduate school at UW-Madison when COSSA was formed. The big question was, long before it was thought desirable by most people on campus, who would pay for it?

So, one of my early diplomatic shenanigans was to figure out which dean or whether, in fact, the chancellor, should pay for it. At the time there was a biochemist who was the dean of the graduate school who immediately said the dean of letters and science should pay for it – "all the social scientists are over there."

In the end, I think, for a few years the College of Letters and Science and the graduate school co-paid our membership, but then I became provost in 1989 and that came to an end. The administration paid for it. So I was able to exercise some rudimentary power upon becoming part of the central administration.

I feel this is also a coming home for me because of my engagement in social studies through my role as a human geographer, immigration historian, demographer of sorts, and urban historian. Having a multi-disciplinary research experience, I felt this organization was very important to me because I did receive support from many at-large entities that support the social sciences.

I was on the National Science Foundation Geography Regional Science Panel and also on the board of directors of SSRC [Social Science Research Council]. I felt, particularly through the '80s, that I was very much engaged with the transdisciplinary range of what was going on. It served me well as Provost and Chancellor and continues to serve me well in my first few weeks as President of ACE [American Council on Education].

In a way, ACE brings me closer to the role of COSSA, that is, advocacy of higher education in a broader sense, rather than just the social sciences and, in particular, advocacy which comes from a membership which is everybody's second organization. ACE is the umbrella organization, has about 1,800 members and, of course, each institution has a niche membership, whether it's AAU, NASAULGC, or AASCU, depending on whether you're a four-year college, land grant, private, public, and so on.

I'm the person who has to bring together these organizations to speak with one voice to Congress and to the

private sector, not unlike how COSSA brings the disciplines together to speak with one voice on the social sciences as well.

My organization, like yours, is going through some re-evaluation. I was impressed by the efforts you've made after 20 years to figure out what is the best strategy to reach Congress, to reach the public, and to connect with each other, which is indispensable if we're to continue to be successful.

The same thing is true for higher education in general, and I notice that in this book marking your 20th anniversary and looking at the meeting's program, you're attacking problems rather than pursuing disciplines. I think one of the great challenges that we all face is how to attack problems not necessarily from a disciplinary perspective, but as teams in a multidisciplinary sense. I'll say a little bit more about that later.

ACE for years has been the main advocate of tuition policy. It's the main organization that deals with federal student loans. It has also been the key architect of much of the efforts with AAU and with the disciplinary organizations supporting NSF and NIH. It's also been one of the key supporters of diversity efforts; the lead filer of amicus briefs on behalf of institutions that still struggle with the challenge of how to create an outcome which results in diverse graduates from our campuses.

The challenge has been to look at the outcomes. That is, the kinds of outcomes that can be clearly understood outside of the universe in which we function. Clearly, higher education, in my judgment in the last 10 years, has probably gone through a series of unprecedented and irreversible changes.

How we are managed, and how we are funded, in particular, has changed dramatically since 1990. If you were to look at the political economy which drives the revenues which then drives the outcomes and allocations to your departments, you would find that their sources vary.

People worry about endowments even in public universities and the re-allocation between tuition and state support has changed dramatically and been only slightly offset by federal loan policy for students. This change is unlikely to be reversed, and with state economies now being squeezed once again, I suspect the tuition pressure will be there for us once more. And I suspect the issue of access to universities will once again become key in the next five years.

Access has been driven largely by diversity issues for the last 20 years. It will now be compounded by genuine and serious issues about access related to socio-economic status (SES), particularly income. If you look at the current profiles of our institutions, the lower decile and certainly the lower 20 percent of our SES are very underrepresented in four-year and doctoral campuses. That will be a great challenge in the Higher Education Authorization act.

My challenge has been to focus on long-term issues these last six weeks when the events of September 11th were so dramatic and, in many respects, demanding of organizations like mine. The immediate issue that had to be faced was whether to take a dragnet approach rather than a surgical approach to the problem. Should there be a moratorium on all foreign students arriving in this country? I'm not quite sure.

But this was a great challenge for people of impeccable liberal credentials as well. The places where these requests were coming from were a great surprise. I think with the help of the California university system we were able to reverse that particular proposal by suggesting that we would collaborate as we always wanted to with the State Department and the INS in a tracking system that could provide a more satisfactory level of comfort about foreign students.

It was very nice to give testimony and put my hand up and say I too came on a foreign student visa. Of course, when I came in the middle of the Cold War, there were serious efforts. I was interviewed. I had to declare myself not to be a Communist. When I arrived, I had to give my address. When I got my Ph.D., I had to leave within one month.

I did all that. It didn't bother me one bit because I came back as an immigrant and I'm now a bicentennial citizen. Clearly, we have, in fact, had a barrage of questions about how to deal with visas. I think a middle ground

was a very reasonable response to these events. But dealing with that proved to be quite difficult because of the polarities that began to develop as a result of September 11th.

The second big issue was free speech on campus. Many presidents on both sides challenged people prohibiting patriotism, people prohibiting contested opinions about what was going on. It's amazing how stressful, ultimately, that is. Faculty autonomy and even student autonomy in relation to free speech remains, perhaps, the single most challenging thing college presidents face, because that's when the greater public get involved and that's when legislative concerns about the so-called bias of our universities becomes so extreme. That, too, was a great challenge.

Crisis management. Normally, crisis management is sustained because of some unique event on one campus. For the first time we now recognize that not only must we manage our resources, not only must we have strategic plans to deal with diminished or changing resources, but we must now know how to deal with a crisis, as a more generic issue.

Of course, on our campuses, we have people who tell other people how to do that, but we never think of ourselves as an organizational culture that might need the same management help.

Finally, the fourth area is the issue of international knowledge, that is, the intellectual capital of our campuses, whether it be in languages or knowledge of an area of the world about which the public and perhaps our politicians knew very little. We need to know how to guide people to those resources.

This was a very positive thing, to recognize that there was still intellectual capital out there that simply had to be exhumed and connected with those who wanted it. That is really what I've been doing for the last four weeks, rather than trying to run an association with a 5-10 year agenda.

I would like to say a little bit about the need for social science to approach higher education as a major institution. We obviously study all other institutions with great care, with often a great deal of criticism, occasionally a little appreciation. But one of the lead industries of the 21st century will be the knowledge industry, of which we're a key part. In a way, we're the oil wells or the coal mines of the 21st century. We're going to attract a lot more attention because we are, in a sense, a location of that intellectual capital. But we ourselves, the institutions of higher education, don't do a great deal of research on higher education. We leave it to the schools of education or to those moral philosophers that write for newspapers.

There is not, outside of the schools of education, a great deal of careful study of higher education, as if it were the banking industry or the high-tech industry or whatever. So the kind of scholarly, scientific, data-driven generalizations that we might have for many other institutions in our society is actually quite weak for higher education.

Of course, the changes that are being described, also as topics of the social science arena, are changes in our demography – the fact that the last decade saw more immigrants arrive in this country than any decade previously (not percentage-wise but absolutely); the fact that we have a change in life cycle career trajectories; the fact that we have transformations of communication, information, and perhaps even how learning occurs through technology. With all of those being studied as processes, they're having very rapid impacts on our own institutions.

My belief is we don't study ourselves because we have not, for the last 50 years, thought of the university as an organic whole or something that has any institutional characteristic. It's being viewed as a segmented university, it's the "multi-university" of Clark Kerr. I would have called it the segmented university, that is, with lots of different interests just simply thrown together, and as long as there was enough revenue flowing in, the new money was allocated out to those different interests.

Only beginning in the late '80s and '90s did that money not arrive in large enough amounts for the new money, only, to be allocated only to those interests. So presidents of the '90s had to seriously reallocate the actual base budget rather than reallocate new funding. That was a fundamental change in the principle by which the post-war university was running, one that's been greatly underestimated.

Why, then, have we survived? We survived because the money didn't come from the old sources. They were federal and they were private and we've seen an absolute sustained rise in biology and the biological sciences in this decade.

Why has that happened? Part of it has been money, but part of it has also been a serious reevaluation of the intellectual framework under which the disciplines were conducting themselves, and something which I think the social sciences had to evaluate if they want to attack problems rather than pursue disciplines.

The University of California-Berkeley, where there is no medical school nor an agricultural school, where therefore the basic biological sciences needed a place in the universe within the broad context, let's say, of the liberal arts and sciences, and they needed new facilities. That was only made possible by a literal collapse of the boundaries of the pre-existing disciplines. Redefining biology as micro and macro, as cell biology and ecology, not the actual form that was being studied. The new building that they received and the distinction of that College of Biology was made possible by having 800 biologists together solving problems, rather than being separated into botany, zoology, and something like 28 disciplines that used to comprise the biological sciences.

The 20th century has been the century of disciplines, and it served us well. I think the new century will require some architectural reconfiguration of the intellectual division of labor. I'm not prophetic enough to know how to do that, but I do think things are going on between engineering and the physical sciences which, again, has an applied and a pure basis. I think what's gone on in biology is the immediate application of their findings; in the information sciences, I see the almost immediate application of findings. That luxurious distinction we used to make between pure and applied knowledge was, in fact, being destroyed in the last decade. While there may be people out there doing "pure research," there are many other people equally bright who can transfer knowledge very dramatically and very rapidly.

The idea of something being pure and something being applied is another of those distinctions which we may have to reevaluate on the basis of what went on in the information sciences, what went on in biology, and what currently is going on in international studies and geopolitics and, perhaps, what has been going on for a long time in public policy studies. These areas were often viewed as secondary rather than primary to the mission of the social sciences. It's going to be very important that we balance out those priorities and take a little advice from other sectors of the intellectual division of labor.

In this 20th anniversary document that we have, we're looking at health, justice, education, agriculture, international studies, and so on – how can we demonstrate how as disciplines we can attack these problems? We may have to also come at it by saying, Is the division of labor which is designed by our professional societies and departments perhaps an obsolete structure now designed primarily for management reasons rather than for the fiery intellectual purpose that your agenda really suggests? This is a tall order.

Looking at the intellectual history of other parts of our campus – in the humanities, I think, the collapse has not had the same outcome. In fact it has caused a narrowing of interests rather than a broadening of interests.

So what has happened in the last decade? I started by saying it's been a revenue challenge, a financial issue. I will end by saying, "Being a little more reflective about what we do – educating young minds (not always that young anymore) and advancing knowledge – may in fact be the real change." Change is going on right around us, but it's not being very well documented because we have lived in our silos and our mine shafts, and we're not as aware of what may be going on, on campus and elsewhere.

I am convinced that we need to check the '90s out right across the campus, try to see whether there are cross-disciplinary events of great value, organizationally, so that the kinds of agenda that's being suggested here may be more easily and effectively fulfilled by the social sciences.

I recently introduced a new program at UW-Madison just before I left, known as a cluster appointment process. I encouraged departments to get together to hire 4-5 people or more. Departments could hire a quartet of people with an interest in a common problem, but all four programs would at least get one position. I managed to find a pilot

program with 20 positions. We gave very, very short deadlines, for which I was excoriated by many people who like a lot of time to make applications even though there was money on the table. Of course, for those 20 positions, which were going to be five clusters of four, ninety percent of the proposals came from the biological sciences, five percent came from the physical sciences, and five came from the social sciences and humanities. I was told this was due to different gestation periods that were operating, and the biologists were all ready to go.

Then we managed to get some private funding and some public funding to put 125 positions on the table. Now, there was serious money on the table. The proportions changed, but not dramatically. Of the next round, 75 percent came from the biological sciences, 20 percent from the physical sciences, and 5 percent from the humanities and social studies.

This is a great university with great social science departments and I'm happy to say, by the third round, they got it. Finally, there started to be a real balance of proposals coming from the social sciences. It was because we intellectually did not have the vision to respond to this, even though the chancellor was a geographer. My motives were clearly to balance out the universities' budget by putting something on the table that would be very attractive to social scientists. It is now, but it took a little time.

It's just an example of some of the challenges we face, and by observing what is going on in the broadest architecture of the university – for example, how the intellectual division of labor, the system of disciplines – which is a creation of the Progressive era. It is not necessary to abolish disciplines, but perhaps we should create some federal structures that facilitate the collaboration amongst us and take advantage of other events in other parts of our campuses.

I might mention that my own role in ACE is to build alliances among universities. The value system of most universities is competitive autonomy. Autonomy is the thing we value most and our competitive edge against other autonomous institutions.

Ten years from now those of us who are really outstanding will be autonomous networks of institutions, not autonomous campuses. Not like systems at the state level, but combinations of different kinds of institutions connected by new technology serving needs in a way that will allow some institutions to survive whose mission can no longer be broad, and needs to be narrowed, but that narrow mission is valued by a larger institution.

We've got some of these events developing which I can call loosely alliances of institutions, a kind of weak federalism that makes possible the delivery of something which cannot be delivered by the rules of the 20th century, because the entire mission of the institution is being undermined by changes in the political economy and by changes in the faculty marketplace.

I think those two areas, far important than mere money, are societal processes, are processes going on culturally within higher education worthy of further study. I'm now involved in the macro view of where higher education sits with respect to public policy, Congressional opinion, and trying to create alliances amongst a variety of different institutions to cope with those challenges. It is a moving target because the product itself or the outcome, higher education, is itself changing.

What I'm saying now is, You are the group that probably has the most acute understanding – in terms of methodology, in terms of data, and in terms of point of view – to look at higher education as an institution. Be an ethnographer, look at something that we ourselves are now participants in.

We may decide not to change. We may decide that the way we are organized is just fine, but I do believe there are some opportunities for recombining ourselves so that we can attack the very problems that are indicated in this 20-year review. With that, I will say, I am delighted to be here. This is a grand reunion. I wish I could stay for the day, but anthrax calls me, not in my lungs, but our campuses are places where lots of it exists, with very poor security. That is my challenge of the day. Thanks very much.

Q & A

ABLER: David has graciously consented to take a question or two.

PARTICIPANT: Perhaps you see technology as the biggest driving force for universities. Maybe I could ask you to elaborate especially since one thought might be the whole proprietary nature of instruction might arise.

WARD: I think that's why we need to look at it. I think technology has changed communication and information on campuses. It's still unclear whether it's transforming learning. In proprietary universities, it has transformed learning systemically, not just the odd instructor. So the University of Phoenix represents a new technological order and a new cultural order in how learning is being delivered. Thank God it's to a community of learners different than we currently deal with in the main. To underestimate that would be serious on our part. The challenge we face is how to take that technology in relation to a not-for-profit or public culture and see whether we can change and cope with that.

The technology is a neutral facilitator in many ways and it's really our own vision of our own organizational culture that will be far more important. It's also key that I don't think technology necessarily will transform learning unless we have an active philosophy of how we want to do that.

Do we want to teach languages differently by immersion rather than three credits a crack every semester, and then people don't know very much after four years unless they have an aptitude. That could be something where we say, "We don't do that very well. Technology could be useful, but we would have to rethink organizationally." Do you have all majors in a language take the first semester of their sophomore year and just do the language and forget everything else for awhile? We would have to rethink how we want to deliver that knowledge. The technology alone won't provide that means. There has got to be a change in the systems that we use to do that.

I do think that without some rethinking of the internal boundaries of the segmented university, technology will not have a breakthrough. There are too many barriers to its cross-coding capacity. Technology is simply a facilitator and without the right organizational vision, it won't have a great effect. The places it has had a great effect are those which have culturally changed to allow that effect to occur.

PARTICIPANT: [Off-mike]

WARD: This is a great question and the one which I think has the beginnings of the answer. For the last 30 years, many of our centers, because of the departmental culture and the segmented culture, have been forced to be subdisciplinary rather than multidisciplinary or hyperdisciplinary in order to survive and find the resources.

One of the things is to try to build something which could be genuinely cross-disciplinary. That was my idea of giving these cluster appointments to departments, so that the departments wouldn't feel threatened by their presence. I think we've got to try to allow the centers to culturally transform themselves into institutes that are seen as much more important to the campus' future because they are attacking problems and that's important.

Some way or other, we've got to find a political economy which makes the fights between departments and institutes and centers irrelevant and in particular allow institutes to develop a curricula that is going to allow us to have the kinds of freshman courses that will have a freshness for the century that I think many of our potpourri freshman courses don't have right now.

I think the answers lie in these entities, if they can only reform themselves, and at the same, the host culture has to reform itself. It's a two-way street. That's hard to do because I tried it. But that's, I think, where the answer lies. Thanks.

Acknowledgements

Howard Silver, Consortium of Social Science Associations: Since this is my first time at the podium, let me welcome you all and thank you all for coming and putting up with the incredible uncertainty that's gone on in the last seven weeks, and particularly for us, in the last week and a half. I'm glad you all found yourselves here this morning.

I wanted to take this opportunity to acknowledge a few people who have come back to COSSA after some time away. We have our very first president, Dell Hymes, who has come up from the University of Virginia and we're glad to see him. We have another former president who gets the award for traveling the farthest to be here, Joe Grimes, who came all the way from Hawaii.

And a third former president who has given more COSSA Congressional briefings and annual meeting talks than anyone else, Al Blumstein, from Carnegie-Mellon. I'd also like to welcome back a former chair of the COSSA executive committee, Bill D'Antonio, who ran sociology for many years. And I thank the rest of you for coming as well.

All of you should have received a copy of *Fostering Human Progress – The Social and Behavioral Science Research Contributions to Policy*. This was a major effort on behalf of COSSA to try to focus on what we viewed as seven societal goals or problems that the social and behavioral sciences have examined, researched, written about, and briefed and given advice to policy makers.

I was responsible for compiling, writing, and editing, and received some help from Will Lepkowski, but this was a major undertaking and I had enormous amounts of help and encouragement from the COSSA Executive Committee, the 20th anniversary subcommittee co-chaired by Catherine Rudder and Margaret Reynolds, and from a whole host of people who provided papers, reviewed sections, and examined parts of the document, and from some who examined the whole document in draft form.

It was a major collaborative undertaking and I appreciate all the help that I received. We are going to spend the rest of the morning on the seven societal goals or problems, and hear from a number of people, some of whom wrote some pieces for the document and others who had absolutely nothing to do with putting the document together.

I told them that they can speak to the document or simply give their own take on the goals and problems. So without taking up any more time, let me introduce the speakers.

The Contributions of Social and Behavioral Science (Panel 1)

SILVER: Our first speaker who will focus on the international affairs part of creating a safer world is Stephen Krasner, Graham H. Stuart Professor of International Relations at Stanford University. He is currently a visiting member of the Policy Planning Staff at the State Department.

Most importantly he was the teacher at Stanford of my son Mark who is sitting over there and who is our official photographer at the moment. Steve taught him international relations and Mark became an international relations major.

Creating a Safer World: International Affairs

Stephen Krasner, Stanford University: Thanks for the introduction. I'm glad you mentioned my relationship with your son since it may be the most significant reason for my being here. I have to say, first, since I am actually a U.S. Government employee, that the opinions that I'm giving are my own and not those of the State Department.

I'm going to give you my personal reflections of what I've learned and thought about in the couple of months since I've been here in Washington. I have to say what I've seen in these couple of months hasn't changed the view that I had before, which was that the relationship between the academic study of foreign policy and international relations in actual policy is pretty attenuated.

If you want to be serious and honest about what the ties are, they're not easy and obvious. I think if you look, and this is something I think we can say with some confidence in terms of the empirical data, there is no institutionalized relationship between international relations or political science at large and the federal government.

There is no equivalent to economics and the Council of Economic Advisors or even between lawyers in various legal affairs offices. It's also clear that people in government are not anxiously awaiting the next issue of the *American Political Science Review*. We have to say honestly that if this stuff were really useful for policymakers, they would have institutionalized their relationship with it.

They have a strong interest in getting things right or knowing as much as they can. That these institutional relationships don't exist, says not that international relations and the study of foreign policy has nothing to say, but simply that the way in which it speaks to foreign policy makers is not direct.

The simplest reason for this is the following: social science, where it makes its biggest and greatest contribution to public policy, establishes cause and effect relationships – that's simple-minded, but I think true – cause and effect relationships which were not obvious to policymakers. That's something that one social science – economics – does very well. It has clear findings, and these findings are not things that most policymakers would be able to arrive at themselves.

By and large, international relations and foreign policy don't have that. I want to suggest at least five reasons why the relationship is problematic or distant or attenuated. First, many of the most important issues that foreign policymakers face are either unique or don't belong to any clear class of events.

There is an excellent report by Nicholas Lemann in the last *New Yorker* about terrorism in which among others, he refers to the work of my colleagues, David Laitin and Jim Firoch, and he also speaks about the attitudes of some people in the foreign policy establishment, like at the RAND Corporation.

The question is this, if you look at the findings, the people that he refers to are studying different things. Some are studying terrorist groups; my colleagues are studying civil wars. If you ask what September 11th is an incident of, isn't it something that we should compare, for instance, with Bolshevik anarchism or Bolshevism or anarchism in the past? Is it a manifestation of civil wars? Is it a reflection of a clash with civilization? There is nothing out there in international relations and foreign policy that would tell you which of those class of events it belongs to.

These theories are not bad theories and the arguments are not bad arguments. But if you're thinking about Colin Powell making a decision tomorrow about what the next stage in the campaign on terrorism should be, no one would be able to go to him with confidence and say, look, here is an absolutely clear finding, this is what September 11th reflects, here is what we know about it, here is what the empirical data says, here is what our confidence levels are. There is nothing out there that would give us that level of confidence. It's not clear what class of events some foreign issues belong to.

Secondly, the findings that we do have are not robust. The best finding we have is that democracies don't fight each other. It's really good and it did really make a difference. You can see that the Clinton Administration wanted to use this, it is a very persuasive empirical finding. It's the best thing we have in international relations. But there really isn't anything I would say that is as good empirically as this democratic peace argument.

The third reason why most findings are not robust and theories turn out not to be very useful is because a lot of the theories that are out there are contradictory. In fact, contradictory theories are, for us academics, our bread and butter. In a political science, that's usually what we argue about.

My portfolio in policy planning deals with democracy and human rights issues. I'm now really trying to think seriously about what kind of policy the United States might follow in the promotion of democracy especially after September 11th. Basically, there are two sets of arguments out there in the literature. One is the older argument reflected in the work of Lipset and more recently Jaworski which essentially says you can't consolidate democracy unless you have some reasonable level of per capita income. You might have democracy kind of occur, but you're not going to be able to consolidate it without some reasonable level of economic development.

The second set of arguments is much more open and puts much more weight on institutional choice and on political leadership. Larry Diamond, who is also a colleague of mine at Stanford, is one of the leading scholars in this area. One of the points that he's made most forcefully is that commitment to democratic values on the part of an elite can make an enormous difference in the promotion of democracy.

Those are two opposite findings. One says, if you don't really get economic development, you're not going anywhere or at least not very far. The other says that you might be able to get pretty far even at lower levels of economic development.

There is no decisive empirical data here. This is the typical problem even for me as a social scientist – saying I'm really going to try to see what these findings look like and see if I can come up with some policy that's more coherent for how the U.S. should go about democracy promotion in the contemporary era. That's a third reason.

The fourth reason you can see utterly clearly. I sit on Policy Planning for the State Department, which is supposed to think ahead three weeks as opposed to two days. So much hinges on specific pieces of empirical information. Policy Planning has been actively involved in thinking about what to do in Afghanistan, and my boss (something you never have to say in academia), Richard Haas, is a key player here. The simplest question is: Are we actually going to find Osama bin Laden?

How this whole thing turns out is important for whether we'll be able to identify the leaders of the Taliban and Al Qaeda and get them or not. If, in a year, we haven't killed any of these guys or captured them, it will be a bad thing. If we have actually killed and captured many of them, it will be a good thing.

There's no social science that's going to help us on that. Even if you look a little more widely at the question of what to do in Afghanistan now, a critical issue is whether we will be able to form some kind of coalition government. I think the policy that the U.S. government has followed – that we need a broad coalition – is right and pretty clear.

Is there anyone who can say with confidence that we know we will be able to get that? I would say probably not, and if there is, it's someone who would rely and depend on what we used to call area studies, a disparaging term by and large in the social sciences. It would require someone with very detailed knowledge of different groups in Afghanistan.

Finally, a fifth reason why I think it's hard to translate academic findings into public policy is because public policy depends on much more than academic findings. One reference in COSSA's 20th anniversary book that I think is absolutely compelling in terms of social science findings is that sanctions don't work very well in terms of reaching foreign policy objectives. There is very good empirical research on this. We went nuts on sanctions in the 1990s. We passed things like the International Religious Freedom Act, under which a country gets classified in a category of special concern. We sanction them. We denied military aid to countries where there were coups, like, for instance, Pakistan; we're now running away from those sanctions as fast as we can.

It's clear enough why sanctions happened. There are domestic political groups that are really concerned with these issues, to whom such sanctions are appealing and attractive. There is not a way in which a social science findings can enter in a compelling way into Congressional discussions of these issues.

There are many reasons why social sciences or international relations findings haven't had much of an impact on foreign policy in a direct way. But there are three ways in which social science really does matter. One is referred to in COSSA's report and it really is compelling. And that is that many decision makers have academic training. Within

the current administration, there are only seven people making the critical, important foreign policy decisions to the American government and two of them, [Condoleezza] Rice and [Paul] Wolfowitz, have political science Ph.D.s. In the last administration, half of the assistant secretaries of state had advance training in political science.

It's clear that thinking as a social scientist does matter even in this very confused world which you confront as a policymaker. It's not the only way to think, but it certainly helps to be able to think in terms of propositions and evidence. That's one way in which social science matters.

A second way (though I don't have empirical examples for this yet – although if my year is successful, I will) is found in Jim March's garbage can model. You're often in situations in which policy is the result of the fortuitous convergence of a problem or solution and an individual. I'm thinking about what I'm doing this year [at the State Department]. A foreign service officer wouldn't be able to do and it is purely haphazard that I am in this position. So social science can work in this kind of garbage can model.

Finally, if you look at the way in which social science is most compelling – and this is an argument which I'm lifting entirely from John Steinbruner, who used to be at Brookings and is now at the University of Maryland – social science does often provide ex-ante-rationalizations for policies which policymakers have stumbled upon and begun to implement, but not been able to fully rationalize.

Steinbruner argues that this is even true for the one policy that we thought we could really say changed things – deterrence theory. The standard view was that deterrence theory developed at random in the late 1950s and really altered American foreign policy. Steinbruner looked at this and his argument is that many of the policies that U.S. was implementing in the 1950s already reflected deterrence theory, but policymakers didn't have a name for it. But giving it a name, and rationalizing it, did make it more systematic in the future. If you look at this process, I think it's also true for the democratic peace theory in the Clinton Administration.

I do think that if you look at this process of rationalization, if you look at social science training, if you look at the way in which the garbage can model might work, it demonstrates that what we study in academia or social science in the field of international relations and foreign policy is not irrelevant. I think it is relevant and it has probably made our policies better, but I wonder if it's a chimera to hope that it will have the kind of systematic effect which we find in some other areas of the social sciences like economics.

SILVER: Thanks, Steve. There is a lot to chew on there. We will have questions at the end of the panel. I also want to note that if I remember correctly, having read some of his co-authored articles, Dick Cheney has a background in political science. He's an ABD and wound up at Wisconsin.

Creating a Safer World: Reducing Crime

SILVER: Our next speaker is Sally Hillsman, who is the Deputy Director of the National Institute of Justice. I have the good pleasure of meeting with Sally about once a month to talk about social science and crime policy and how the National Institute of Justice is helping to keep social scientists and criminologists funded and keep crime going down.

Sally Hillsman, National Institute of Justice: Thank you, Howard. It's a pleasure to be here with you to share some of my personal reflections on the role of the social and behavioral sciences in influencing public policies toward a very important goal – that is, reducing crime.

I think we're all especially sensitive about our efforts to produce a safer world in the aftermath of September 11th. It's important to note that this event came at a time when we in the United States have been experiencing an unprecedented decline in violent crime for the better part of a decade.

I want to focus my remarks on crime in America, particularly street crime and the contribution made by the social sciences. I'll return later to the issue of international crime and terrorism when I offer some thoughts about

future social science research on crime and violence. I might add that my remarks here are entirely my own and don't necessarily reflect the opinion of the National Institute or the Justice Department.

It's a testament to the work of scholars in the social and behavioral sciences and to the close engagement many of them have had over the last years with practitioners that our empirically-based knowledge about criminal behavior, crime prevention, crime control, and the administration of justice is remarkably rich and increasingly robust.

That's actually a more significant statement than it might sound when you contrast it with the conclusion a mere 35 years ago by the President's Commission on Law Enforcement and Criminal Justice that "the revolution of scientific discovery has largely bypassed the problems of crime and crime control."

It is important to note in this context that the government's primary responsibility for tackling crime and community safety in the U.S., though not necessarily elsewhere in the world, has always rested with state and local and not federal police, prosecutors, judges, and correctional leaders.

This is as true today as it has been throughout our history as a nation. One consequence of this particular state-federal relationship in crime policy and crime control practice has been that the rather limited investment made by the federal government in research and development in this area since it first began such funding 35 years ago.

Prior to the 1970s American social scientists studied delinquent behavior and some other aspects of deviancy, but we knew little scientifically about criminal careers, the effectiveness in controlling crime of social control agents (police, prosecution, sentencing and so forth), or the impact on crime of informal social control mechanisms in neighborhoods and communities. The National Academy of Sciences did not develop a standing committee on crime and justice research until 1975, shortly after the federal government created the first science agency with social science research funding devoted exclusively to these issues, the predecessor of the National Institute of Justice.

Thirty-five years of social science research and evaluation, however, have made a tremendous difference in our understanding of crime, the justice system, and the effectiveness of strategies of crime control and prevention. Just scan the 23 volumes of the University of Chicago Press *Crime and Justice* series, the NIJ's Congressionally mandated *Assessment of Programs to Prevent Crime*, the University of Maryland's book, *Preventing Crime*, or a parallel book that the British government has just published.

In making societies safer, the social and behavioral sciences have had a major impact over the last 30 years on improving our ability to explain the developmental, social, and other factors in anti-social and criminal behavior and its desistence. Among landmarks are a number of longitudinal studies on the causes and correlates of serious juvenile offending, birth cohort studies, and the longitudinal, multi-level study of individuals, families and neighborhoods by the Project on Human Development in Chicago Neighborhoods led by Tony Earls.

Research such as that by Cathy Spatz Widom on the causal links between child abuse and neglect and later violence has had a significant policy impact by focusing our attention on early intervention with particularly vulnerable children and youths to reduce future crime.

But the advances of social science research have also recently reminded us that interventions with *individuals* are not the only potentially effective direction for reducing crime. Sampson and Raudenbusch's work in the Chicago Project which I just mentioned has reinvigorated earlier, but less methodologically sophisticated, research measuring the impact of *neighborhood* characteristics on reducing violence, demonstrating powerful effects of community-level factors that are independent of race and class. These findings are offering important challenges to public policies that are widely accepted today about the role of public disorder and "broken windows" in combating crime at the neighborhood level.

More recently, social scientists' understanding about criminal behavior has taken another direction as a result of the migration into criminology of research techniques developed by geographers. Analysis of crime using geographic information systems are enabling criminologists to understand crime events as they occur over time in specific locations. Researchers have developed statistical models of the spatial patterns of serial killers' behaviors that have

allowed police to narrow the search range for suspects sufficiently to increase the likelihood of a killer's apprehension. Spatial analysis tools have also allowed social scientists to develop predictive models of crime events to identify the locations of future hot spots of criminal activity in order to increase the effective deployment of prevention and law enforcement resources. Since the 1980s, therefore, the concept of the criminal career of *individuals* has been joined by the concept of the criminal career *places*.

In seeking to make society safer over the last three decades, the social sciences have also played a significant role in evaluating and improving the operation of the criminal justice system. I want to emphasize that this is more than just "what works." It is the slow process of integrating the analytic and evaluative tools of the social sciences into the operational perspectives of criminal justice agencies.

The first place this happened was with the police, what George Kelling has referred to as the "quiet revolution" that has reshaped American policing. Police organizations, like many other criminal justice agencies, are functionally resistant to the involvement of outsiders. Social scientists have been the quintessential outsiders. This began to change, however, in the 1970s as a more sophisticated police leadership began to question the impact of long-term policing practices on crime and on fear of crime. Kelling's "quiet revolution" began with a controlled experiment in Kansas City, Missouri that tested the conventional wisdom of police executives about crime control effectiveness of neighborhood preventive patrol. The conventional wisdom began to collapse under the weight of experimental findings as did other accumulated wisdoms about police practices as policing research and evaluation increased in the 1970s and 1980s.

The impact of this program of research on public policies at all levels of government, including federal legislation to provide billions of dollars for community-oriented policing, is clear. Traditional policing practices have slowly been replaced by more proactive, problem solving and prevention-focused approaches to policing, policies that are now facing the same scrutiny from social scientists as did earlier wisdoms.

What is significantly different today, however, as David Weisberg has noted, is that these early studies and the subsequent development of more sophisticated, research-based approaches to policing "has made evaluation an important part of decision making in major police agencies." This same pattern can now be found increasingly in other parts of the criminal justice system, including courts, corrections, and prosecution.

The response of the entire criminal justice system to spousal assault began to change as the result of a controlled experiment in Minneapolis and subsequent replications during the 1980s. Until then, physical violence among intimates was considered more a family matter than a crime. While police responded, they rarely arrested the suspect. This program of experimental research indicated that a more active role by police reduced revictimization under at least some circumstances. The increasing power of victim advocates and their focus on violence against women during this period coalesced with emerging research findings, and the 1994 Violence Against Women Act included strong pro-arrest policies and funding.

There are numerous other examples of processes and procedures in today's criminal justice system that Americans take for granted to promote safety for our communities that are, in fact, the relatively recent results of social science experimentation and research. For example, the routine release of large numbers of poor defendants on their personal recognizance in lieu of money bail or pretrial detention while awaiting trial is the result of a controlled experiment conducted in New York City in the 1960s. The subsequent development by researchers of valid and reliable risk-of-flight and risk-of-rearrest scales that could be used by judges to make safe, pretrial release decisions resulted in federal bail reform legislation and parallel legislation in the states. Most courts in America today use some version of the process that was developed, designed, and pilot tested by researchers in New York and replicated throughout the country.

Social scientists' introduction of urinalysis as a validation measure for self-reported drug use in early studies of arrestees identified a far higher level of polydrug use among offenders than had been previously recognized. This led to policies, including those in the federal government's National Drug Control Strategy, supporting the expansion of drug treatment and drug use monitoring by court and correctional authorities when offenders are released into our communities.

The development of strategies to ensure that child crime victims are interviewed only once, rather than repeatedly, by different criminal justice authorities in order to avoid their revictimization and to increase the reliability of their recollections, and the use of video court appearances by child victims to provide reliable testimony while also protecting the defendant's rights to confront his accuser, are now routine and accepted practices in the American legal system that are based on the findings of social science research.

I think it's important at this point to draw some distinctions between the impact of the behavioral and social sciences on the *performance* of the criminal justice system, on public *policy* about crime control, and on crime itself. To me, there is little doubt that the social sciences have had a significant, and perhaps profound, impact on the operations and performance of all aspects of the criminal justice system in the United States over the last several years, and that criminal justice leaders have engaged in often successful innovation and adaptation as a result of their increasing close collaboration with social scientists. But does this mean we have influenced public policy?

At one very important level, yes. As I noted earlier, 95 percent of the crime and justice business in the United States is conducted at the state and local level, and most of it at the local level. In a very important sense then, criminal justice leaders such as police chiefs and their mayors, elected sheriffs and prosecutors, judges, probation, parole, and correction leaders *are* crime policy makers in the United States. What they do is, in effect, what much of public policy about crime is in the United States.

We can see this from research on state sentencing laws such as "three strikes." Research shows that the real impact of this legislative policy is dependent on what individual prosecutors do in implementing or not implementing the statutes. The prosecutors, as independently elected local officials, vary enormously on how they do this. This reflects the reality of crime in the United States. There is no one drug or one violence problem. There is one in Boston and there's another in San Diego. There's one in Manhattan and there's another in Brooklyn.

Yet, it remains difficult to dispute Al Blumstein's contention that "it cannot yet be claimed . . . that . . . [research and evaluation] results and insights have been major influences on the formulation of crime control policy." His explanation for this focuses on the role of politics. "With a strong political influence in the formulation of crime policy [in the United States], research has contributed less to the substantive formulation of policy than the base of knowledge warrants."

This would be hard to dispute, although there are certainly some examples of research that have begun to refocus at least some aspects of federal crime policy. For example, the historical preference in the U.S. for punishment in drug-related crimes has resulted in additional emphasis in federal legislation on the role of drug treatment. This is a direct result of research findings on the effectiveness and cost effectiveness of correctional drug treatment. I heard *New York Times*' correspondent Fox Butterfield say not long ago that nowhere in the current public policy debate is there as great a disjuncture between researchers and practitioners on the one hand, and the public and political rhetoric on the other, as in the field of crime and criminal justice.

One of the many paradoxes is that while policymakers often talk in sweeping terms about the globalization of crime, when criminal justice leaders in the United States act effectively, they do so largely by recognizing that all crime is local. It is at the base of the crime pyramid – in neighborhoods and communities – where the public experiences the problems of crime and disorder and where the bulk of law enforcement and prevention take place. The rhetoric of public policy debate, therefore, is often out of sync with the reality of crime that practitioners face and that social scientists study in the shifting social, demographic, and economic conditions that cause neighborhoods to change and hot spots of criminal activity to emerge or decline.

This still leaves open the question of whether social science research has contributed to the reduction of crime. This is a hard question to answer although it is an important one. Social science has greatly contributed to the knowledge about what works to reduce crime, but knowing "what works" and making it work are two different things.

Communities in the U.S. are becoming more results-oriented. This has made criminal justice leaders more attune to the "what works" literature of the social sciences. It has also opened the door to social scientists who want to work directly with them to reduce crime.

There is widespread public demand for more effective government and this increases the accountability of local public officials and keeps policymakers on the hook for changing strategies that don't work into ones that do. To succeed, however, they need routine, real-time, multi-faceted, community-based (small-area) data and highly skilled social scientists who can use those data to help them understand with considerable specificity the problems facing a particular neighborhood or community. That is, all crime is local. We need to study it where it happens.

Both of these – community based data and social science partners – remain in short supply. Yet, there is a growing number of examples of the use of social science research to develop systematic, data-driven evidence to understand crime and violence and to guide problem-solving strategies to reduce crime and violence at the local level.

One is the NIJ-sponsored work of David Kennedy and his colleagues at the Kennedy School of Government and the Boston Police Department. The researchers use spatial analysis to understand the nature and location of juvenile gun homicide in Boston and provide strategic feedback to the police department and community stakeholders on the impact of the strategies they were testing to reduce gun homicides by juveniles.

In a little more than a year, a social scientist's rather traditional study of gun markets became a neighborhood-based examination of youth gangs and then a broad-based collaborative strategy for problem solving (and, incidentally, a test of deterrence theory) that virtually eliminated kids killing kids in the central gang neighborhoods of Boston.

Kennedy's role as the researcher became the more complex role of the research partner that reduced Boston's high youth homicide rate to zero. This strategy for using social science research (and researchers) to directly impact crime is not new, but it is growing. In the coming years, all 94 U.S. Attorney's offices will be tackling gun crime using variations of this strategy.

As I have indicated, behavioral and social science research and researchers have made tremendous contributions to tackling the problems of crime, safety, and justice in the United States over the last 30 years through both basic and applied research; but there is a very long way to go. Among the many directions for research in the near future that I could mention in closing, I'd like to touch quickly on three.

First, it's my personal opinion that we need substantially more research-based knowledge about community-level variations in crime rates. Only then will we be able to explain why some communities are more resistant to crime – especially violent crime – than others despite similar social characteristics such as poverty. I believe this will only come with the further development of the types of sophisticated community measurement techniques pioneered by Sampson and Raudenbusch and with the development of better community-level data.

There are insufficient sources of data on a wide enough set of common community-level factors currently available in American cities for social scientists over the last decade to have robustly tested the different hypotheses about the causes of falling crime rates across different American communities. We are certainly not ready to systematically understand rising crime rates in these same communities should they begin to appear in the next decade. I personally believe that we need to build these data sources now by helping communities transform the vast amounts of operational data they collect, but don't use, into integrated information for policymaking and analyzable data for research.

I also think we should encourage the expansion of national small-area data systems that are already under development, such as National Incident-Based Research Statistics (NIBRS) and the Census Bureau's American Community Survey.

Second, I think most researchers in the field of criminology would agree that we need more attention from all the social sciences on transnational crime and criminal organizations. In doing so, I think it is also useful to remember that while many of today's problems such as drug markets in the U.S. are part of complex transnational crime industries, wherever these criminal enterprises and networks drop their roots to do business, it is neighborhoods and communities in the United States that experience and fight these activities as local crime problems. Their problems and efforts should be studied closely in order to understand and reduce transnational criminal activity.

Finally, I suspect all of us would agree that we need more research on both terrorism itself and on counter-terrorism efforts that, again, often take place at the local as well as the national and international level. My colleague Marge Zahn has been scanning the recent American social science literature on violence and reminds me that the vast bulk of it focuses on interpersonal violence, including the very important areas of intrafamilial violence, violence against women, and gun violence. However, she has also pointed out that very little research in the United States is focused on collective violence. I think the events of the last several weeks have led us to understand that this is a very large gap in the work that we all have been doing.

In conclusion, let me just thank you, Howard, for asking me to join you this morning. I also want to repeat that I think the work of the social sciences has made a tremendous difference in the last 30 years. While it has sometimes been very frustrating to recognize what we don't know, the good news is that this leaves much work left for us to do and for future generations of social scientists to contribute to. Thanks very much.

SILVER: Thank you, Sally. I just want to point out that two of the people she mentioned, Al Blumstein and Marge Zahn are in the audience. So if you want to ask them more about what she said, you will have chance.

Improving Health

SILVER: It's now my pleasure to introduce Raynard Kington, who is the Associate Director of the National Institutes of Health for Behavioral and Social Science Research. This also means he is the Director of the Office of Behavioral and Social Science Research, which has done a very important job of helping to infuse social and behavioral science research throughout what Congress always describes as a biomedical research institution (NIH). Most importantly for us, Raynard is also a former COSSA Congressional seminar speaker.

Raynard Kington, National Institutes of Health: Thank you. It's a pleasure to be here this morning. I will say that I am not speaking on behalf of the government. These are personal opinions and not the opinions of NIH.

Let me give you an outline of what I want to talk about. First, one of the great successes of behavioral and social science research – the area of smoking. Second, I'll identify two topics where there have been tremendous advances in the science that truly hold great promise for helping us to improve the health of the public. We're just at the cusp of developing interventions to actually change the world in these two areas.

Finally, I'll discuss two areas that I think are particularly important for the future of social and behavioral science related to health.

Let me start out with our smoking slide. Any behavioral scientist who wants to brag about what we've done has to have a slide like this. This is clearly one area where the behavioral and social sciences can legitimately claim credit for an extraordinary change in American society.

In 1965, 41.9 percent of the population smoked – 51.2 percent of men, and an even higher proportion of physicians. In 1999, the smoking rate dropped to 23.3 percent – 25 percent for men and 22 percent for women. Of course, we will never be fully successful with this campaign until the rate is zero, but I don't think anyone can argue with the extraordinary changes in behavior of this population that have taken place over this period of time.

However, we can't be complacent. There are some bad things on the horizon. For example, probably one of the most concerning pieces of evidence that we've had recently is the evidence suggesting that as Hispanic women acculturate, they actually begin to smoke more. We're seeing this in other ethnic immigrant groups as well – with the acculturation into the American society, the population goes from having a relatively good health profile to a relatively bad health profile.

In spite of our successes, change is looming on the horizon that will really force us to apply some of what we've learned. Probably the first thing we learned with smoking, and this was a painful lesson for us, that just telling people that a behavior is bad for your health isn't enough.

That may seem stunning in retrospect, but a lot of people thought that that was all that would be necessary. We've learned the hard way that that's not the case. We've also learned that interventions really have to be directed at multiple levels. We've distinguished between the challenges of helping a person who is already smoking, for example, quit by using counseling and pharmaceutical therapies to deal with the very real physiologic addiction to nicotine, from interventions that prevent people from smoking at every level to education to counseling by physicians to more societal-level interventions that essentially stigmatize the behavior of smoking.

Now we have the opportunity to apply what we've learned in the area of smoking to new areas where we are now seeing significant problems – in the areas of social support and health, where we're just at the cusp of taking what we found from studies that have highlighted causal pathways and translating them into interventions, and in the area of racial and ethnic disparities in health.

There is an extraordinary amount of research now that confirms social support as an important predictor of a wide array of health outcomes – mortality, health behaviors, cardiovascular reactivity, infectious diseases, recovery from myocardial infarction, and so on. That's just the short list. There are strong relationships between social support and a wide range of health outcomes. We've also seen that the magnitude of this association is not trivial and, in fact, the size of the risk associated with morbidity and mortality for many outcomes is comparable to that associated with other factors for which we've had very intense interventions. These include high blood pressure, obesity, sedentary lifestyles, and smoking.

We're just now beginning to develop the theoretical models for explaining how factors like social support are ultimately translated into health outcomes. I won't go into it now, but there is a model from the University of Pittsburgh Mind Body Center, which is a research center funded jointly by the Office of Behavioral and Social Sciences Research and the National Heart, Lung, and Blood Institute [NHLBI].

Along with developing theoretical models to help us understand what's going on, we're also just now beginning to develop interventions to translate our knowledge of the importance of these factors into interventions. One of the most intensive efforts that is being tested now is the NHLBI Enhancing Recovery in Coronary Heart Disease Patients Study, also known as the ENRICH study.

This is a multi-center randomized clinical trial involving 3,000 patients after a myocardial infarction. There's an intensive psycho-social intervention targeting both depression and low social support, and the outcomes of interest are survival and reinfarction. We expect to have the results from this intervention soon.

It's precisely these types of interventions that are the point of leverage in terms of changing practices in medicine and in public health in this country, and these types of interventions – rigorous trials to assess the interventions dealing with particularly the hard outcomes of death and morbidity – are precisely how we will ultimately have an impact on changing the way medicine is practiced and translating some of the results that we've seen into real interventions.

The second area that we clearly recognize as having great potential for translating what we know into interventions is the broad area of racial and ethnic disparities in health. There are both successes and continuing sources of frustration in the American health system.

On one hand, we've had extraordinary improvements in the health status of every population: men, women, young, old, and every racial and ethnic group between the beginning of the century and the end of the last century. The increase in life expectancy for African Americans more than doubled that for whites (increased by over two-thirds).

In spite of these extraordinary successes, we still have persistent differences across racial and ethnic groups. Those of you who pay attention to the mortality rate may have noticed that within the last year, there were dramatic changes, because the standardized population went from 1940-2000, and the magnitude of the differences magically decreased in one year. All of you know how that happens, but for some people, that was a big surprise. With the 1940 standard, the African-American mortality rate is almost 50 percent higher than that of whites. We also see, under that standard, Hispanics actually having a lower mortality rate than non-Hispanic whites, although by most measures of morbidity and health related quality of life, Hispanics have substantially worse health than whites.

The data for the American-Indian population are much tenuous, although it looks as though the Asian population has a lower mortality rate, but again, it is higher for specific diseases and some measures of health-related quality of life. The American-Indian/Alaska Native population, when corrected for problems with the quality of mortality data, probably have a mortality rate that is higher than that of non-Hispanic whites.

When we look at what accounts for these differences, we see both great potential and continuing challenges for our research community. One of the few studies that actually tried to attribute how much of the differences in mortality are due to various factors found that 38 percent of the black/white differences are due to family income. About one-third are due to six risk factors: smoking, systolic blood pressure, diabetes, cholesterol, body mass index, and alcohol intake. Another third or so are unexplained. When we look at specific risk factors we see sometimes dramatic differences across racial/ethnic groups.

The 1990 National Health Interview Survey found that, of employed adults, both Hispanics and African Americans have substantially higher rates of sedentary lifestyles compared to non-Hispanic whites. The National Health and Nutrition Examination Survey looked at rates of obesity, comparing the NHANES II which took place in the late 1970s to the NHANES III which took place between 1988 and 1994. For women of every group, obesity levels increased substantially. But we really see dramatic differences across racial/ethnic groups within any year. Over 40 percent of Mexican-American women are obese (using the criteria of body mass index greater than 30). African-American women also having substantial rates of obesity – over 35 percent. The challenge now is to translate our knowledge of what’s happening and what might be the causes of the differences across racial/ethnic groups into real interventions specifically targeted to extraordinarily diverse communities with very different health risks, and which may require quite different types of interventions to really improve health outcomes.

Turning to the future, I’ll speak briefly about a growing area of interest in research at NIH – so called positive health. This refers to the study of good health outcomes instead of the usual method of looking at disease and disability.

We know that poor communities have the widest variance in health outcomes (although health outcomes there are in general much worse). We see significant numbers of people who seem to make it through in spite of having bad health risk factors as well as bad community level factors. This presents an opportunity for us to understand why it is that some people seem to do well in spite of having poor risk profiles. We see this as a counterpoint to studying specific illnesses and disease and we see this both at the individual level and at the population level.

Let’s look at infant mortality rate by the mother’s race/ethnicity. Two things are remarkable about these data. One, the non-Hispanic Black rate is more than double that of whites, but we also see that the total Hispanic rate for infant mortality is approximately equal to that of whites in spite of having significantly worse socio-economic status and other risk factors.

These types of population level anomalies present opportunities to try to understand how we can get more people to have better outcomes in spite of their risk profiles that ordinarily strongly predict poor health outcomes.

The next topic echoes what was said earlier this morning. We have an extraordinary need for interdisciplinary research. As was pointed out, the biomedical scientists seem to have gotten it. Across biologic research disciplines, it is very difficult to tell where physiology ends and molecular biology begins and where the various disciplines actually are divided up. It may have taken 20 years of NIH working to achieve that and a strong drive of technology. The behavioral and social scientists don’t seem to have gotten it. I strongly agree with the comments of those who have

pointed out that among the behavioral and social sciences, we really aren't seeing those barriers drop. We certainly aren't seeing those barriers drop between the biomedical sciences and the behavioral and social sciences. Until we figure out a way to lower these barriers, I think they will be substantially slow research progress in behavioral and social sciences towards promoting health.

Two examples of areas that just cry out for interdisciplinary research – and in which we're seeing it – are cardiovascular disease and injury prevention, which are cutting across a wide range of biological, behavioral, and social sciences. There are other areas that are not so obvious.

Perhaps one of the most salient at this point in time is research on response to bioterrorism. I must say that I have been disappointed that there has been basically no contribution of behavioral and social scientists and health in the public dialogue about response to bioterrorism, when we all know that the researchers that will probably make the most important contributions to addressing these issues are those who help us understand how best to inform people about risk, how they should respond to it, and know to do it in such a way that ultimately will help people minimize risk. Perhaps the heat that the Department received about how they responded might encourage their turning to behavioral and social scientists who might be able to help us make better choices in the future.

We're also interested in training new types of scientists. There is a wide range of training programs that we're supporting at all levels to try to generate these researchers who are able to cut across disciplines. We are particularly interested in trying to get scientists who cut across the behavioral and social sciences.

For example, in two days our office and the National Human Genome Research Institute will hold a very interesting meeting where we will bring together 10 of the leading biomedical scientists, molecular biologists, geneticists, and so on, and 10 behavioral and social scientists, in the same room. The goal is to try to map out a strategy for developing the scientific infrastructure that will allow us to finally determine the role of environment (in the equation of genes and environment) in health outcomes. I think it will be a very interesting meeting. What is particularly notable about this meeting is that we tried to think of social scientists who really understood biologic processes and we arrived at a very short list, even with generous criteria for understanding biologic processes – that's a big problem.

Finally, I could not end without making the plea that as we begin to change the composition of our scientific workforce in behavioral and social science and health, we have to continue struggles to increase the diversity of our scientific communities. I hate to bring this up, but look around this room. It's rather striking from up here that the leaders of the scientific community and behavioral and social sciences don't reflect, on the surface, the diversity of our population. If we're seriously to address problems like racial and ethnic disparities in health, we must have a more diverse scientific workforce. As we try to change the type of scientists that we produce, I can only hope that we will also try to maintain efforts to increase diversity. I will end there.

SILVER: Covering the subject of health and behavioral and social sciences in 15 minutes is quite a task and Raynard did a marvelous job.

Promoting Fairness

SILVER: Our final speaker on the first panel is Deborah Jones-Merritt. She is the Director of the John H. Glenn Institute for Public Service and Public Policy at the Ohio State University. She's also a professor at the Ohio State University Law School and a member of the COSSA Board of Directors, and as Ph.D. graduate of Ohio State, I feel deeply honored to have her here.

Deborah Jones-Merritt, John H. Glenn Center for Public Policy: Thanks very much, Howard. It's been brought home to me listening to the introductory remarks of the previous panelists that I'm the token state employee on a panel of three federal employees. I'll say that the State of Ohio has no idea what I'm saying and certainly is not responsible.

On a more serious note, I think it's common to think of all the sciences – whether we think of the social or the natural sciences as abstract intellectual inquiries – as far removed from the field of policy. In fact, sometimes scientists want to define themselves that way and say that it's the applied people who work on policy and the scientists are pure.

I want to argue though that the opposite is really the case. Two things that distinguish all scientists, whether we talk about the natural world or the social world, are the goals of showing that things are not as they appear to the naked eye and formulating theories that will either help us control or change the things in the world around us that we don't like. Those two integral parts of our mission are as important to defining us as a science as the factors of using controlled experimentation or the other sorts of protocols that we talk about. In a sense, in our whole mission, policy is interwoven with the whole definition of who we are as scientists.

Nowhere is this more true than in the field of equality and fairness, the topic that I was asked to talk about. In this field, the influence of social science on policy goes back at least to the beginning of this last century. I'm not going to take you through this year by year, but will visit briefly the year 1908.

In that year, there was a Boston lawyer named Louis Brandeis who submitted a brief to the United States Supreme Court defending a law that limited hours for factory workers. As part of that brief, Brandeis assembled extensive social science evidence showing the negative effects of long working hours. This was a such a distinctive approach to include social science in a court brief that it became known as the "Brandeis Brief," and the phrase is still used today to describe similar court briefs. But we still had a lot to learn about fairness and equality back in 1908.

Brandeis won the case and the court discussed his social science evidence approvingly. It may actually have made a difference in that case. This was a period of great *laissez-faire*, both in the political world and in the courts, which tended to strike down any sorts of restrictions on big business. This is the only case in which the court upheld the restriction on business during that era. So the social science may truly have made a difference there.

Almost 50 years later, the court again relied on social science evidence in what may be our most famous single court decision in the area of equality, *Brown v. Board of Education*. In its footnote 11, the court cited research by Kenneth Clarke, Mamie Clarke, and others showing that segregation hampered the educational development of minority children. That single footnote has spawned a cottage industry debate that continues today about whether it really made a difference.

Did the court really care about the social science evidence? I doubt very much that the court really was affected by the social science evidence in the sense of a single "aha" moment. I think the social scientists' impact on that court decision was much more profound. For more than a decade before *Brown*, social scientists had been gathering evidence and doing a very good job of distributing it to show the negative effects of segregated education on children.

At the same time, legal scholars, who we like to think are a subset of social scientists, were formulating theories to bring to the courts that would incorporate this evidence. These views were permeating learned society in the United States in a way that a single argument in a case never could. The justices heard these arguments. They read about them in the newspapers, they heard about them at cocktail parties. It was becoming part of the intellectual discourse.

Certainly by 1954, it hadn't changed the majority of minds in the United States, but the evidence was out there, and, luckily, it influenced the nine justices who decided *Brown v. Board of Education*. I think that this accumulating evidence affected their view of equality. They could no longer think that schools that were separate were equal when they had been challenged increasingly in the previous 10 years by evidence that showed that the effects on children were really quite different.

Since *Brown*, our society has witnessed tremendous advances in equality as well as continuous contributions by the social sciences to those advances. Let me remind you of just a few of these (though many of you are more familiar than I am with them).

Social sciences taught us that prejudice is endemic and largely unconscious in all fields from employment to, housing, education, and even retail decisions. One of my favorite studies is the one that shows the differences in prices that car dealers will offer to white men, white women, minority men, and minority women. Without realizing that we're doing so, we prefer attractive people, white ones, and in many cases, males, for jobs and for other benefits. Hundreds and hundreds of tests or studies and other types of innovative research have shown that these things are true, that what we perceive with the naked eye – an equal world – is not really a true world. Social science has also taught us some surprising things about the remedies we have adopted.

It has shown us that, yes, affirmative action has worked, that minorities fare better in the United States today than they did 30-40 years ago, but that in most cases, the preferences extended by affirmative action have, in fact, been very small, much less than is perceived by the society around us.

Affirmative action has been necessary to overcome these unconscious biases and to force us to treat women and minority more equally than we would otherwise. We've also learned that affirmative action does not produce special benefits for minority women, that rather than being double beneficiaries of affirmative action, they often are treated worse than either minority men or white women.

We've learned that under some circumstances, instruction to suppress stereotypic thoughts is actually counterproductive and makes people focus more on their biases. Social science has also played a crucial role in demonstrating to us the creativity and productivity of older workers, research that was fundamental in supporting advances like legal protections for the aged.

It has also shown us the tremendous cost to disabled Americans when they are not allowed to work and at the same time has demonstrated that most of the accommodations of the workplace are much cheaper than employers envision when they are first asked to implement them.

Social scientists taught us very surprising things about our political and justice systems on the side of fairness and equality. We've learned that eyewitness testimony is highly unreliable, that jury size can have important impacts on the nature of deliberations and their outcome, that police officers just like the rest of us are affected by unconscious biases so that minority suspects are treated differently than white ones and female victims are treated differently than male ones.

Sometimes many of these lessons begin to seem obvious in part because we have known about many of them for many years now. There is some excellent research done by a colleague of mine at Ohio State that shows that in elections on ballots, simply having your name listed first on the ballot is a tremendous advantage. In fact, the whole Florida debacle is really explained as much by the fact that Bush was listed first on the ballot as anything else. All of the other ballot errors were small compared to that. Other states rotate names on the ballot and Florida did not do that.

Sometimes, social science is needed to make us confront something that we may not really want to take on, even though it should be obvious. Social science has also contributed very substantially to theoretical advances in what fairness and equality mean.

This hasn't simply been a process of knowing what equality is and then having the social science go out and find us some dramatic examples of how we're not living up to our notion. It was really the social scientists looking at these studies and then exploring them on a theoretical basis who produced, for example, a shift in our thinking about gender equality, from an original paradigm where we focused on making woman more like men and treating the women the same way we treat men, to a new paradigm in which we think about how the entire societal structure is based on male patterns of life or male patterns of behavior. That's a theoretical change in how we actually define equality. In the same way, it was social science that gave us the idea of intersectionality – that while we think about race and gender, we also have to think about the intersection of those two and about ways in which minority women may have different experiences than either white women or minority men.

I can't pretend that the policymakers always pay attention. I could actually stand up here and list cases in which the U.S. Supreme Court has ignored terrific social science research or has even gotten it wrong, has cited it the wrong

way, and come to absolutely the wrong result. There are even more instances in which other policymakers, legislators, and regulators have ignored very good social science research on equality and fairness. Unfortunately, unlike the molecules in the natural world, we have a vested interest in the way things are. We can be resistant to recommendations that we should change and we have a human tendency to cling to the fact that we do know how the world is even though the scientists are telling us that it's different.

I still believe that my eyewitness testimony is pretty darn good even though the scientists are telling me that it's not. But that, after all, is why social science is so important in this area. If you think about it, this is really the only force that is combating our assumptions, each of our individual assumptions that what we see from our small corner of the universe is true. It is social science that is able to step back and tell us that's not the way it is. You need to think about the world from a more global perspective.

What then remains to be done in this area? I think it's common to say since September 11th that the question is what does not remain to be done. The challenges now seem so overwhelming and so enormous. We need to understand the personal and social forces that create terrorism, especially the ways that inequality and unfairness may have nurtured those roles in other countries and in our own country as well. (We don't know if this anthrax attack comes from international or domestic sources.) We need to understand how the fear of mass attacks affects people in our society, not only so we can design ways to be safe, but so we can also protect all this progress we've made on equality and fairness. We need to start thinking about how our reactions over the next six, nine, and 12 months may begin to eat away at those protections of equality.

In addition to those overwhelming challenges, I'll mention just four more that I see as the agenda for at least the next 10 years on fairness and equality. The first is to continue what we've been doing for the last 50 years. Almost half a century after *Brown*, empirical studies continue to show marked differences in race outcomes in our country as well as gender differences. We've only begun to scratch the surface of age discrimination, sexual orientation. All of these are areas in which we need to continue to document and bring new theoretical insights into meanings of fairness.

At the same time, we need to shift to some new areas. I think the area of economic inequality in itself is one that needs to become a center stage focus for us. It's been there as part of our studies of race, gender, and these other categories, but economic inequality in itself is one that's a rising concern in our nation as well as in the world.

Third, our aging population is going to raise fairness issues in new contexts. As the population becomes older, we're going to face all sorts of questions already out there. What proportion of our societal resources should be allocated to medical care during the last years or months of life? Who should bear the burdens for the long-term care of the aged and how are our public program and the resources for them going to change as the adult population includes a decreasing percentage of workers and an increasing percentage of those drawing upon the resources?

The global economy was generating fairness issues of its own even before September 11th. Is it fair for countries to reject goods that are produced under conditions that would be illegal in their own nations? Or is that an unfair form of trade discrimination? What if the objectionable conditions arise from deeply rooted religious or cultural differences? Does it make a difference if you're answering that question from our perspective of what we consider the enlightened Western civilization or from the Islamic perspective that would treat women quite differently? What is fair under those conditions of global trade?

The events of September 11th have made many of these questions more poignant, but they really are timeless ones. As during the last 50 years, social scientists will be at the forefront of defining both what we mean by fairness and increasing the conditions that foster it. Let me stop there so we have some time for questions for the whole panel.

Q & A

SILVER: We've covered a lot of issues here, but now you have a chance to ask questions or give your own perspective.

PARTICIPANT: This is for Ms. Hillsman. I've done some work with Larry Sherman, whom I sure you know used to be at Maryland, on the issue of collecting data at the local level – crime data specifically. One of the problems we always ran across was that whether you could get the data was a function how the local police department collected the data. I'm just wondering whether or not NIJ has looked into this and whether or not you see any way of improving data collection efforts at the local level?

HILLSMAN: That's a very important question. One of the things that we know is that much crime data is actually agency or administrative data rather than data that comes in a more epidemiological sense from the initial or real phenomena, although obviously the introduction of victimization surveys in the United States have given us another perspective on the amount and types of crime other than agency data.

I guess I would say there are two important things. One, the federal government cannot tell local police departments what to do. There really isn't that kind of relationship in our federal structure in the U.S. Although it is important that we are now collecting, in some jurisdictions, incident-based data rather than, or in addition to, the traditional FBI data, I think the most important dimension of getting local police departments to collect better data – whether it's the NIBRS data or other kind of data – is to make those data useful to the police departments themselves in their work.

As a result, I think we need to work much harder at the local level to encourage police departments to be using their own data, because as those of you who have used data from any kind of agency know, more people want to use the data they generate themselves. I think that's the direction that we have to go, which is part of the reason I emphasize the issue of problem solving and the development of strategies at the local level.

The second thing I would say is that we have national victimization rates, but the collection of victimization data at the local level is very critical. That's a very tough area because victimization data are difficult in small areas, because they're relatively rare phenomenon and you need large samples. That's very expensive and that's another area we have to pursue.

Once again, the more communities want victimization data for their own purposes (both to know what the nature of the problem is and whether or not the strategies that they've put in place have made a difference), the greater the likelihood that those kinds of data will begin to develop in communities across the U.S. I think researchers can really help encourage the appetite at the local level for those kinds of data.

MERRITT: I just want to add something about another way to collect data, because I think many of you have this capacity at your home institution. We're not the only ones who have done this, but one of the things that the Glenn Institute did in Columbus was to form a partnership with the City of Columbus and the United Way of Franklin County; we've actually incorporated this partnership and we call it Community Research Partners. We've gotten the city to put in about \$150,000 a year and the United Way \$200,000 a year. The university actually only contributes \$35,000 a year because there is an appetite for data among these outside groups and they know they need the university expertise to help them put it together.

This group now has a staff of four and they're trying to integrate the data that agencies are already collecting and collect some of their own, and they're also doing evaluation services for all sorts of human services agencies. It's a wonderful idea that the city came to us with to improve collection of local data. I was delighted to hear how useful it could be once we have it.

PARTICIPANT: I'm pleased to hear each of the panelists address the issue of terrorism. I wonder if you might give us your opinion on what you think is the most effective contribution we could make in the short term in translating knowledge from the behavioral and social sciences to combating terrorism, to confronting it, and to coping with it.

MERRITT: I'll give one very quick one while the others think. To me, for the social scientist, you would be focusing on the reaction of the home population in the U.S. to mass threats like this, how is it that people react when they're faced with something like the September 11th attack or with something somewhat more amorphous like the bioterrorism and chemical sort of attack.

I think our policymakers need to be armed with that information. They need to know how the population is likely to react so they can determine the best ways to implement health preventive measures and to think about how to protect civil liberties in the face of likely reactions.

KINGTON: I'll just repeat what I suggested and that is to figure out how best to provide information about risk for a very diverse population who is receiving information in all sorts of ways. I think that behavioral and social scientists have a great deal to contribute on that front that we really haven't in any organized, cohesive way thus far.

HILLSMAN: In the short term, there are two things related to both of the comments that have just been made. One, we actually do have a history in the U.S. of communities developing activities in response to the possibility of threat, both war-time experiences and Cold War experiences. I think we have not done very much to explore what has been done in the past and what the relevance is today. Second, we really need to take a close look at the way in which federal, state, and local entities are going to be relating to one another over the next couple of years and see what the consequences of that are. There are two ideas for the short-term, but I think there are a lot of long-term things to do as well.

KRASNER: I agree with the previous three speakers, but I guess I should try and address what social science has to tell us about what we can do about terrorists. The answer is we have no idea. We're looking at least three different levels. We do not know what the relationship is among them and we do not know how to systematically address any one of them. We have an elite terrorist group. We can say confidently that it isn't going to go away. We have a social base from which this terrorist group is being drawn. We have a larger set of societal environments within which a social base exists. We don't know what the relationship is between these three levels. If we could suddenly transform the regime in Egypt and the Palestine-Israeli dispute, exactly what would that mean for Al Qaeda? It's not clear.

I think this speaks to how difficult these issues are at a foreign policy level. We do have some experience with terrorist groups and we know the cases of the Red Army faction in Japan and the Baader-Meinhoff Gang in Germany. These groups reproduce themselves over several generations despite intense police efforts. You can say with confidence that it's not going to go away easily, but in the larger task of how to transform the environment from which these terrorists are emerging, I don't think we have very good ideas about how to do that systematically, period, which is why none of us have a good answer to it. We focus on what we know about.

SILVER: Herb Simon was right. We are the hard sciences.

PARTICIPANT: This is a topic change back to what we used to worry about before 9-11. Across the country in various federal district courts, there is a real attack on affirmative action in the universities. One of the responses to that is the concept of diversity, that it's very important to build a diverse community. It's a value that we all hold strongly. Michigan tried to defend its position and my colleagues from Michigan can talk about some research they have done in social science to demonstrate the importance of diversity. Do you see that as an important angle, and how university-based do you think that research should be?

KINGTON: Let me respond to that on a couple of fronts. The Association of American Medical Colleges (AAMC) and the Institute of Medicine recently held a symposium in honor in the late Dr. Herbert Nickens about affirmative action in the health professions. Myself and couple of colleagues reviewed the scientific literature relevant to the question of whether or not increasing diversity among physicians is an effective policy tool for addressing disparities.

There is a compelling amount of research already existing that minority students have different types of practices and are more likely to go out there and practice in communities and provide care in communities that are most in need of care, and they are more likely to provide primary care, which is arguably a more effective way to address basic healthcare problems in these populations. There have been over 10 studies over a 15-year period, with very consistent results.

Two sets of arguments are more difficult to make. One suggests that patients prefer to have providers of their same racial/ethnic groups and if they have them, they'll be what the health profession considers to be better patients. The evidence there is shaky and there's a lot of work to be done on that front. The second area where we need a lot

more research, and which takes the Michigan argument to another level, is the idea that medical schools with diverse student bodies are more likely to produce a different type of physician. There is actually some evidence suggesting that there is wide variation – for no obvious reason – in the diversity of medical schools across the country, and it’s not just a function of the populations in the states where they are. That creates an opportunity to really look at the effect of a diverse student body on the type of physician for both minorities and non-minorities.

I think that that hasn’t happened and there’s a huge need to have that happen and it’s possible. The data are conceivably there. There are studies that track attitudes coming into medical school and leaving and there is a significant amount of variation across medical schools. It just hasn’t happened yet. I think there’s now a push to try to do research like that.

MERRITT: It’s tremendously important, but I’m afraid it’s too late. The Michigan case is going to be argued at the beginning of December in the 6th Circuit. It will be before the Supreme Court a little over a year from now and decided by June of 2003. An interesting, compelling question is how we get these policy questions fed back to the research community. This is the point which it might have persuaded the court. It might still. I’m not a complete pessimist about this, but it’s going to be a difficult argument.

In a sense, what was necessary is what I talked about with *Brown*. It’s already a little too late to introduce good studies into the court record, though you could introduce studies like this at the appellate level. But it’s already a little too late for that.

What we really needed was the past 20 years that evidence mounting up, being talked about in the *Times* and the *Post* and the scholarly journals so that the justices going into the argument already knew that it made a difference. I wish we could look back at this and figure out where we went wrong in not signaling to the research community that this was the evidence that was going to be overwhelmingly needed.

PARTICIPANT: I’ve got a quick comment about the terrorism issue and then a question for two panelists. I just want to put in a disciplinary plug for anthropology on the terrorism issue. I do think that both history and anthropology are disciplines that have knowledge of these Middle Eastern cultures that are the breeding ground for the current terrorism. It’s the knowledge of the languages, the actual field experience, the particular dissertation work that anthropologists have been doing over the last 20 years that will provide some kind of context for understanding these ideologies and the complexity that each one of these nation-states presents. I think there is stuff out there that people are beginning to listen to because it’s hitting the pages of the *New York Times* and hopefully it will get further into the channels of government. There is a lot that can be done to further our understanding of Islam.

I have a question for both Sally Hillsman and Raynard Kington about the institutional changes that are going on both in the criminal justice system and the medical system and what is going on with research there. I think both places over the last 10 years are experiencing privatization and where we see it in the criminal justice system is the building of private prisons and the changing nature of incarceration; it is less in the hands of states and more in the hands of private companies.

I’d be interested to know whether that has any impact on crime and whether social scientists are paying attention to those kinds of changes. Similarly, in health care where we’re seeing privatization of HMOs and Medicaid. In terms of minority disparities, what impact do these changes have on the way health is being delivered and on outcomes for patient health?

HILLSMAN: Privatization in the criminal justice system has had social science attention. Frankly, if you look on the correctional side, most studies have been analyses that deal with public policy issues concerning the cost of one versus the other. Less has been done on the effectiveness of private versus public prisons in terms of recidivism and other kinds of outcomes. I think more research needs to be done in that area.

There is another area that you didn’t allude to, but I think is very important in the area of safety. That is that there are more private “police” or security forces in the U.S. and elsewhere than there are public police. The research on private security is limited and I think it’s a very important area, in terms of safety and for conceptualizing what

public policing and the public provision of security is going to look like in the future in relationship to the growing private security system.

KINGTON: With regard to healthcare, there has been a lot of research since the '80s on the differences between for-profit deliverers of healthcare and not-for-profit, which is a big divide. To be honest, I think that some people were disappointed that there were fewer differences than one might think, primarily because the same incentive structure was being applied to the for-profit and not-for-profits.

I think there is increasing concern that we now have literally hundreds of studies showing that racial and ethnic groups are treated differently once you get a ticket into the system, that getting a ticket isn't enough, and are even wide differences once you are in the system.

We're just beginning to see studies that are beginning to open the door on what happens in the room between a provider and a patient to truly understand what's driving the differences. To the extent that we've been able to control for things like severity of illness and preferences, there are still unaccounted differences that are hard to attribute to anything other than some type of bias, almost certainly unconscious. We're just beginning to see the studies that are beginning to open that door to see what the physician offers, what the patient accepts or doesn't accept, what the clinical situation is. I think we really want to know what's going on.

Furthermore, we won't be able to come up with solutions to change the differences that we see. There's been a lot of interesting research occurring now. I think it's shifted from the big picture to finding out what's going on between individual providers and patients and I think that's probably an appropriate shift.

SILVER: Please join me in thanking the panel. I think we covered a lot of ground and they did it very well.

The Contributions of Social and Behavioral Science (Panel 2)

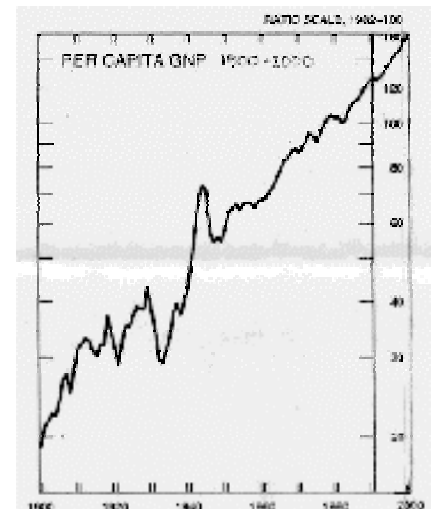
SILVER: We've addressed promoting fairness and reducing crime and improving health and creating a safer world. Now we'll look at some other clear contributions of the social and behavioral sciences to public policy: increasing prosperity, protecting the environment, and educating the nation.

Increasing Prosperity

Our first speaker will address increasing prosperity. Carl Christ is Emeritus Professor of Economics at John Hopkins University and Chairman of the Board of Directors of the National Bureau of Economic Research. Carl has had a long and distinguished career in exploring how economics research has led to increased prosperity. Now he'll tell you about it.

Carl Christ, John Hopkins University and National Bureau of Economic Research: I'm pleased to be here. Perhaps I should say something about what prosperity means. It means different things to different people. Raynard Kington gave an excellent presentation about one meaning, which has to do with the health aspects of life. There have been spectacular increases in life expectancy all over the world and spectacular decreases in infant mortality. I will not say that these are due to economic research, but I think that's one aspect we may want to think about.

The more usual meaning of prosperity among economists has to do with the material standard of living. If you'll look at the GNP graph, you see 100 years of real output of the U.S. economy per person. Real output means that it is not simply a measure of the number of dollars that are produced each year, but it is corrected for changes in the purchasing power of the dollar.



We've had inflation over the last 100 years, as you're aware. These numbers showing the per capita gross national product of the U.S. are corrected for inflation. There has been approximately an eightfold increase in real output per person in the last 100 years. That comes out to about 2 percent a year compounded. That doesn't seem like very much, but 2 percent will double every generation or so, about every 23 years, and that is the record of material output of goods and services in the U.S. per capita. I won't say that economic research has been the cause of this. It has been the cause of the smoothing of the fluctuations which you see in the second half of the period. The ups and downs in the graph are much milder in the last 50 years than in the preceding 50 years. That's related to the work that's been done in economics.

I like to use the definition of economics presented by Frank Knight, who is one of the leading economists of the past century. He defined economics as the study of the allocation of scarce resources among alternative ends. The idea is that there is something that we want to maximize and we can't raise the level to infinity because there are limitations on the amount of resources we have. We have to be careful about how we allocate our resources.

In economic research this deals mainly with income and wealth and output of measurable goods and services. In principle, a good economist will not forget that there are things that are not sold which are also important that people have in mind when they try to maximize their level of satisfaction.

For example, leisure is an important good. It's not sold in the market, but any analysis of labor economics has to take account of the amount of leisure that the working population enjoys. Clearly, when working hours are reduced, leisure can be increased.

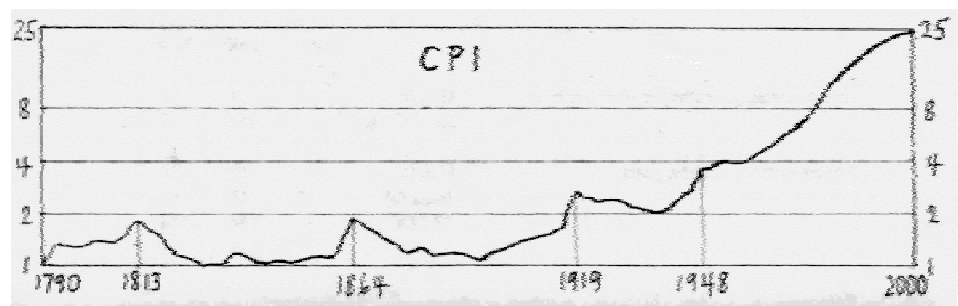
Environmental quality is another one and the general type of work that you do to earn your living is another one; people choosing a profession don't look simply at the income they can receive, but also the kind of activity they're going to be engaged in. The fortunate people are those who get paid for pursuing their hobby. That's the way I feel about my life.

Economic theory is about optimal choices. There is another branch of economics which is the empirical branch, which deals with the testing of theory, describing economic behavior, and measuring the amount of degree of growth and prosperity.

Our question is, How much does economic research contribute to growth and prosperity? I'd like to mention four ways that I think are relevant. One is to provide economic data. It's not possible to set forth sensible economic policy in the absence of information.

Private economists invented the idea of national income and national output back in the 1920s. The National Bureau of Economic Research, which I happen to be associated with, was one of the founding fathers of national income analysis. Price indexes were first put forth by private economic researchers. Those are important in order to adjust dollar expenditures for purchasing power. Data is very important.

A second is the fact that we know through economic research how to control inflation. We haven't done a very good job of it. Look at the graph, which shows a little over 200 years of price history. If you follow the price indexes since 1790 you see that whenever we had a major war, the price index doubled and then after the war it came back down to where it was.



This happened in 1812 and the Civil War. It almost happened in WWI. The price level didn't come back quite to where it had been before WWI. But since the middle of the 1920s, we have not had a substantial decline in the price level and it's economic policy which has produced this inflation.

I can say a little bit about how that has worked in the process of learning how to smooth out business cycle fluctuations, which is the third way that I'm going to mention that economic research has had an effect. What has been learned is that the federal government's spending and taxing can be controlled in such a way as to ameliorate economic fluctuations.

A depression or a recession is a situation where total spending by the private sector is reduced. The state governments cannot print money so they cannot do very much to ameliorate recession. The federal government has the authority to print money and it can, in a recession, spend more and tax less. This helps to counteract the decline in private spending which is a cause of the recession.

The problem in the last 50 years has been that that, as you'll see by looking at the graph, it was rather overdone in the sense that spending has increased too much and taxes were not increased enough. Federal deficits were very large and they were financed substantially by turning on the printing press and printing money.

When a lot of money is printed and turned over to the private sector, private agents spend it and this drives prices up and that produces inflation. In the post-WWII period, we've had an experience which has been moderately successful in soothing out economic fluctuations, but has been a clear failure in trying to maintain a stable price level.

The major reason for this stable price level over the 150 years before WWII was that the U.S. was on a gold standard. You cannot run an inflation if your price level is tied to a commodity like gold. In the last 50 years, our monetary policy has been freed of the restraints of the gold standard, which is a good thing, except that it allows too much money to be printed, which was done and which is a bad thing.

The fourth area is in the understanding of economic growth. It has been substantial as you can see by looking at the GNP graph. Where does it come from? It comes partly from increases in the amount of resources devoted to production. Labor is one major type of resource and capital equipment is another. It comes also from increases in the productivity of those resources. There has been a great deal of research on measuring productivity. Essentially, the way you do it is by measuring output using the National Income and Output Statistics. You measure inputs of labor and inputs of plants and equipment.

Notice that output has grown faster than inputs. Then you begin to understand what produces increases in productivity: improvements in the quality of inputs. In particular, education improves the quality of labor, as do improvements in the knowledge devoted to the production process that comes from research activity, and not only pure research but also development.

We understand a fair amount about this. We understand that countries that have low standards of living to start with, which have left themselves open to international trade can, by taking part in international trade, increase the productivity of their resources more than countries that have been closed to such trade.

I want to finish on one warning which is related to the previous talk on equality. One of the difficulties in economics is that policies that are chosen to try to redress inequality of income and wealth very often, and in varying degrees, interfere with the effective use of resources in the sense that they reduce the amount of output that can be had from a given set of resources.

There is a kind of trade-off. If you allow the economy to run with no concern for equality, let the chips fall where they may, you may get a very efficient economy which is producing as much as it can with the resources it has. On the other hand, if you are not satisfied, as many of us are not, including me, with the distribution of income and wealth that results in a totally private property economy, and you try to devise policies to even-out the income distribution a little bit, you need to be very careful not to kill the goose that is laying the golden egg.

For example, when the energy crisis struck in the '70s, the U.S. policy response was to impose price controls on gasoline. I think this was a bad mistake because it resulted in many people spending hours waiting in line try to buy the limited amounts of gasoline that were available. Since the prices were not allowed to go up, there was no incentive for anybody who could produce more gasoline to do so, and it was rather a mess.

Another example is that it may be deemed important to protect an American industry against foreign competition by restricting imports. One way to do it is through a quota which flatly limits the number of units that may be imported. Another way is by way of a tariff that applies a percentage tax to each import. A tariff is substantially superior because it means that if anybody figures out a way to do something better or more efficiently, there is an advantage in doing it. With a fixed quota, no inventions or adjustments can ameliorate the situation. Thank you.

Educating the Nation

SILVER: Our next speaker is Susan Fuhrman, who is the Dean and the George and Dianna Weiss Professor of Education at the Graduate School of Education, University of Pennsylvania. She's also the Chair of the Management Committee of the Consortium for Policy Research in Education, which has done substantial work looking at the school reform efforts of the past 20 years or so. Susan will tell us how we can educate this nation better.

Susan Fuhrman, University of Pennsylvania: Thank you and it is a pleasure to be here to talk about the contributions of social science research to education policy and practice. I think many important examples are cited in the chapter in *Fostering Human Progress*, including the fact the research on the importance of early childhood education led to the initiation and the expansion of Head Start.

In the late '80s when we looked at state policies and found they were enormously disconnected and fragmented and also that our international competitors, who had much more coherence around their curriculum, professional development, teacher preparation, and other policies, were way outperforming us, that research contributed to the standards-based reform movement that all states are now engaged in today.

Research on how humans learn has made contributions to that standards reform movement as the standards are crafted. Many of them try to reflect what we know about how students and adults make sense of new information and incorporate it with what they already now.

To take a very recent example, the third International Math and Science Study, which took place in 1995 and again in 1999, has yielded very important information about the U.S. curriculum in math between grades 4 and 8. It revealed how we were repeating subjects over and over again while other nations were forging ahead. Those findings are now being used by the 28 states and districts that participated in the benchmarking study in 1999. They are analyzing their own data and figuring out ways to reflect what they're learning in curriculum and professional development.

I would like to focus my comments less on the good examples than on the future of increasing the utilization of research in education. I think that this is an extremely important goal and one which will be difficult to reach; we have long way to go.

Let me address the utilization side first. There is not a climate for the use of research in education, not as good as we would hope. You can witness this at the very top as policymakers right now try to develop and implement new accountability systems that hold students and schools responsible for achievement on standards.

There is increasing information about the technical fallacies associated with the tests that are being used. For example, we know that many of the assessments are not aligned to standards in the way that they would need to be to reflect achievement, even though testing companies claim that they are. We know that many of the current assessments are not sensitive to instruction, so if you were to improve instruction, which is the goal, that improvement might not show up on the assessments and some irrelevant factors would influence how students did on the assessment.

Also, we are increasingly aware of information about the error terms of these assessments. A recent important paper by Kane and Staiger showed that in North Carolina, the year-to-year fluctuations are so great that you cannot assume they represent progress or the lack thereof at all, and that holding the students in schools accountable for yearly

progress is probably a grave mistake leading to the possible misclassification of as much as a third of the people involved at any one point in time.

If we move to practice, to the district and school level, we find a climate that doesn't necessarily prize research the way that those of us in this room would hope. I just participated in a study of three large districts adopting comprehensive school reform. We found that even though there was considerable rhetoric around the importance of evidence in choosing comprehensive school reforms, philosophy always trumped that evidence. People liked certain reforms and they didn't like others. They were somewhat more concerned about evidence with their own local internal reforms than they were with national reforms that they were adopting. Unfortunately, the national reforms often came with additional money and national legitimation.

The picture is even worse at the school level. We surveyed teachers and visited schools in two of the cities and teachers were quite clear about what they value, which is what they can see with their own eyes. Between 80 and 90 percent of teachers said that the endorsement of other teachers was the best evidence of quality in the comprehensive school reform. Only 60 percent gave such support to published research, and even more depressing for those of us who are researchers, 35 percent said the findings published by researchers should not be trusted. Clearly, evidence-based practice is not a part of the culture of education.

There are a number of reasons. Everybody is an expert; this is especially true of policymakers, because everyone went to school. I also think that there are deeply rooted traditions in education which conflict with the goal of attending to evidence. One of the most important is the notion that practice is isolated, that it takes place behind a closed classroom door, the teachers do not share with one another what works and what doesn't.

It is very difficult to develop a set of professional norms that evidence can inform because practice is so isolated and thought of as an art and not a science. I also think that research can take a portion of the blame here and that the research base in education is weaker than it should be.

A good example is the comprehensive school reform that I was referring to earlier. Even if people in districts and schools had been more attentive to evidence, they would have had problems picking designs that were sufficiently based on research. The American Institute for Research did a study of 28 comprehensive school designs and found only three had sufficient support to be called research-based.

I think there are four ways in which education research often falls short. One is that too often, research designs are not suited to the questions that policymakers and practitioners need answered. When this critique is launched these days, it often means there are too few randomly controlled trials in education and, therefore, an inability to answer the causal questions that people want to see answered.

I think that's true. I think it's changing and there is a lot of creative thought right now about using assignment by place. For example, schools and districts might be more easily assigned than students under certain circumstances, but even random assignment of students is increasing in education.

I did not mean for this remark to apply just to that. I think there are many instances where research is stretched to answer the questions that policymakers and practitioners need answered. When this critique is launched these days, it often means there are two few randomly controlled trials in education and, therefore, an inability to answer the causal questions that people want to see answered.

As a Dean, I can be self-critical, but I think we may offer methods courses to graduate students that are excellent, but insufficiently prepare them to ask good questions and to know which methods to employ in answering those questions.

A second problem is that we don't do enough longitudinal studies in education. Our policymakers want to know whether effects last and we tend to do shorter studies. A lot of what I have to say is reflective of the fact that education research is underfunded, but a relationship is circular between our quality and underfunding.

We don't do enough replication. We don't do enough studies that confirm findings or that tell us how certain reforms or policies or practices work in different contexts. We have a real premium on newness. I think education research is faddish just like education practice often tends to be. We always push towards the new, and even when it comes to dissertations, we rarely value replication and accord it the place where it might be done more often.

We don't do enough synthesis in education. I think a particular problem is when there are ideological divides that are reflected by different findings. You could think of the research on choice or on bilingual education or on reading. Until recently, there was very little consensus about reading, but plenty of battles.

When policymakers hear those battles, they don't know what to believe and tend to discredit a lot of what they hear. If we could do good syntheses that put together information and shared the weight of the evidence, I think we would have a lot more credibility.

I want to end on a very positive note because I think a lot of this is changing. I wanted to tell you about the Campbell collaboration which is taking place at the University of Pennsylvania under the leadership of Bob Boruch. Many of you may know about this, but it's an attempt, named after Donald Campbell, to create up-to-the-minute, up-to-date syntheses of research and education and social policy areas and to provide them in a very accessible way. It is web-based and you can find the collaboration at <http://campbell.gse.upenn.edu/>. It's a multi-national effort. I think there is funding from Sweden and Great Britain and from foundations in the U.S. I definitely think there's hope. People are attending to these issues. The goal of increasing research utilization and fostering progress sets is very well placed.

Protecting the Environment

SILVER: Now, to address protecting the environment, we have Mike Toman, who is a senior fellow at Resources for the Future, which is one of the more significant non-governmental organizations in examining the environment and use of natural resources. He is the former Director of its Energy and Natural Resources Division. He served as a Senior Economist on President Clinton's Council of Economic Advisors, working on natural resource and environment issues. He has also been an economist at the Federal Energy Regulatory Commission.

Michael Toman, Resources for the Future: Thank you for having me today. Listening to Dean Fuhrman's comments, I realize there is a lot of similarity between education research and my field. Everybody is an expert about the environment because we all breathe. There are an awful lot of examples where philosophy trumps evidence. So, you may hear a few common themes here as I go.

Shamelessly stealing a joke from a friend of mine in the field, some of you may think that environmental economics is a living oxymoron. But actually economists do care about the environment. I'll give you a flavor for how that works out professionally as well as personally.

For the last five years, I've spent a great deal of my professional time thinking about the problem of global climate change. Let's take that problem and use it as a device to talk about the role of economics in particular. I'll try to say a few words about the other social sciences with apologies to colleagues in other fields here who will not be adequately represented in these comments.

We have these complex chemical, physical, and biological processes at work with human beings changing the chemical composition of the atmosphere, which then leads to a lot of possible effects that could be large and long-term. The government in the U.S. spends quite a bit of money researching these processes and globally quite a bit more money is spent. That's a good thing, but if all we understand is the chemistry and the biology and the physics of these processes, our knowledge base is wholly inadequate for guiding public policy. If we don't have the human dimension, then we haven't really captured the essence of the problem.

Notwithstanding the fact that we sometimes call the program here in this country the human dimensions of global change, the human part doesn't quite get all the attention that it might benefit from getting. So key questions that come up in the climate change area (and they come up when you're dealing with any natural resource degradation or environmental damages problem) are the following:

What are the human, not just the geochemical, consequences of climate change? Who is affected and when? How did individuals respond to lessen these adverse impacts on their own, something that in the climate area is referred to generally as adaptation? How can individual or collective actions be taken to mitigate the risks?

Mitigation can occur in a variety of ways. When economists approach this problem, they basically try to think about the global environment as a public good. The kind of thing that produces value for society at large even though it's not a commodity that is transacted in markets like oil or timber or the apparel that we're wearing today.

We generally tend to take the benefits of having that public good as given, in terms of people's choices between them and other values in society. But as I'll try to indicate later, an increasingly important area of interdisciplinary research is looking at how preferences are shaped and evolve over time.

Now, what I said really applies to climate change, but we could apply it to the accumulation of stratospheric ozone in large cities like Los Angeles and sometimes Washington. We could apply it to global deforestation and the potential loss of significant biodiversity and a variety of other problems. They can operate at local as well as regional and global scales. When economists approach this kind of problem, we do tend to think of the environment as a commodity, but that doesn't imply that we've degraded it. It simply means, as Professor Christ suggested in his comments, that there are a certain amount of resources that we can devote to a variety of applications. One important one is the protection of the environment at different scales, and in making those kinds of investments, we'll get certain benefits.

We'll preserve certain things in a better state, but we'll also incur costs and we have to think about the way we balance those costs and benefits. Then, when policy is called for as it typically is in this area, we have to think about how best to design the tools for intervention.

The first thing an economist asks is why are we observing what we are seeing today, and the forces of economic growth that Professor Christ talked about can have very important effects on the environment, both positive and negative. This is one of the important areas of research these days.

To what extent does economic growth lessen the burden or enhance the burden on the environment? It depends, but we're beginning to understand better on what it depends and in which cases it can be positive or negative. In addition to overall economic growth, another important driver includes the evolution of technology, which is not itself exogenous, but the product of public policy and individual incentives.

Changes in institutions, what economists refer to as infrastructure, play a key role in this, as does population growth itself, and its interaction with all these other elements. These are not simple uni-directional forces. The relationship between economic growth and the environment is not uni-directional. The environment is one of society's assets and its state will have important implications for economic progress, especially in those parts of the world that are much more dependent on natural resources, which tend to be the poorer parts of the world.

Understanding something about the drivers may reveal a need for some kind of intervention to achieve more environmental protection or resource safeguards than would otherwise emerge from purely private decisions. The first step for an economist is to think about the incremental benefits and costs of some kind of intervention.

The word *incremental* cannot be stressed enough in thinking through how this works. There was an article in *Nature* magazine where some of the better known environmental and ecological economists got together and said, This is the value of the world's entire natural resource base, not just the coal and the oil and the timber, but also the biodiversity and the wetlands and all the rest. They simply enumerated all these categories, put together a pastiche of estimates, aggregated it all and found out that it was some multiple of the world's GDP. As some of us pointed out at

the time, that could be thought of as a bad estimate of infinity. If we had to do without all natural resources and environmental services, all life would stop.

That's not the typical public policy problem. The typical public policy problem is somebody wanting to build a subdivision on the Eastern Shore. We're going to lose some wetlands if that occurs. What is the cost to society of that vis-a-vis the private benefits the developer and his or her customer would get?

We look for incremental benefits and costs. In doing so, we try to understand not just those benefits and costs that work their way through markets, but also the many factors that work their way through other means that aren't mediated through markets. In addition to looking at productivity issues and technical advances, we also look at tangible and intangible effects on human health, the risk of premature mortality, recreational values, and even spiritual values in these kinds of analyses.

To do this work, economists have had to invent tools that are on the border between science and art. One of these tools (mentioned in the booklet) is this idea of using survey-based techniques or contingent valuation methods. Basically, if you have no other way to infer value because you can't find any other data to use, you can simply administer a survey and try to get people to very faithfully recreate their value. Time doesn't permit a detailed discussion about how difficult and controversial this method is, but at least it illustrates how hard it can be to get at these values. On the other hand, if you don't try, and you set these by default to zero, you've underestimated what you want to try to measure.

Fortunately, there are other cases from which we can draw inferences a little more straightforwardly. There is some evidence that, after you normalize for all other factors, housing prices are positively related to the quality of the local environment. We can use evidence like that to try to infer how individuals value a better environment. In that kind of assessment, we're implicitly assuming that when people are making choices about where they live, the kind of house they buy, the kind of recreational choices they make, the kind of jobs they take, and so on, they're fairly rational and fairly well-informed.

That's a controversial assumption that does weaken the case we can make empirically for some of our valuation work. We invent and use complex models to try to look at cost and benefits that put the whole economy together. We feed into those these difficult valuation studies to try and understand what a non-market attribute might be worth. Having gone through that assessment, I want to come back in a minute to the question of what attraction that approach has in policy and what that means in research.

Another area that economists have spent a lot of time on in the last 20 years is the design of policies. That is a question that an economist probably would answer using cost-effectiveness. How much can environmental improvement or resource protection can we get per unit expenditure or resources that society gives up? The goal is to get as much as possible for each unit of investment.

The research overwhelmingly demonstrated, both in theory and increasingly in real experience, that when we can use economic incentives to encourage people to protect the environment (rather than push against them), we can get a lot more for a lot less.

The prime example of this in the U.S. is that power plants are able to buy and sell the right to emit sulfur dioxide. It does not mean that they are given some kind of immoral right to pollute or the ability to buy and sell the right to do some immoral act. This is not the environmental equivalent of prostitution.

But it is within, in fact, a regulatory system that establishes a rather strict cap on emissions and the ability to transfer the power of incentives into increased flexibility and meeting the overall policy goal at a much lower cost than would have otherwise been the case.

Economists have advanced their knowledge considerably in this area as well as in understanding a number of other important parts of the institutional landscape. For example, the way that liability law interacts with incentives to create greater or lesser environmental hazard, the way that international agreements do and don't succeed in promoting

global interests versus local interests, and importantly, given the recent Nobel Prize in Economics, the role that information, incompleteness, and asymmetry have in determining the way that we can regulate when regulators know a lot less than those being regulated.

A lot of this analysis really refers to overall net benefit. It's a very convenient fiction that we can simply aggregate all these things together. An important challenge in research is understanding the winners and the losers – the distributional consequences. That's important for understanding the overall importance or effectiveness of an environmental policy, but also for unraveling some very complicated cause and effect relationships that come up in the environmental justice area. This is an area in which I think we're making a lot more progress, but we clearly have more to do.

I'll comment on two practical problems of the environment to give you a flavor for this on a more everyday level. Ground-level ozone. There have been studies that indicate that it can harm people's lungs when it's inhaled. This is the basic chemical constituent of smog. Those studies have tended to be somewhat equivocal in what they show. They tend to show that only a very small population is adversely affected, and only if they're exposed to high levels of ground-level ozone for a long time.

Asthmatic children tend to be a vulnerable subgroup as well as the elderly. EPA has been trying to tighten ozone standards and has issued new regulations. These new regulations would operate uniformly across the U.S. in all places. Los Angeles and rural Wyoming would have the same standards and be required to make the same investments. There is no doubt that there will be some improvement from that regulation. Will the benefit of that regulation exceed the cost? It depends on who you ask and how you measure benefits and costs.

Climate change. There seems to be increasingly little doubt that climate change is a serious problem. There are still some scientists who would argue the contrary, but I think the evidence is fairly clear. Most of the costs that we would incur to restrict emissions would probably occur in the next 20 years – during most of our lifetimes and certainly well within the youthful lifetimes of some of our children. Most of the benefits would occur when our children are middle-aged or their children are growing up and assuming their places in society. How do we value those benefits one and a half to three generations or more in the future against our costs today? If you were to use a standard model of discounting that economists like to use which simply compares everything in terms of its rate of return, you would choose to emit a lot of greenhouse gases today and worry very little about the future.

That seems to be a bad outcome. On the other hand, if we took all that money and invested it in the health and wealth of the poor parts of the world, perhaps they would be better off than if we invested in climate change protection. There are no easy answers, but these points at least illustrate the tradeoffs.

I've given short shrift to the other social sciences. Let me just mention briefly that economists work with political scientists to understand the dynamics of issues within the policy process, the way international agreements work, and the incentives for coalition formation, which is crucial in translating a research finding about what seems to make sense and what can actually happen.

Psychologists, sociologists, anthropologists, and geographers increasingly work with economists to understand how values are formed, how community norms operate, and so forth. Finally, economists are beginning to read history because it helps us understand why we are where we are today.

What difference has all of this made in terms of policy and what does it tell us about research? There has been broad-based acceptance of incentive-based approaches, for instance, of the sulfur dioxide example I mentioned before. I think that's been a major win for research and for policy and for societal well being. We accomplished a lot less, both in procuring complete science on cost and benefits and in legitimizing the idea that looking at the tradeoffs of costs and benefits is okay to do.

Part of the problem here is the large gap between how experts assess environmental problems and the way the lay public assesses them. The lay public tends to worry about nuclear power explosions at power plants and tends to discount the risks of indoor radon exposure. Experts who look at the risk from a more scientific perspective turn those

two around in their ordering. There is still a question of whether it makes sense to do the kind of cost-benefit analysis that economists routinely do and how it would be used in the policy process.

Clearly, in the social sciences we need to better understand what that resistance is and how we should be altering our research to be useful. We also need to simply understand more about the political environment in which that work is done.

We are beginning to understand better the incentive effects of various institutional arrangements, as I've already mentioned, but we still have a major challenge in figuring out how to overcome implementation problems, whether it's the environmental side-agreements in the North American Free Trade Agreement or a climate treaty, which for now doesn't appear to be making a lot of headway.

Last, but certainly not least, is that question about the fundamental drivers and the interaction between economic progress, demographic change, and the environment. Seeing these as interrelated, intercausal, and not just in a uni-directional relationship is absolutely vital.

But here, I think our research cupboards are really understocked. Much of our analysis still uses very fancy models that would apply to a highly developed economy, but only are beginning to adapt to describe the realities of the institutional and economic strictures that are confronted, for example, in India or Brazil or a small island state. We have a lot more to do before we can fully contribute good advice and good understanding in that area. Since that affects four-fifths of the world's population, it should be a major target for future research. Thank you.

Q & A

SILVER: Both Susan and Mike mentioned the problem of philosophy trumping research. Do either of you see any way around this?

FUHRMAN: I think it would be naive to assume that it won't happen fairly frequently, but I think there are things we can do to mitigate against it. I certainly think that better evidence is important, but I also think that we haven't done enough with our audiences to educate them about research.

Certainly, in teacher preparation or leadership preparation we don't focus efficiently on what would qualify as an evidence-based practice. We don't take enough time to walk through the criteria for good research. In dealing with policymakers, for example, when we present evidence, we don't sufficiently talk about the bases from which we're drawing and the limitations or the advantages of a certain kind of research. We need to do a lot more of that.

TOMAN: Since I'm only an amateur educator, I'll just say, amen. In the economics area, we haven't done nearly enough to present what it is that we're analyzing when we do these analyses and to help the intended audience, including policy audiences, understand what can and cannot be inferred from these sorts of studies.

It's very important, as I suggested in my comments, to talk more about the winners and losers, and it's also important to make very clear the distinction between an analysis of overall costs and benefits, which I do strongly believe should be a part of public policy decision making, and the notion that this can become a simple rostrum for making decisions independent of any other important considerations.

I guess I'm a naive optimist. I think if we keep plugging away at this, we can get audiences to better appreciate the importance of what we're doing. If we're going to invest X billion dollars and one investment will save a few thousand lives and the other will yield a few million, there's a story to be told there if we can figure out the right way to tell it.

PARTICIPANT: Particularly in the field of education, it seems to me a lot of the research has talked about the importance of the families from which these children come. I notice that so little research goes into that. In terms of

economic allocation, the question of human capital development. Is it advantageous to spend more money on the families and more money on education from the prenatal stage to those early baby stages? How would we work out the economic analysis to determine the outcome of that?

FUHRMAN: I think the research is becoming clearer on the importance of the early years and investments in it. One of the problems we have is that there are different domains in policy and in research for dealing with these kinds of questions. We so rarely get the cross-societal questions of investment in the family rather than the schools.

We tend to put ourselves into our own institutions. When we talk to education policymakers, very often we would talk more about the family if their jurisdiction related to it. Talking to legislators, for example, we're dealing very often with a K-12 or a higher education policy committee and not a health or welfare or some other committee. We tend to fall in with their divisions and do not ask enough of those big cross-societal questions.

CHRIST: There's a substantial amount of research in economics on the relation of education to the capability of the people who have been educated. It's important to try to separate out work of this kind – the difference that education makes versus the difference that it makes when different kinds of people are brought to the educational process in the first place.

I think you're absolutely right that early experiences in childhood make a big difference in the receptiveness of a person to education and the ability to take advantage of it. I think that's a crucial question and it's one that considerable research in economics of education has dealt with.

PARTICIPANT: [off mike.]

SILVER: I think she's referring to what David Ward talked about earlier in the morning in terms of interdisciplinary clustering within universities to address problem-focused areas. In the case of family studies and education, then, how do we break through the disciplinary boundaries found on campuses.

CHRIST: Where there are barriers, it certainly makes sense to try to break them down, and economists are beginning to learn something from psychologists about the way people behave in situations of uncertainty. I think that political scientists are beginning to learn something about the effects of incentive systems on private behavior.

It's hard to break down these barriers because each subject has its own traditions and its own methods of work and it's easy for a young student to follow the steps that have led to distinction in that field in the past. It's only the more mature people who open their eyes and look around and try to see connections and contribute to breaking down these barriers.

PARTICIPANT: I'd like to take a variant of Howard's question about the degree to which ideology and knowledge are in conflict, that is, the degree to which we see research findings driven by ideological perspectives. It gets to the infrastructure of social science and the way in which knowledge starts to clarify what we know and what we don't know, particularly in a setting where replication may not be as frequent as we would like. There is a long time-lag between ideologically driven research findings, which are not unheard of, and the degree to which they show up as general knowledge.

CHRIST: I would like to comment on this. I've been struck very much with the degree of ideology in economics. I came into economics through physics where there isn't very much ideology, comparatively speaking. It struck me that in the 1950s when I was just entering the profession, most of the bright young people in economics seemed to do work where the general flavor was – if the goal of government policy is to improve general welfare, the people who go to work in government are committed to this. Most of the research dealt with means whereby government policies could improve welfare. It seems to me that beginning about the time that Barry Goldwater ran for President and then Ronald Reagan ran for President, the bright ideas came more from the other side of the ideological spectrum in economics and you got a lot of bright young people asking the question, is it the case that people who go to work for government and who engage in government policy are single-mindedly devoted to the general welfare? Or is it the

case that sometimes a person who runs for office or takes an appointment or other type of federal, state, or local position has interests of his own?

This gave rise to a growing branch of economics called public economics which deals with the question of how to design a constitution and a set of legislative strictures which will influence public officials to get them to pay attention to the public interest rather than their own private interests.

I think there has begun to be a little bit of a swing back now in the last 5-8 years. We're now getting some of the younger people interested once again not in knocking government, not doing research showing why government can't do anything right, but rather taking a balanced mixture of the two kinds of work that were done in the '50s, and then later in the '80s, to try to form a more realistic and effective way of designing public policy, which would take account of the fact that the people who administer it have goals which, in some cases, don't necessarily coincide with the goals of the people who put them in the office.

In addition to public economics, we have a lot of work in economics about the difference between agents and principals. The problem is how does a principal design a contract so that an agent will do what a principal wants him to do rather than what the agent wants to do. There is a big field of research that has been very productive in this respect.

Celebrating 20 Years of COSSA

Janet Norwood, COSSA President: On this, the 20th anniversary of COSSA, we realize how important the work of creating COSSA and building it has been over the last two decades. We realize today more than ever how many of the issues confronting Americans today depend on our understanding of multi-disciplinary research. It's a multi-disciplinary world in which we live and the social sciences are important in the identification, illumination, and solution of the problems we face.

I'd like to take the opportunity also to pay tribute to the contributions of Howard Silver to COSSA. He's worked tirelessly to promote the social sciences. His vision and his success over many years has helped enormously to bring this organization to where it is today. I'd like to ask all of you to stand up and to join with me in offering a toast to COSSA. May it continue to grow in influence and in its success in promoting the social sciences.

SILVER: Thank you. I should take a minute to respond to Janet and say I really appreciated that. I appreciated your support and the support of everybody in the room. I feel greatly honored.

I also want to say, as I said earlier, one can't do it alone. COSSA has had the support of its executive committees over the years, its Boards of directors over the years, and its staffs over the years. In particular, I'd like to thank the current staff who are sitting among you, Angela Sharpe, Chris Ryan, and John Wertman, and the person who keeps our books honest, Karen Craft. Given the contingencies we've been working under for the last seven weeks, they've been terrific in helping to pull this event off today.

With that, let me turn the meeting over to Arnita Jones, who is the Executive Director of the American Historical Association. She will introduce our speaker.

Lunchtime Address: Ernest May, Harvard University

Arnita Jones, American Historical Association: It's a real pleasure to introduce Ernest May, who is one of America's most distinguished historians. His academic career has been focused at Harvard University where he's Charles Warren Professor in the History Department and also a member of the faculty of the Kennedy School of Government.

He has served in several administrative positions there as well, including Dean of the College and Director of the Institute of Policy. His scholarly work has focused on diplomatic and foreign relations history ranging from studies of the Cold War to the Monroe Doctrine to the Cuban missile crisis and, most recently, Hitler's conquest of France.

Many of these works are focused on the decision-making process, the way people who make policy use information, particularly historical information. A good example of this kind of effort and my personal favorite among his works is *Thinking in Time: The Uses of History for Decision Makers*. It focuses as much on the misuse as the use of history. It's this aspect of his work that has led him outside the academy to serve as an advisor and consultant to government, particularly in the defense and intelligence agencies.

That role is somewhat unusual for an historian. Historians are also particularly appreciative of his attention to government records, their preservation, accessibility, and declassification. It's not very glamorous to work on this set of issues, but it's very important and we're in his debt for his leadership in this area.

It's particularly appropriate then for Professor May to be here with COSSA as it celebrates its 20th anniversary by highlighting the utility and value of social science research, because more than any historian I know or know of, he's thought seriously about historical knowledge, historical research, and indeed historians themselves, and how they can be of practical use on problems policymakers confront every day.

It was this conviction that led him to a very imaginative experiment, and it's actually the way I first met him about 20 years ago during the worst of the period of overproduction of doctorates in history and virtually every other social science and humanities field.

Most graduate faculty dithered and despaired, but Ernest May launched a program. Some of you may remember the Careers in Business effort. It was an initiative to try to place people with history, humanity, and social science training in careers other than higher education. It was a noble effort. It didn't lead to structural change in graduate education, but it provided real help to a generation of scholars who were caught on the wrong side of the academic supply and demand equation. It provided important moral support for people who were trying to get new programs started in public history, policy studies, and so forth.

With that, I'd like to welcome Ernest May and I look forward to hearing what he has to say today on the role of history and the problems that confront us.

Ernest May, Harvard University: Thank you very much, Arnita. You always have a sense in an introduction like that that you're hearing your obituary. I'm very appreciative. I'm really grateful for the opportunity to speak to this group. It's a conventional phrase, but it's true.

This is a very important organization, and has been a very important organization for two decades and I'm particularly glad that I can speak as a member of the American Historical Association and that I was introduced by the Executive Director of the association, because it's marvelous that our profession is part of this group.

As you know, a lot of people who are historians think of themselves as humanists rather than as social scientists and that's quite appropriate. It is a discipline that in some ways is a bridge. The question of whether history is a social science or one of the humanities is one that people debate from the first day of graduate school on through the rest of their career.

When students ask me this question as they often do, I give them a flip up answer, but it's not an unrealistic answer. It depends on whether you're seeking research support from the Social Science Research Council or the National Endowment for the Humanities (NEH) [laughter]. It goes a little further than that because that actually does help to frame the kinds of questions that historians address.

I want to do two things today. One, I want to ply my trade a little bit and talk about the history of social science research. I know that you went over some of this in the morning and you're going to go over more later today, but I want to say a word about it because we are in a period of high emergency. We are in a period that perhaps deserves to

be called a crisis. It is likely that we will for a considerable period of time be in a condition of waging war. I think it's worth thinking about the relationship between social science research and past crises or emergencies in our history.

Second, I want to say a little bit about history as one of the disciplines represented here and what historians can do in this period of emergency and crisis. The general proposition that I think emerges if you look at the history of social science in the U.S. over the last century and a half is that bad times are good for social science. A great burst of work in social science was prompted by the economic crisis of the late 19th century – the panic of the 1870s and 1890s that gave rise to work in labor economics and progressive reform. The first world war produced another burst – remember the large shipload of experts in geography, history, and other fields that accompanied Woodrow Wilson to Paris in 1918, 1919.

Obviously, the Great Depression was a big spur to the studies of economic theory and practice, particularly in macroeconomics. The second world war produced a lot of work on cultural studies; anthropologists, sociologists, a large number of historians and people who did political studies were involved directly in the war effort, making up for the government's lack of knowledge in foreign areas. The Cold War saw extraordinary work done on issues related to the existence of nuclear weapons. There was collaboration between social scientists and physicists and mathematicians; I'm thinking particularly of the kind of interchange that took place at the RAND Corporation in the 1950s, which spilled over to the universities, with people like Tom Schelling. In the '60s there was the civil rights revolution and elements of turmoil at home that produced a big surge of work in social science. And later work was spurred by the energy crisis of the 1970s.

When you think about periods when the country has faced real problems, we are probably in such a period again. Just to pause on a question that was discussed this morning – bad times have been good for social science; has social science been good for bad times?

There were times when the work was applied questionably, to say the least. For example, in the Progressive period, one of the things that developed was the idea of the initiative referendum, which I think proved to be a bad idea in practice, if you look at the ballots in California. If you look at the people who accompanied Wilson to Paris, some were overly zealous in their pursuit of self-determination, encouraged fragmentation in East-Central and Southeastern Europe, which contributed to problems that we've lived with since. In WWII some of the studies of foreign countries were at least superficial and sometimes misleading in shaping the way in which we then ran propaganda efforts and civil affairs programs in the aftermath of the war. In the '50s and '60s, a lot of the work that was done on nation building, particularly in Southeast Asia, again seems dubious.

But I think the positive side is stronger than the negative side. Most of what was done resulted from attacks on the economic crises in the late 19th century that took form in the Progressive Movement. Most of that really did have the effect of adding to the relative leverage of consumers and workers against employers. You can argue that the late 19th century had an excessive amount of leverage against the other elements of the marketplace.

The work that was done by Richard T. Ely and other labor economists really helped a lot, as did the work of the progressive social scientists who helped Hiram Johnson in California and who did some very good things, apart from the referendum issue. While it is true that some people who advised Wilson were perhaps excessively dogmatic toward self-determination, if you look at the records of the foreign offices – our State Department, the British foreign office – you see the visions of the future that professional diplomats had as they went into the negotiations over peace. They really were indifferent, for the most part, to linguistic and ethnic difference, to the kind of evolution that had taken place in East-Central Europe. They were indifferent to the fact – as it was put forth by the British political scientist R.B. McCallum in a book written during WWII, looking back at public opinion on the last peace – that the diplomats did not recognize that the day was long past when all that folks said was, “Where are you going my pretty maid?” It had become important for people to be able to read maps and street signs and hear their own language. There were some excesses, but on the whole their work was beneficial.

Certainly, there is no question that the work of a variety of economists – particularly macroeconomists – in thinking about the business cycle, and thinking very hard about the calamities that came upon the international economy at the end of the 1920s and the beginning of the 1930s, really has contributed to a much greater ability to manage the cycles and the risks in the economy.

I would give great credit to the people who thought about nuclear weapons. It is true that what they observed was in some respects a kind of theology divorced from reality. Nonetheless their work did permit us to work our way from a position of “sitting on the edge of a volcano” to the point at which the focus came to be on measures for and negotiations for arms control. To the social scientists who were involved in that, I think we owe a great debt.

The net result I think is positive, and we can be happy with what social scientists contributed to our past emergencies and crises. It lays a foundation for thinking optimistically about the future.

I think it highly likely that we are entering a time like earlier periods of crisis, that this is not just a momentary emergency but one that will last for a considerable period of time, that it is going to result in government – including the legislative branch – calling loudly for contributions from social scientists to help address these problems. I think we are likely to get the kind of demand for area studies and area specialists like we had in the 1950s as a result of finding that the Cold War had expanded into the third world. There will be a demand for analyses and expertise, not just for language and culture and history, but for marrying that expertise with the ability to analyze social structures and political structures in ways that can be useful for thinking about other parts of the world in which we will be involved.

It is crude social science to say that this pattern happened in the past so therefore it’s likely to happen in the future. I think it’s more, though. In the past, it has been the case that in one case after another, where there was a sense of national emergency, a sense that we as a nation were ignorant and needed tools with which to think about these problems, the social sciences offered those tools. I think it highly likely that there will be loud calls on us.

I’d like to say a bit about the role of historians in all of this. Simply put, I think historians contribute to our common enterprise in essentially three ways. One is that we are very important to other social scientists, because we do look harder at basic data. We look at more evidence and we look more skeptically at evidence of events and people than do most people in other social sciences. The result is that we can force people who are more theoretically bent in other social sciences to look harder at their data.

I’ll give you two examples from my own area. One of the popular theories that you all know in international relations is the argument that international relations is essentially a competition between power-seeking states, and that when there is a hegemon, as the U.S. is today in the world and as others have been in the past, there is a natural tendency on the part of other states to band together to try to balance the power of the hegemon. That’s perfectly reasonable. But the historian Paul Schroeder, a historian of international relations at Illinois, spent some time inspecting this proposition and testing it against what we knew about state-interstate relations between 1648 and 1945. And he wrote a long article on international security in which he wrote that most of the time, that’s not what happens. In fact, most of the time when one power had become significantly stronger than others, the tendency of most other state and governments was not to join forces against it but to do other things – hide, be isolationist, or join the bandwagon. That’s an important contribution, and raises lots of questions.

One more example. One of the most popular current theories in international relations is democratic peace, which is the argument that democracies are less likely to come in conflict with one another, indeed that two liberal democracies are very unlikely to have a war. Again, the logic is easy to see, and it was developed as a matter of logic, not as any generalization of past experience. Again, some historians have looked at this, and it’s not like Schroeder’s challenge where they found at lots of cases, but they find is that when a conflict develops between states that were on friendly terms previously, people in those governments and states have tended to draw institutional and ideological differences between the two that they hadn’t drawn before.

To take the most conspicuous example, as of the late 19th century, people in Britain tended to regard Germany as the other parliamentary monarchy. Germany had wider suffrage than Britain and had different aspects of democracy. It was only as Britain and Germany came into conflict, in the colonial race and the naval race in the late 19th century and early 20th century, that people in Britain started to draw the distinction and say that Germany was institutionally different, that it was not a parliamentary monarchy but something else, something closer to being tyranny.

That’s the kind of thing historians can do – raise questions by fingering the evidence about generalizations that

seem highly plausible in their internal logic that deal with social phenomena.

The second thing historians can do is to help the people who make decisions think about the way in which they are using history. People in public life are very prone to reason historically; they are less likely to draw on some body of theory than they are to make use of an inference from some experience that teaches a clear lesson.

There are two famous examples. In June, 1950, Harry Truman got a telephone call from Secretary of State Dean Acheson who reported to him that the North Koreans are attacking South Korea. Truman's immediate reaction was that it needs to be defended. What's striking about that is that the generally policy question – "What should we do? What is our interest in South Korea?" – had been addressed at length in two NSC papers in the previous year. Those papers had been reviewed, and each time the President signed a document saying, "We have no interest in South Korea, it's a liability to us. We'd be better off, essentially, if South Korea were to join North Korea."

But the reason Truman responded as he did in June of 1950 was not because he changed his mind about Korea – which then, as earlier, he would have had a little difficulty in placing on a map – but because he saw this as an act of aggression that called to his mind events of the 1930s – the Rhineland, Ethiopia, Czechoslovakia, and so on. He drew this lesson from the experience of the 1930s, that when aggression occurred it was important to stop it right there, or the Japanese would bomb Pearl Harbor again.

As another example, if you look at the records of U.S. decisionmaking with regard to Vietnam in the Johnson Administration in 1964 and 1965, it's quite clear that he and everyone around him had in their minds lessons that came from the Korean conflict. One was the domestic danger of losing South Vietnam, the sense that if that happened it would be like the loss of China in the 1950s. But a second was simply the analogy. Here's a country divided between a Communist north and a non-Communist south. "Surely we can accomplish in Vietnam what we accomplished in Korea."

Most of the time the analogies are not quite that powerful. They are more ambiguous. But one of the things historians can do is to notice those and to think about the details of them and to raise questions about the extent to which the analogy really applies in the current situation.

In the Kennedy Administration, the historian, Arthur Schlesinger, Jr., as an assistant to Kennedy, was useful in many ways, and one was this. At the beginning of the 1960s it was accepted fact that you cannot have Communists in a coalition government – "they would take it over." Arthur pointed out that was not always true, you did not have to take it as a law of politics. It helped the Kennedy Administration think about coalition governments in places like Laos and elsewhere.

Now let's take a current example. I think the Pearl Harbor analogy [for September 11] is very strong for a variety of reasons. It is certainly not like the analogy of the 1930s or the analogy of Korea, but it is strong. It's worth publicly raising questions about the similarities; there are many points that are not similar. In some ways, you can substitute that analogy for what occurred in the 1940s. It was the fall of France in the spring of 1940 that really persuaded a lot of Americans to pay attention that the U.S. was inescapably tied up with Europe and could not avoid involvement in that conflict.

Lastly, historians can be sources of suggestions. In many cases, the suggestions need to be worked out by other, more systematic social scientists, but we can produce suggestions simply by being a kind of memory bank for government.

I'll give you an example. Take the new Office of Homeland Security. It has a very large charge and is just beginning to get organized. The concept is unclear. But there are some suggestions that come from our experience in WWII. I think there is a rough analogy between the mission this office is supposed to have and the mission given to the Office of War Mobilization in 1943. Obviously, the task is quite different but the conditions are similar.

In 1943, we were barely holding our own in Europe and the Pacific. The war production effort was a mess of tangled jurisdictions, people pursuing their own agendas. You had aircraft bodies in one place and engines in another

and they were not getting put together. You had agencies taking advantage of the war to pursue long-term agendas. One example was the Army. They had a huge project for drilling for oil in the Yukon. This was presumably an order to provide aviation gasoline for planes that would be based in Alaska, but it would be years before it would materialize. It turned out, upon examination, that in fact the gasoline that was needed could be supplied by one tanker run going up the west coast less than once a week. But there were many such examples.

Congress then, as now, was very concerned about it, was conducting investigations, was calling for the creation of a super-department to bring things together. Roosevelt was perfectly responsible for the chaos that existed. He saw that this idea of a super-department was not a workable solution, and what he did was create the Office of War Mobilization. He did it with an executive order significantly more sweeping than the one President Bush signed for the Office of Homeland Security. Roosevelt appointed former Senator, former Supreme Court Justice, James Byrnes to head it and said he was to prepare programs and policies to regulate the American production effort, including agriculture, bridges, and so on. The order said he was to issue directives to departments and agencies and report to him as to whether they had done it. Roosevelt accompanied this with a letter to Byrnes saying there is no appeal for your decisions. "You are for practical purposes, the assistant President."

Byrnes did his job with a very small staff; he never had more than 10 people. He had a very small office in the White House. What he did was to get together the heads of departments and agencies (he would not deal with number-two people, he would only deal with cabinet heads and agency heads). If the cabinet secretary couldn't be there, that department was not represented in the meeting. He asked them what they would agree was the order of priorities. Second, what department or agency was best equipped to take the lead on a particular task? Third, he asked, Are you prepared to accept that lead, my action in transferring the resources and people required across departmental lines, issuing a directive that requires that people in your department follow orders coming from other departments? Then he issued these directives to work with people in Congress, to use the Bureau of the Budget (the predecessor of the Office of Management and Budget) as a kind of staff arm to move people and resources.

It worked. A year later, the things Congress had been complaining about were not there. The war production effort was fairly well organized.

I think this is an interesting example, with many suggestions for Governor Ridge and his new office. That's the kind of thing we can do as historians – make suggestions that come from our store of knowledge of the past and make suggestions for people in other social sciences.

Let me close by repeating that in the past, bad times have been good for social science. On the whole, social science has been good for the country. We are in another such time – a period of crisis or emergency – where we will be called upon to contribute, and I hope we all pull our oars together. In most of these crises in the past, we worked together.

I think another experience of the past that all of us can use is the experience of the early nuclear era. The lesson is not to create a new federal research center, as the RAND Corporation was created then, but to have a way of making use of the existing centers across the country – academic centers, think tanks, etc. – to get them to work together, promoting real multidisciplinary, interdisciplinary work involving all of us.

Q & A

PARTICIPANT: [Off-mike]

MAY: That's a different set of questions. People have been calling attention to the experience of the British, the Russians, certainly Alexander the Great. I don't find those as interesting as this one. It seems to me this is a mood in Congress. There is a bill that rolling along in the Senate and in the House to create a super-department for homeland security. I think Roosevelt was dead right in thinking it was a bad idea then, and it's a bad idea now. This is an illustration of how it can work without that.

PARTICIPANT: [Off-mike]

MAY: Let me make two points. One is, if you're trying to address analogies that are in the heads of people from the U.S. policy world, they're going to be from American history. One of the big problems in the area in which we're now involved militarily is the education system. How might it be possible, for example, for outsiders to have some effect on the education system in Pakistan so that there might be some sort of restoration of a public education system sometime. That involves thinking about the cultural setting – why is it that public education has practically vanished there? What could reverse that course? How could it be done? How can we help to make that population less dangerous to us and to its neighbors? Those are very large questions that involve trying to understand cultures about which we know very little.

As it happened, oddly, one of the most particularistic pieces of historical research I ever did, which was about how did the U.S. come to be recognized as a great power at the end of the 19th century, was funded by the SSRC. On the other hand, a program of developing teaching cases on exactly this – the uses of history in public life that could be used in public policy schools – was funded by the NEH.

Social Science and Public Policy: William Julius Wilson, Harvard University

Felice Levine, Executive Officer, American Sociological Association: Thank you, Howard. I feel this is an easy introduction to make. I suspect that everyone in this room is an F.O.B. – friend of Bill's. I'm pleased to count myself among them.

William Julius Wilson really needs no introduction and if I thought that wasn't the case, we have a really fine biography in the materials that COSSA prepared on all of the speakers. He is the Lewis P. and Linda L. Geysler University Professor at Harvard University and a sociologist par excellence with a primary appointment at the Kennedy School. He received his Ph.D. in Sociology from Washington State University. The Sociology Department at Washington State University was one of the leaders in opening up access and opportunity for social scientists across fields, and continues to play an important role.

I could take up much of the hour and intrude on Bill's talk time by reciting his accomplishments, his publications, and his awards. I would be remiss if I didn't mention that he is a past president of the American Sociological Association and, as Howard said, a past president of COSSA and a very loyal and long-term advocate of COSSA. He is a MacArthur Fellow. He was elected to the National Academy of Sciences, the American Academy of Arts and Sciences, the National Academy of Education, the Institute of Medicine . . . I can go on and on. He is the only non-economist to have won the Seidman Award in Political Economy. In 1998, he won the National Medal of Science, which is among the highest scientific honors in the United States and rarely conferred on social scientists. There was a period there where Howard and I would constantly get phone calls about the next marvelous event that we could attend. It was really a privilege to see the important ways that Bill was honored and rewarded for very seminal contributions in sociology and the social sciences more generally, and for linking social science to very central issues of social policy.

One of the things we talked about is the compartmentalizing of the disciplines and even compartmentalizing of sectors of institutions and our need to think more broadly across education, the family, the criminal justice system, and the healthcare system.

At the 1998 dinner in honor of the National Medal of Science winners, Howard and I were privileged to sit with Bill's family. Bill talked about his daughters and got them to talk about their lives even though this was an event where we were honoring Bill. And then he was ushered away and we got the proud daughters and the proud sister talking about Bill and what he was like as a little boy. I think it's important for us to remember that, that we're all part of the broader social context in which we fit and that Bill's achievements, his academic achievements are part of a broader life and broader sense of significance that he's brought to that life.

As I said, today we are celebrating and discussing and analyzing and scrutinizing and anticipating the links between social science and social policy. That has really been at the core of Bill's career for 35 years. He's been a central figure in social science, advancing significant policy issues and building those bridges. His work on poverty, joblessness, race, and now welfare reform are testaments to the rising significance of social science to addressing the challenging issues we face. Bill is an educator in the broadest sense of that word and an advocate for the importance of the social sciences.

In that regard, perhaps we've had no greater friend in the social sciences, in times of opportunity and adversity. It's been a special opportunity that when we've needed to call upon Bill, he's there, as he is today.

William Julius Wilson, Harvard University: Felice, thank you very much. As a former President of the Consortium of Social Science Associations (COSSA), I am very pleased to be a featured speaker on the contributions of social and behavioral research to public policy and to help celebrate the Consortium's 20th anniversary. In my talk this afternoon, I want to address the important issue of expanding the domain of policy-relevant scholarship in the social sciences.

The 1996 Gulbenkian Commission Report on the Restructuring of the Social Sciences stated that the traditional boundaries in the social sciences have been weakened by pressures for change. These pressures include those associated with the rapid expansion of the university system that created increased specialization which in turn "encouraged reciprocal incursions by social scientists into neighboring disciplinary domains;" and those from feminist and other groups that have challenged the parochialism of the social sciences.

In the process there has been, according to the Report, a growing recognition "that the major issues facing a complex society cannot be solved by decomposing them into small parts that seem easy to manage analytically, but rather by attempting to treat these problems, human and nature, in their complexity and interrelations."

But, the report does not discuss another major pressure felt by the social sciences that I believe will ultimately have an impact as great or even greater than those pressures that have emerged during the past several decades. I refer to the impetus to address policy-relevant issues that are associated not only with the country's emotional and economic adjustments to the events of September 11, but, more fundamentally, that grow out of the struggles of nation-states to adapt to the impact of rapid technological and economic changes on individuals, families, communities, institutions, and the society at large.

Technological innovations are occurring exceedingly rapidly and the lagging societal adjustment to these changes in many areas of life have placed a strain on our basic institutions and challenged traditional practices in preparing individuals to fulfill adult roles and responsibilities. Take, for example, the impact of the decline of the mass production system. The skill requirements of this mode of production were reflected in the system of learning. Public schools in the United States were principally designed to provide low-income native and immigrant students the basic literacy and numeracy skills required for routine work in mass production factories, service industries, or farms. Today's close interaction between technology and international competition has eroded the basic institutions of the mass production system. In the last several decades almost all of the improvements in productivity have been associated with technology and human capital, thereby drastically reducing the importance of physical capital and natural resources.

Moreover, in the traditional mass production system only a few highly educated professional, technical, and managerial workers were needed because most of the work "was routine and could be performed by workers who needed only basic literacy and numeracy." Accordingly, workers in the United States with limited education were able to carry home wages that were comparatively high by international and historical standards. Not so today.

At the same time that changes in technology are producing new jobs, however, they are making many others obsolete. The workplace has been revolutionized by technological changes that range from robotics to information highways. A widening gap between the skilled and unskilled workers is developing because education and training are more important than ever. While educated workers are benefiting from the pace of technological change, lesser skilled workers face the growing threat of income stagnation and job displacement.

The impact of technological change has been enhanced by international competition. In order to adjust to

changing markets and technology, competitive systems are forced to become more flexible. Companies can compete more effectively in the international market either by improving productivity and quality or by reducing workers' income. The easier approach is this latter low-wage strategy, which the United States has tended to follow. Many new jobs have been created, but, except for the last half of the 1990s and the year 2000, incomes of lower paid workers have been in stagnation, despite incredible job growth.

These changes puzzle many policymakers, and as they have turned to economists for some of the answers, the limitations of relying solely on a paradigm embedded in a single discipline could not be more apparent. The traditional economic models failed to explain the strange recent phenomenon of a tight labor market and low inflation in the United States. In the latter half of the 1990s into 2000, the United States experienced one of the tightest job markets in memory, yet this low unemployment did not fuel inflation, and, especially prior to 1997, did not lead to significant increases in wages. What now seems clear to an increasing number of social scientists, including economists, is that a strictly economic explanation is no longer sufficient to explain the relationship between employment and inflation. Sociological and psychological explanations about workers' responses to the growing internationalization of economic activity, including the threat of job displacement, are now being integrated with the economic explanations. Allow me to briefly elaborate.

Between 1993 and 1997 the US economy added more than 14 million jobs. And in 1997 the unemployment rate declined to 4.3 percent, the lowest in thirty years. Yet, prices did not increase very much during this period, in part because wages, the main element of costs, did not increase much either. Despite high levels of employment and labor shortages in some areas, workers were surprisingly hesitant to demand higher wages. Few would have predicted that kind of behavior in such a favorable job market. As the economist Paul Krugman pointed out in November 1997, "apparently the recession and initial jobless recovery left a deep mark on the national psyche." He pointed out that workers' confidence had been shaken by downsizing and the specter – real or imagined – that many of their jobs could be done for a fraction of their salaries by workers in Third World countries. Indirect evidence of workers' anxiety could be seen in the rate of voluntary resignations. Usually, when unemployment drops, voluntary resignations increase because the favorable job market enables those who resign to find new jobs, presumably at higher pay. However, the "quit" rate actually declined in 1997, a period of low unemployment.

In a 1997 survey of a random sample of the American public, 68 percent of the respondents overall and 72 percent of the non-college graduates surveyed expressed concern about the exporting of jobs overseas by American companies. Reflecting on the situation in 1997, the economist Krugman argued that workers in the United States feel that they cannot rely on weak unions to bargain effectively for higher wages, and if they lose their jobs they feel compelled to take other employment soon on whatever terms they can get. "With such a nervous and timid workforce," states Krugman, "the economy can gallop along for a while without setting in motion a wage/price spiral. And so we are left with a paradox: we have more or less full employment only because individual workers do not feel secure in their jobs . . . The secret of our success is not productivity, but anxiety."

In retrospect, this argument may have been overstated, especially given the rapid increase in productivity growth in the late '90s and its dampening effect on inflation, but Krugman's argument does provide a clear example of how pressures to confront policy-relevant matters are forcing social scientists to address complex issues with explanations that integrate perspectives from different disciplines. This kind of pressure contributes to the erosion of rigid disciplinary boundaries.

But I believe that the pressure to confront policy-relevant issues will not only contribute to the integration of the social sciences, it will also increase policy-relevant research within the various disciplines. Moreover, I predict that the disciplines that most rapidly and widely respond to these pressures will attract the largest share of public and private foundation resources for research and institutional expansion. Nonetheless, there remains strong resistance to the practical application of social science research. And our vision of the domain of policy-relevant scholarship in the social sciences is limited and will have to be expanded, as I shall now endeavor to explain.

Those of us involved in policy-relevant research are fully aware of the intense pressure to address problems that concern the nation. Yet many social scientists argue that we ought to wait until a sufficient amount of good data are accumulated before we make any policy recommendations or enter the policy debate.

However, as Robert Lynd pointed out in his classic volume, *Knowledge for What*, published over six decades ago, if social scientists wait for more data before offering policy recommendations, or if they avoid issues of public controversy because of lack of data even though their theoretical ideas or hypotheses would elevate the level of the debate and broaden perspectives, decisions will be made and policies will be formulated anyway – without their input.

I fully agree and would like to take this opportunity to further challenge the assumption that the social sciences should not attempt to influence the national agenda until there are “sufficient” or “adequate” data by arguing for (1) a broader conception of the use and application of policy-relevant data, even preliminary data; and (2) an increase in the role of theoretical ideas, hypotheses, and concepts in national policy debates.

Let me begin with the first point – broadening the conception of the use of policy-relevant data. Just as one will rarely find in the social sciences a dataset that would unambiguously and incontrovertibly determine the validity of a major theory or the correctness of a major factual question, so too is it uncommon to produce a data set that would unambiguously and incontrovertibly resolve a public controversy. Although any social scientist would like to have the greatest confidence in his or her data, sometimes preliminary data can be used to reveal the narrowness of a public debate or to challenge the general consensus on an issue, and thereby demonstrate the need to take other factors into consideration.

For example, a few years ago the public policy debate in the U.S. over the causes of the breakdown of the poor black family narrowly focused on the adverse affects of welfare. In a paper first presented at a national welfare conference in Virginia in December 1984, Kathryn Neckerman and I argued for the need to consider the role of male joblessness in the growth of poor single-parent African American families. The aggregate census-type data we presented in support of our position clearly suggested, rather than firmly established, a positive relationship between male joblessness and solo-parent families.

Nonetheless, the paper drew a lot of attention. It not only altered the terms of the debate in academic circles and triggered a round of new research among poverty researchers, but it made policymakers on Capitol Hill more aware that the issues surrounding the rapid growth of single-parent poor black families were more complex than had been previously assumed. Today male joblessness is routinely identified as one of several important factors in the growth of solo-parent families and the discussion of contributing factors no longer narrowly focuses on the receipt of welfare.

Furthermore, any discussion on the need to expand the domain of policy relevant scholarship has to address the problem of what the Harvard sociologist Stanley Lieberman calls, the “formalistic fallacy,” the view that data for generating policy recommendations ought to be obtained from the use of certain formal procedures or techniques. Non-quantitative research – for example, ethnographic research – is therefore considered inappropriate for generating policy recommendations.

Although all scholarly work should be subjected to critical review, concern should focus on the logic of inquiry – the structure of explanation, the significance of concepts, and the nature of evidence – not on the procedures or techniques used. Let me briefly elaborate on this point as it relates to public policy-relevant research.

Quantitative social science established its hegemony in the 1970s. Ethnographic research in fields such as urban poverty, which had been revived in the 1960s, was basically dormant in the 1970s. In the 1980s, however, we were beginning to see a shift in focus away from quantitative versus qualitative research to an approach that emphasized integrating the two strategies in empirical studies that focused on problems such as urban poverty.

There are several intellectual and practical issues involved in the integration of quantitative and qualitative techniques. These issues relate to the important distinction between the context of discovery and the context of validation. Whereas the context of discovery is concerned with the way in which fruitful concepts, hypotheses, and theories are discovered, the context of validation is concerned with the evaluation of the products of science, and therefore with making the evaluative criteria as explicit as possible.

I emphasize this distinction because a number of people have maintained that the best way to integrate ethnographic and quantitative research is to use the former in the context of discovery and the latter in the context of

validation. In other words, it is argued that ethnography ought to be used to generate hypotheses that could then be tested with quantitative research.

More specifically, the major objection to using ethnographic research in the context of validation is the inherent difficulty in generating a sample representative of a larger population. However, there is another type of sampling crucial to theory testing that addresses the issue of whether the conditions specified by the *theoretical assumptions* that guide the research are represented. This is known as theoretical sampling, defined as selecting a number of natural cases that fit the conditions appropriate to the assumptions of the theory.

For example, in my book *The Truly Disadvantaged*, I outlined a theory of the social transformation of the inner city and a number of the key hypotheses incorporate the notion of “concentration effects” – the effects of living in highly concentrated poverty areas. One of these hypotheses states that individuals living in extreme poverty areas are much less likely to be tied into the job information network system than those living in marginal poverty areas.

I contend that this hypothesis could be tested by a participant observer who selects a neighborhood that represents an extreme poverty area and one that represents a marginal poverty area, and who observes patterns of work-related interactions in each neighborhood over an extended period of time. Some people may want to question the degree of rigor involved in testing such a hypothesis with participant observation techniques, but this approach is clearly consistent with the logic of validation.

Ideally, one would want to test this hypothesis with more quantitative sources of data that would include a large number of individuals from a variety of urban neighborhoods. But the ethnographic research, including leisurely conversations with people over extended periods of time, could uncover many subtle patterns of behavior and experiences that are difficult, if not impossible, to ascertain with the more conventional research techniques. To eschew such research in the policy domain on formalistic grounds is to limit the potential of the social sciences to influence or contribute to important policy discussions.

However, the contribution of the social sciences to the policy arena need not be based on empirical studies or research findings. As Carol Weiss of Harvard University has pointed out, the theories, ideas, and concepts of the social sciences “may also help to shape what it is that the public thinks about and what it is that governments do.” Weiss argues:

“Although good data are useful and build credibility, equally important is the [social science] perspective on entities, processes, and events. Participants in the policy process can profit from an understanding of the forces and currents that shape events, and from the structures of meaning that [social scientists] derive from their theories and research.”

An important function of social science is to use existing theories or theoretical frameworks to advance our understanding of social processes or structures. In other words, social scientists can provide “enlightenment.” “Sociological ideas, more than discrete pieces of data, have influenced the way that policy actors think about issues and the types of measures they have been willing to consider,” states Weiss. The social sciences “bring fresh perspectives into the policy arena, new understandings of cause and effect; they challenge assumptions that have been taken for granted and give credibility to options that were viewed as beyond the pale. They provide enlightenment.” Likewise, political scientist John W. Kingdon points out that although social scientists “can be very good at documenting the existence, frequency, incidence, and intensity of a condition,” they are also frequently “able to show policymakers that the world works in ways that might not have occurred to them,” and that social scientists’ knowledge of the way the world works enables them to make better cause and effect connections than others. For all these reasons, I believe that it would be shortsighted to discourage or overlook the use of theoretical insights from the social sciences to inform public policy debates.

In summary, I have tried to make the case for expanding the domain of policy relevant scholarship so that we can (1) be more flexible in the kinds of data that we use and the ways in which we use them, and (2) recognize the important role of social theories, concepts, and ideas in the formulation and discussion of public policy issues. I firmly believe that we will become more active and influential players in the social policy arena as a result. Thank you. I

have time for some questions.

Q & A

PARTICIPANT: [off mike]

WILSON: There is no question that the intrusion of policy preferences muddies the water, but we're human beings and it's unavoidable. In the long run, the arguments that survive are those that withstand critical scrutiny. Even though there is a debate among social scientists about vouchers that reflects ideological preferences, it's far more sophisticated than the debate going on in Congress about vouchers.

Regardless of the ideological positions taken, I would still want thoughtful arguments and commentary as part of the ongoing debate. Eventually, arguments will be sorted out. I maintain that the works that can withstand scrutiny are the ones that will have the lasting influence.

PARTICIPANT: I'm Louise Lamphere, I'm an anthropologist, and I was really pleased at your evocation of ethnography as an appropriate data gathering technique. I want to go back to the issue about the kinds of jobs that were created in the '90s and the whole issue of globalization and whether that puts fear into workers.

What kind of data is there about the kinds of jobs that have been created over the last 10 years? My sense is that a lot of jobs that have been created are service sector, low-paying, short-term jobs anyway, and that the phenomenon of outsourcing has created jobs outside of large companies, not just at the service level, but at the managerial and professional level.

The kinds of jobs people are thinking of are the ones which they hold that are not particularly being affected by globalization itself, but this process of creating jobs outside of large corporations where there are fewer benefits, and are more short-term. People's willingness to move or thinking about how stable their situation is impacted by changes in the internal job market on its own.

WILSON: I pointed out that global competition forces companies to develop strategies so that they can become more effective. The low-wage strategy has been heavily used in the U.S. I would maintain that outsourcing is part of that strategy, it's related.

The creation of these low-paying, short-term jobs is one of the reasons we have this growing gap between the haves and have-nots in the economy. What's interesting is when you contrast the period immediately following WWII to about 1970, and the period from the early 1970s to about 1997, you see a significant difference in the kinds of jobs that were created. The jobs created in the earlier period were protected by strong unions, which meant that workers not only received higher wages, but health benefits and so on. With the decline of the unions, the basic support for workers was eroded.

People concerned about the plight of low-wage workers argue that we need to strengthen our equalizing institutions to protect workers, including strengthening unions and providing better education and health care. The low-wage jobs of today do not have the basic supports or fringe benefits that they had in previous years.

LAMPHERE: I didn't mean to imply this was just private industry. I think government is very much involved in this. There is a lot of this happening in government agencies that are beginning to outsource their janitorial services.

WILSON: You see how effective it is in the private world.

LAMPHERE: I totally agree with you.

PARTICIPANT: We've often discussed how difficult it is in forming public policy debates to communicate

something about large quantitative data arguments to public policymakers. As I'm sitting here listening for your call to think about more nuanced descriptive ethnography work, how do you think we could prevent that work from becoming just a competition of vivid examples? How could you envision that work actually influencing the public policy debate and what would you tell Howard to do as he runs Congressional briefings?

WILSON: I would hope that ethnographers who get involved in the public policy debate would not just provide examples from their field notes, but would be able to reveal important relationships that might not occur to us.

For example, I'm now involved in a major study of the effects of welfare reform on families and children in three cities, Boston, Chicago, and San Antonio. This study includes a longitudinal survey of 2,500 families. But it also includes an ethnographic study of 270 families in these three cities over time.

Our ethnographer uncovered some relationships that we did not detect in the survey. They found that there is a relationship between mother absenteeism and tardiness on the job, on one hand, and childhood illnesses. It seems that these kids in these poor neighborhoods are sick all the time. They have low-grade fevers and so on, mothers are constantly trying to deal with their children's illnesses. When you have plumbing that's stopped up and not repaired and flies and maggots that breed all kinds of diseases, the conditions can be very brutal. Some of our ethnographers were getting sick in these environments. When they talked to employers, the employers assumed these people weren't coming to work because of a lack of work ethic.

I think the ethnographic data showing a relationship between mother work performance and childhood illnesses is a very important public policy finding. I would hope therefore that as ethnographers get more involved in the policy debate, they will have concrete relationships to reveal, instead of simply providing examples for relationships that are already apparent.

PARTICIPANT: You've suggested that we inform and expand the policy debate by presenting theories and concepts that maybe policymakers are not considering. What concepts and theories would you use now to expand the debate and the policies regarding understanding and preventing terrorism in the U.S.?

WILSON: I'm not going to go that way because my theories don't really address that issue. However, there is going to be a lot of pressure on social scientists to help explain the public's reaction in a time of crisis. I hope that they will be prepared with the kinds of theories and data that provide compelling answers and policy recommendations.

PARTICIPANT: I'd like add to Bill's statement on the work in the welfare reform project. That underlies the theme this morning and the basic point is the need for interdisciplinary rigorous discovery and for moving beyond the labels of one tradition or one approach being more scientific than another.

One of the things that I found most fascinating about some of the reports from this project is the integration of the ethnographer working on their work concurrent with analyses from GIS data. It's not just a health issue, because the proximity (or lack thereof) of throughways to health services revealed through GIS mapping showed these mothers had to do often heroic things that resulted in not being able to appear on the job.

Now, each of these methods, in and of itself, would not have added that insight, but, as we were discussing in the sessions this morning, the combined mapping of our conceptual and empirical frameworks is providing some very fascinating science and very relevant policy implications.

PARTICIPANT: You raise the issue of expectations, and part of that is that the public policy arena expects social science findings to fully inform the policy debate. What you're suggesting is that it's not so much the findings as much as it is the social scientists who will bring their individual and collective perspectives to sensitize the debate to bring new perspectives. You've done a lot of that and I wonder if you could reflect on the degree to which your own criticality becomes important, in terms of the relationship to the policymakers and their ability to trust those insights rather than dismiss them because they're not backed up by definitive research findings.

WILSON: I had a conversation with President Clinton following his economic summit conference in which I was one

of the few non-economists invited to that conference. One of the things that the President said to me is that I see your work cited all over the place. He was impressed with the frequency with which the work was cited across disciplines. That provided credibility to him because it suggested that my ideas were compelling. He thought that was important. Thank you.

The Future of Behavioral and Social Sciences

Norman Bradburn

SILVER: We have three very distinguished presenters who are going to look at the future of the behavioral and social sciences. Norman Bradburn is the Assistant Director for the Social, Behavioral, and Economic Sciences at the National Science Foundation (NSF). Last night at the Board of Directors meeting, we heard the good news that Norman has decided to stay at NSF for another two years. We are delighted.

Norman Bradburn, National Science Foundation: I think I'm about to do a foolish thing, that is, to talk about the future: I'm almost certainly going to be wrong. But I will give you my personal view. I should say that I do not speak for the National Science Foundation.

I thought I'd look at what some of the change drivers are that are moving the behavioral and social sciences in different directions. I'll let you draw your own conclusions about where it will end up. I'll organize this around people, ideas, and tools. I'll start off with tools.

Some of the new tools that are being used in the behavioral and social sciences are going to make the biggest difference. Some have been around and some are pretty new, but all are having a big effect.

The first set of tools making the biggest difference in psychology are the non-invasive methods for studying the brain, ways of being able to see what's going on in the brain as people perform cognitive tasks or have emotional responses. It's generating a lot of excitement in psychology.

The second big area is what I call computer control of experimentation. There are many different types. In cognitive experiments, computer programs control the precise measurement of reaction time and the display of complex stimuli, and modify the stimuli according to the response. You can test things that have been hypothesized, but not studied, for 100 years. Complex computer labs have made possible experimental economics, and the study of interactions that are computer mediated.

Another thing that is being made possible is the Web collaboratories, the sharing of experimental protocols where people can run experiments in different universities and labs. They use the same research protocols, and the data all go into a central data file so you can get larger sample sizes and more heterogeneous subjects.

A new tool that is just coming in, and which I think is not yet totally feasible, is wireless computers, which make it possible to take some of these experiments out of the lab and into the field. You can study experimental games and perform cognitive experiments cross-culturally or with subjects that are not lab-bound the way that they've been traditionally.

Finally, in the survey world, the Web allows for graphic and multimedia presentation of information and ways of improving measurement. There are a lot of problems with Web-based surveys now, but eventually they will become truly revolutionary.

A third tool that is not so new is data libraries – data archives such as ICPSR [Interuniversity Consortium of Political and Social Research], but also libraries of images and pictures. NSF has funded a library for fMRI images, and also for sounds and languages that are endangered, and for archives of large free text. We need to develop standards of documentation so that large datasets and other kinds of data can be shared.

There are also new ways of putting together geographic information made possible by geographic information systems (GIS). This is leading to increased use of spatial data and a place-based social science. GIS has already been used in studies of land use, and we're able to do a whole lot of studies that we couldn't do before. There are new analytic methods coming from biology and physical sciences, such as DNA analysis and new types of material analysis, chronochemistry, ways of studying the evolution of humans and the diffusion of peoples around the world, that are made possible by these kinds of imported methods from biology and chemistry and physics.

Finally, we are on the cusp of new types of sensors. There's a great deal going on in the natural sciences, particularly in biology, to develop sensors of various kinds, to implant sensors in the skin as well as for a variety of security applications. There is going to be a whole set of new types of sensors, in addition to the traditional ones. I think these raise many practical and ethical questions about how observations are done, many new types of observations will be becoming possible.

Statistical tools are another type. Two that I think will have impact on social science development are data mining, that is, finding patterns in very large data sets where you don't really know what you're looking for at first, and hierarchical analysis, which is extremely important for doing comparative research where you want to compare units that are at different levels of aggregation, like countries and people within countries, or school systems and classrooms. They will lead to new types of research design and better analysis of these designs.

Another technique which is becoming more widely used, particularly in political science, is what political scientists call "agent-based modeling," that is, stochastic modeling of interactions made possible by computers which allows for the simulation of many complex interactions. These are some of the tools that I see making changes in the social sciences of the future.

In the realm of ideas, there are probably no truly new ideas, but different ones come to the fore, as well as different ways of thinking about them. I think that rational models are emerging as the central dominant paradigm in the social sciences, as you may have read in the *Chronicle Of Higher Education* a month or so ago, which reported on reactions to that. I think they certainly have been strong for a long, long time in economics. They're becoming extraordinarily strong in political science and are emerging in sociology. I think that these are starting points against which variations or violations of rational behavior can become the grist of research or argument. They are a paradigm for working out the base case against which other models will be tested, and this testing will move the fields in important ways.

Structurally, I think there is renewed interest in network analysis. Epidemiology in particular has shown renewed interest to networks. This is also an area where one needs new statistical tools and new mathematical tools for representing networks. Perhaps we need to look at engineering. My reading of the history of earlier efforts to advance networks came to very little because graph theory is not very amenable to solving extremely complex networks of the sort the social scientists need. It's not an area I know a great deal about, so I'm less confident about that.

As I mentioned, hierarchical analysis is important. The study of structures has always been a popular subject of research. I think there's new interest, particularly in comparative research, because there is better data available on a comparative basis and they are more sophisticated.

What are the up-and-coming fields? I think fields will become more multidisciplinary. Human and natural coupled systems are a growing area of research. The whole environmental movement has greatly emphasized that. I think it's an area in which the social sciences are getting extremely involved and where there is good interaction.

Another field that I think is growing is communication and language at the individual level, where work in the pragmatics of language is a central issue. The use and development of techniques for measuring meaning is a central problem for linguistics. Linguistics has generally been a target when university administrators want to get rid of a field. It's survived that bad reputation and has become an exciting field.

At the societal level, we need more attention to the study of symbolic systems, particularly those like religion,

which are fundamental to society. The neglect of the study of religion means that there is no real body of knowledge to fall back on in our present circumstances where religion has become a major factor in world affairs.

All of these drivers mean that there has got to be better training in math, statistics, and computers. We need to know how to work better with engineers, mathematicians, and computer scientists. That's not a one-way street. They have to learn to work with us as well.

The new tools are too sophisticated and change too rapidly to expect one person to know it all. That means teaming up with those who understand the tools. All of these things will change the organization of research, and we'll move away from the single investigator with his/her graduate students working in their lab towards multidisciplinary centers or institutes. It's also a more capital intensive research mode, so we have to think about funding agencies. It's a different type of funding than we've been used to in the past.

Finally, all these tools are interrelated and feed back on one another. We're in for very exciting times, but we'll talk about that 20 years from now.

Barbara Torrey

SILVER: Our next speaker is Barbara Torrey from the National Research Council. She's the Executive Director of the Division of Behavioral and Social Sciences and Education at the National Research Council. She was also at one time President of the Population Reference Bureau.

Barbara Torrey, National Research Council: What I'd first like to say is happy birthday to COSSA, and that the COSSA newsletter is the best newsletter in town.

The future is a risky subject to discuss. But I said I would talk about the future of behavioral and social science partly because I was trained as an economist. We are so often wrong that it doesn't bother us anymore. We are wrong on a quarterly basis. It does sort of tend to embolden us as we think about the future.

I do agree with Ernest May's discussion at lunch that hard times are very good for social scientists. I think it's going to be true over the next 10 years for us. In fact, during WWII, the government came to Ruth Benedict for help in better understanding the Japanese. She wrote *The Chrysanthemum and the Sword* in response. It gave people a sense of who the Japanese were. I think we have that chance again but this time it will be about Muslim cultures. I spend a lot of time with physicists and biologists at the National Academy. Since 9-11, we've been in these meetings, asking, "What can we do? What can science do?"

Much of the discussion is about bugs and guns. The physicists say we can make better weapons. The biologists say we'll figure out how to deal with bioterrorism. Then they look at me and say, the behavioral and social sciences have to figure out what the root causes are. It feels like an accusation that we haven't done it yet. What's important is that we don't let other people define our questions for us, but that we define them for ourselves.

Let me describe three areas that I think will be in increasing demand in the next 10 years.

One is going to be governance issues. Last year, George Bush said he wasn't interested in nation building, but he's in it now. Unfortunately, we did not continue the research we were doing at the end of WWII. We're going to have to do a lot more of it. We keep talking about promoting democracy around the world. There are many different forms of democracy and we need to be smarter about what form fits what society. We have to have a much better map of where we are and what is possible with better governance.

Another area that's very important is behavioral economics. I was always frustrated in studying economics because my R-squares were so low. None of the professors minded because I had highly significant variables, but they were explaining so little. My feeling is that behavioral economics is really filling-in what we were all missing 40 years ago in graduate school. It's going to be very important to us, not only in terms of understanding our own economic behavior, but also in understanding what part of our economic behavior is universal – i.e., can be translated from the

Bronx to Borneo – and what part is unique.

The third and last area that I think is going to become much more important in the future is social network analyses. We've been in this area a long time, but we've only recently started to make a fair amount of progress. Roy Radner, an economist at Bell Labs, was asked 40 years ago, "Can you tell what people's personal familial networks are so that Bell Telephone can figure out a pricing policy for them?" He started doing this with a number of other economists and they couldn't get very far. They quickly ran out of computer memory, but that's because computers were very small. They didn't have a way of analyzing it and they finally just threw up their hands.

We finally have ways of beginning to look at these network issues. The work is very interesting and crosses a large number of disciplines:

- Tom Schelling, who is the first and last word on a whole series of ideas, was looking at neighborhood networks 30-40 years ago and looking at tipping points within neighborhoods.
- Population demographers, looking at family planning programs overseas, have been wondering how illiterate women learn about family planning outside of their villages, and how that information spreads through what kinds of networks.
- Barry Wellman, at the University of Toronto, has been looking at how computer and social networks interact with each other and mutually change.
- Historians of science are looking at innovation networks and how a new idea moves into industrial application.
- Organizational researchers are looking at what kinds of networks are most productive. You have some very interesting new questions in terms of organizational network productivity.
- Laura Carstensen, a social psychologist, is looking at different networks of the young versus the old and finding a consistent difference across social classes and across ethnic groups in these different networks. Her research shows how one kind of network is much more functional than another.
- We have two of our best criminologists here (Al Blumstein and Sally Hillsman). They look at illegal networks. That is where a number of the questions regarding terrorism are likely to focus.

I think all of this work that I've described is descriptive at this stage. We didn't have a lot of theory or infrastructure. But I recently saw something very interesting from Sandia Labs and Indiana University. It was mapping research literatures in 3-D. You could see what was connected and what was not connected, and then you could see how it was changing over time, how things were aggregating, falling apart, and isolating themselves. It was purely descriptive rather than analytical, but you could see the potential of all the kinds of thing we might be able to do using this technology. But it reminded me very much of an fMRI and the role that the fMRI has played in psychology and behavioral science. It's an fSNI, an fSocial Network Imaging; I hope we can make good use of this.

I've recently seen two articles about how ecologists have come to social scientists because of our ability to look at networks and our analytical techniques. We have a comparative advantage in network analysis. This advantage is likely to be the beginning of long, fruitful collaborations across a number of other scientific disciplines.

I have spent so much time with physicists and biologists that I have become very envious. I spend most of my working days envious of others. The physicists had a wonderful time in the first half of the 20th Century. The biologists have thrived in the second half of the 20th century. Now it's our turn; we're going to thrive in the 21st century. We'll start with your reception, Howard. Thank you.

David Featherman

SILVER: As you know, Rita Colwell [Director of the National Science Foundation] agrees with you. Our final speaker is David Featherman, who is the Institute Director and Senior Research Scientist at the Institute for Social Research at the University of Michigan. In an earlier life he was president of the Social Science Research Council.

David Featherman, Institute for Social Research, University of Michigan: Thank you, Howard, and it's a pleasure to help celebrate this special anniversary of COSSA. In my earlier life as President of the Social Science Research Council, Howard was a real joy to work with and for those of us in the hinterlands of New York City, COSSA has been a very important partner and ally of behalf of social and behavioral science.

What to say at this point of a very stimulating day? To be the last person following two distinguished colleagues, there is nothing more reassuring than gazing into a hazy crystal ball of the future and seeing some of the same things that these distinguished others see.

I want to share some remarks from the point of view of the role that Norman Bradburn had when he was Provost at Chicago or the role that I now have as Director of ISR at the University of Michigan. My remarks are less about the future than about conditions in the present that augur for the future. I want to mention three and end with a question. Some of this is going to seem fairly straightforward. But I want to tease some nuanced implications.

The first observation: The data of behavioral and social science continue to expand in their range and complexity. More than ever before, we are less defined as a field by the kinds of data we use and the kinds of analysis we apply to them.

Today, as a function of greater tendency to broaden the range of intellectual questions and, more importantly, of the partnerships across the sciences that we work heavily to develop, the range of data that we work with is much broader, and more so in a digital information age.

I could go on with many examples and could be repeating what has already been given. To save time, I'll collapse this and repeat this first point. It has an implication later on that isn't necessarily entirely positive in its consequence when broadly seen from the point of view of academic social science.

What social and behavioral science is today and in the future is hardly definable by the nature of the data we use. In the digital information age, the future shape of behavioral and social science is more likely to be limited only by our creative imaginations and ability to work collaboratively across disciplinary and epistemological communities. Our future is not limited by the kind or volume of data, in my point of view.

The second point is that our expanding data are more complex, voluminous, and integratable. But who will guide the manageability and usability of this complexity? Even in the more longstanding domains and databases like surveys and censuses, the sheer complexity and volume of our data are increasing astonishingly, and these databases can be aggregated at different temporal and spatial scales. And then there is the opportunity to use the new kinds of data – brain images, digital images of street blocks over a period of weeks, or streaming audio of streetside conversations. Our data archives are seeking more effective ways of cross-referencing these new holdings and their conventional ones, as digital libraries and archives. But despite these heroic efforts, these archives are not likely to be able to keep up with the volume and complexity of new data that will be stored decentrally in servers worldwide.

Massive, complex databases are very common across the sciences, from astronomy to genomics to the social sciences, and that may be fortunate for us in the behavioral and social sciences. Since massive databases and the need to develop tools for data retrieval and data mining are a shared challenge, NSF and other federal funders may want to focus special attention to them as such. We should be the beneficiaries in the future, if that happens.

Barbara mentioned computing a while ago, and I want to say a little bit about that. I have a particular take on supercomputers which I'll share with you in a moment. [The University of] Michigan is a partner with the San Diego supercomputing center. One of the highlights of that application is that it was going to specialize in meeting the needs

of social science.

It hasn't worked very well. One of the challenges is our unique computing environment. The power of desktop computing increases apace and is effective for most social science research. But for research taking advantage of these massive integrated databases – and datasets may have petabyte capacity in the future – the coming challenges are more problematic.

For example, new statistical methods – i.e., information theory, signal processing, applied probability -will be crucial to the “engineering” problem of efficient compression, data storage, and retrieval of this kind of data mass, and new data mining algorithms will be needed together with appropriate software using parallel processing on supercomputers.

However, most of the supercomputer applications in engineering assume that data reside for brief periods during which massive cycles come to play on that database. In our situation, a petabyte of data may be held in storage for a long period of time during which large numbers of users simultaneously select portions of the database for analysis. This makes us I/O intensive, requiring sophisticated and fast data mining algorithms and data visualization tools, as well as lots of computing cycles.

We in the behavioral and social sciences need more effective partnerships with hardware and software engineers, and they have to understand that our parallel processing and supercomputing needs are different from those of engineers and physicists. We need our own supercomputing center.

My third point is that our infrastructure requirements are expensive and tend to be overlooked as part of our research portfolio. I'm going to echo some points that Barbara Torrey made in *Science* magazine some time ago. On the whole, our community of science has been less attentive and outspoken on behalf of our infrastructure than have our colleagues in the natural and physical sciences.

That may be because we require fewer bricks and mortar elements, fewer fleets of ships. That we require and have accumulated an infrastructure for social and behavioral science and that it is and must become an essential part of our portfolio of supported scientific enterprise is a realization not to be taken for granted.

Our research portfolio now contains many large and continuing cross-sectional and panel studies. Arguably, these and other core databases of the social and behavioral sciences are part of our infrastructure as much as the data collected by various agencies in the federal statistical system.

The large data archives like the one at Michigan and its counterparts internationally are also part of the infrastructure. But we have no organized, community-wide process, no single or collaborative oversight organization, be it the SSRC [Social Science Research Council] or CBASSE [Commission on Behavioral and Social Sciences and Education] or federal collaboration between the NSF and major other federal funders like NIH [National Institutes of Health], NIA [National Institute on Aging], and NICHD [National Institute of Child Health and Human Development] to oversee and to fund this infrastructure.

Specifically, we have no consortium that systematically evaluates these elements of infrastructure and takes responsibility for seeing to their appropriate funding. And, we don't want to go back to a point of view – held in some federal funding quarters just a few years ago – that places this core infrastructure into a zero-sum competition with new, investigator-initiated science.

So if we add all these things up: the sum of the accumulating infrastructure of databases and data related facilities; the real cost of high quality surveys, including some of the new technologies for data collection; add to that the growing complexity and the kind of data we continue to work with – these all add up to a very large financial outlay. It's simply no longer possible to do what we do on the cheap unless one is willing to ignore the costs (and responsibility to progress in our science) of maintaining the existing scale of infrastructure, even as we address the costs of smaller-scale, highly innovative projects.

The last point is really the flip side of the first point about the growing and complex collaboration that we have with kindred sciences. The question is, Will our theoretical consensus require a rethinking of the human sciences?

Norman suggested that we will have a new nomenclature for scientific enterprise in the future. I agree. Under what rubrics will we store our science? In the year 2020, when the National Research Council conducts its evaluation of graduate degree programs in the social and behavioral sciences, among other areas, how will we be organized? I think our nomenclature will be quite different. Bill Wilson referred to the Gulbenkian report. That was subsequently published as an edited work by Immanuel Wallerstein under the title, *Open the Social Sciences*. The book suggested that behavioral and social science might benefit from rethinking some of its past epistemological assumptions, including linear cause and effect and systems seeking equilibrium.

It suggested a more vigorous conversation with the natural sciences over issues of complexity and adaptation. We see that going on across many campuses that have complex systems programs in alliance with physics, engineering, and the social sciences. It called for new mathematical as well as statistical tools for representing time-place relationships, non-linear behavioral processes, and changing social systems far from equilibrium points. This new approach in one sense threatens traditional boundaries of the social sciences and some of our tacit epistemological assumptions about social systems and how they should be measured and modeled.

By contrast, the recently published second edition of Denzin and Lincoln's *Handbook of Qualitative Research* describes the rich and extensive expansion of the post-positivism domain of qualitative behavioral and social science research. That too exists in the academy.

The epistemological contrasts between these extremes on the continuum of behavioral and social science is part of our collective research portfolio in academe. It's a problem for the future in some respects. The intellectual communities working beyond post-positivism are frequently associated with feminist scholarship which, in many ways, represents an extraordinarily rich and challenging portion of our portfolio on both sides of the qualitative-quantitative divide. The historical turn in the human sciences during the past two decades and associated reaffirmation of the importance of cultural, racial, and gender context owes a good deal to feminist scholars and scholarship.

Speaking as someone in academic social science administration, who has to negotiate this great complexity and epistemological breadth on a campus with a rich array of behavioral and social science research, I'm often uncertain whether we can retain a coherence under a single rubric: social and behavioral science. Will we be required, in order to make progress, to further segment our enterprise into more coherent but less broadly encompassing epistemological communities? Against that possibility, and for now, I am always reminded of the intellectual challenge and scholarly importance of seeing our research portfolio now and in the future, what we aspire to become, in its full breadth.

In doing so, I think we have to contend with some of the challenging assessments that are currently being written about some of our core disciplines. I just spent last week reading and writing a review of Stephen Cole's book, *What's Wrong with Sociology?* It is a stunning critique of the potential disintegration of a discipline. That's an extraordinary problem in contemporary social science insofar as it is true for sociology, if not also for a few other of our core disciplines. It's a challenge about how we organize this creative energy that we've been talking about in the behavioral and social sciences.

Thank you.

Q & A

SILVER: I think we have a little bit of time to challenge or ask questions. I'm glad to hear that people have faith that we'll be around in 20 years.

PARTICIPANT: This isn't meant as an unfriendly question, but I noted that the speakers did exactly what I would have done if I had been asked to address this and talked about the internal evolution of fields from the inside out,

with less attention on something that I'd like to ask you to address. What do you think is going to be expected and demanded of the fields from the external environment, and are we up to that?

BRADBURN: Herb Simon did a paper on the kinds of problems social sciences can solve. Usually in crisis situations, people want an answer to a question that is not really the kind that the social sciences could ever answer, and certainly not now. I think there will be pressures in the near-term to contribute whatever we can to understanding the terrorist situation and the broader situation of what is going on in the world with which we've lost touch.

A big question will be how we can maintain a free society when we have security concerns, how do you strike that balance. I think there are many other issues, including ones I hear all the time: What can social sciences contribute to improving the educational system? How do we learn? What can contribute to our understanding of how we learn and how schools ought to be organized? – that whole bundle of issues at all different levels. Those are political issues, organizational issues; everything the behavioral and social sciences do is in one way or another tied to what people worry about.

That's a big demand that is coming. The problem we all have is finding the way in which to define the problems in ways that we can solve them or contribute something to them. I think environmental problems are taking a back seat these days, but before 9-11 they were high on the agenda. I think the issue of energy consumption, which is a repeat of what was going on in the 70s, will need a little bit of reviving.

You see it a little bit now in the question of why we are so dependent on the Saudis. It's an isolationist thing – "These people are giving us all this trouble; why can't we do without them?" What do you have to do to be able to do without them? That's not the right way to frame the question.

FEATHERMAN: I think it's important that we be asked to try to help solve problems. If we were ignored, we would hate it. Looking at the history of how we came to be as social sciences, we were asked to help solve problems with social reform at the turn of the 20th century and with social policy questions around the Great Depression and WWII. We were really always at the mercy of being asked to help and it has been a driving force for us. The challenge is not to be a problem solver; rather, we should be problem finders, making sure that the solution being sought is for the problem that needs solving. What is the set of interrelated issues? That gets to the comments that Bill Wilson was making.

In many respects, we are seeing a better balance these days between internal intellectual forces of evolution (of what social science is and does) and outside ones, like being helpful in policy circles.

The danger is that we withdraw inside the academy and don't get into the fray – then we lose the opportunity to be problem finders and join forces with others in problem solving.

TORREY: I'll just add that we are most interdisciplinary when we are working on problems that are not defined by a discipline. My sense is that we always have to have our own intellectual agenda. When a problem arises, we may be able to address that problem, but we have our own intellectual questions that can address the problem somebody is specifying for us.

PARTICIPANT: It has been fascinating to hear where you all think the social sciences are going. I've been trying to listen in terms of where we ought to be going. We heard a lot throughout the day about translating the knowledge we already have to address the issues of social science, infusing the behavioral and social science in technology and multidisciplinary research, deploying new technology, developing infrastructure, and even embracing the humanities.

But no one has mentioned data. I may be old fashioned, but I think the importance of where we should be going is to collect data on the lifestyles of humans, organizations, and institutions in society as comprehensively as ever has been done before. Is there any comment about that?

FEATHERMAN: I actually thought that was implied in my first point. We are intensively in the business of collecting all kinds of data, expanding the range and volume of our "core" data. We're intensively pushing our

imaginations on how to measure things that are inherently immeasurable in some final sense. That's what we do best and I see us doing that extensively. From there I addressed some other issues. I'm sorry if I didn't say it plainly enough. I think we're doing that in collaboration with many different disciplines inside and outside behavioral and social sciences.

BRADBURN: I would agree except that I would be happier if some of the data selection programs were more focused on or came from better theoretical frameworks. What amazes me is how often we get into a problem and start working on it and then look and say: "where's the data?" and it turns out it isn't the right data.

That may be an inherent problem because our ideas are always changing about what the right thing would be, but sometimes it seems like the most obvious things aren't there at first; after the fact, they become obvious. I was pushing a little bit on observational data because, the way things are going, our ability to collect data from people and have direct access to them is going to be very circumscribed. That's one of the reasons I think observational data is important, but it may be that natural observations may become a bigger part of the database.

PARTICIPANT: I was struck by Barbara Torrey's first question that she was asked about what we know about terrorists. I have a suggestion of a book that my interest many people here. It's called *The True Believer*, by Eric Hoffer, from 1951.

BRADBURN: It's a book I have read and grew up on. It was an extremely influential book when I was in graduate school among social psychologists who at the time were studying phenomena of this sort.

There is fashion in people's interests in particular kinds of problems. Many of the kinds of problems that are now being talked about were studied intensively in WWII and right after. It's a marvelous book. I think it is out of print, but probably in the light of recent events, it will come back into print. It's qualitative research at its best.

PARTICIPANT: Everybody today has talked about the importance of pursuing interdisciplinary research in the social sciences, but so many of our institutional structures, from academic departments to NSF funding systems to other funding systems, are stuck in the disciplinary mode. What suggestions do you have for providing guidance, what incentives would soften the rigidity of those boundaries and stimulate more interdisciplinary activity?

BRADBURN: I am a strong believer in multidisciplinary research. However, I am also a strong believer in training people. This is a very difficult organizational problem for universities. I think people who are very well grounded in their disciplines do the best multidisciplinary work.

I came out of a multidisciplinary program, but we had to learn each discipline deeply. It's even better if you know several disciplines, but I think that the kind of programs I have been involved with since were totally multidisciplinary. The students who came out these programs missed some theoretical structure to build on.

My solution would be for the central organization of universities to have departments doing the basic training and separate institutes, that are multidisciplinary, doing the research. I've seen this work in physical and biological sciences.

FEATHERMAN: I tend to agree with Norm. I think living in the kind of Michigan environment that I do and with ISR being prominent in the chemistry of intellectual life and behavioral and social sciences at Michigan, there's nothing as exciting for me as having colleagues who go back to teach economics or political science who say they teach their courses differently than they did when they started because of having participated in these long-term, interdisciplinary projects at ISR.

In our laboratory space you have young economists doing laboratory experiments who are talking with the psychology graduate students. An enormous cross-fertilization takes place in those contexts that finds its way back into the classroom and gradually transforms future generations. That's one way that it happens.

BRADBURN: One way to conceptualize that is that it transforms disciplines over the long-term, but it doesn't destroy them.

TORREY: I agree with you on many points. I spent my entire career on interdisciplinary work, but there are some real downsides, and it's very hard to figure out the structure to solve them. It comes partly from having done a departmental review with the University of Michigan in the Department of Anthropology last year. They all said, "All of our folks are involved in these institutes all over campus, and their time is incredibly split." This is a real problem for junior faculty, because if they're spending their time in the Center for Middle Eastern Studies or the Center for European Studies or Women's Studies or whatever, they're not in the department, or if they are in the department, they're doing two things at once and it's really tough on them to teach and to publish. The more we proliferate these centers, the harder it is on the people who work in them and for the departments and centers to function, because we're asking people to do too much. I think we still haven't come to a set of structures that work very well within the university structure for fostering interdisciplinary work and keep people trained in departments.